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An Examination of the Cost of Capital Implications of FIN 46

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ABSTRACT: This study examines whether the adoption in 2003 of FASB Interpretation No. 46/R (FIN 46), *Consolidation of Variable Interest Entities—An Interpretation of ARB No. 51,* changed the cost of capital for affected firms. Using comparative analysis on a broad sample of 11,719 firm-quarter observations for 1,389 firms during the period 1998 through 2005, we find evidence that FIN 46 significantly increased the cost of equity capital for firms with affected variable interest entities (VIEs), an increase of approximately 50 basis points relative to firms reporting no material effect from the standard. Further, firms consolidating these formerly off-balance sheet structures experienced the largest increase. Taken together, these results suggest that FIN 46 reduced the opportunity for firms to use off-balance sheet structures to artificially reduce their cost of capital, a matter of regulatory concern.

Keywords: *cost of capital; off-balance sheet; special purpose vehicle; variable interest entity; FIN 46.*

Data Availability: *All data are available from public sources.*

I. INTRODUCTION

In response to Enron and similar accounting scandals, the FASB issued in January 2003, and revised in December 2003, Interpretation 46, *Consolidation of Variable Interest Entities—An Interpretation of ARB No. 51*, FIN 46 and FIN 46R, respectively (hereafter referred to

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collectively as FIN 46). Effectively, FIN 46 changed reporting for many off-balance sheet activities, termed variable interest entities (VIEs) under the new standard.[1] Although it increased transparency, FIN 46 was potentially costly to firms, conveying new risks to market participants that could affect the cost of capital. The prior literature regarding the economic impact of this type of mandatory change is mixed (e.g., Khurana 1991; Chen et al. 2010). In recent research, FIN 46 is shown to have real effects on investment activity and product-market competition (e.g., Bens and Monahan 2008) that are theoretically linked to firms' cost of capital. The purpose of this paper is to directly examine the FIN 46 cost of capital impact given the current regulatory and academic debate.

Examining disclosures in annual filings, we identify a sample of 244 firms with off-balance sheet structures impacted by FIN 46 and a control sample of 1,145 firms disclosing no material effect from the standard. Examining 11,719 firm-quarter observations over the period 1998 to 2005, we provide evidence that FIN 46 significantly increased the cost of equity capital for firms with affected VIEs, producing an average increase of approximately 50 basis points relative to the control sample. Further, firms that consolidated these formerly off-balance sheet structures experienced the largest increase following adoption.

Firms have long used off-balance sheet entities to obtain financing and avoid reporting debt on their balance sheets.[2,3] Advocates argue that these structures benefit investors by allowing management to access additional sources of financing, reduce risk, and lower firms' cost of capital. Critics, however, charge that these arrangements are mainly driven by accounting, not economic considerations and any reduction in the cost of capital is artificial (Reason 2002). Recent corporate scandals likewise suggest these entities can be used opportunistically by firm managers, with sponsoring firm investors remaining generally unaware of underlying risks.

The stated purpose of FIN 46 was to "achieve more consistent application of consolidation policies . . . [and] provide more complete information about the resources, obligations, risks, and opportunities of the consolidated enterprise" (FASB 2003, 4). Proponents of FIN 46 argued that gains in investor confidence counteracted any additional cost. Conversely, critics argued FIN 46 conveyed new, unsubstantiated risks to market participants and the complexity and opaqueness of FIN 46 offset any informational gains.[4]

Theoretically, in a frictionless market, FIN 46 should not affect firms' cost of capital as long as, upon adoption, investors perceive no change in expected cash flow and risk posture. In fact, increased transparency may lower firms' cost of capital (e.g., Botosan and Plumlee 2002). Departing from the theory of frictionless markets, other evidence suggests costs to investors could increase under a mandated standard such as FIN 46 (for instance, see evidence in Khurana [1991] regarding SFAS 94).

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[1] FIN 46 introduced the term "variable interest entities" to distinguish those entities from other off-balance sheet activities not subject to FIN 46. In this paper, we use the term VIE when discussing off-balance sheet activities subject to FIN 46 and we use the broader term "special purpose vehicles" (SPVs) otherwise.

[2] Besides their reporting advantages, these entities often isolated risk, in part because they were considered "bankruptcy remote" as the assets of the off-balance sheet entity were protected from bankruptcy proceedings if the sponsor declared bankruptcy. This isolation often allowed firms to obtain lower cost financing (Reason 2003).

[3] Structures involving off-balance sheet financing include various operating leases or operating guarantees; sale and leaseback transactions; real estate investment trust financing; public or private joint ventures; research and development partnerships; sale of receivables with recourse; and bank financial instruments such as guarantees, letters of credit, and loan commitments (Mills and Newberry 2005).

[4] The ongoing debate regarding proper accounting for off-balance sheet structures provides evidence of the continuing interest in and complexity of this issue. Most recently, SFAS 166, *Accounting for Transfers of Financial Assets—An Amendment of FASB Statement No. 140*, and SFAS 167, *Amendments to FASB Interpretation No. 46(R)*, alter accounting treatment for these structures.

The *Wall Street Journal* (2003) noted FIN 46 could require firms to consolidate as much as $500 billion in assets and liabilities, and some financial analysts warned these changes could "affect almost every line in the financial statements" (Credit Suisse 2003, 1), thereby potentially affecting loan covenants, credit ratings, and regulatory capital requirements. Practitioners further noted FIN 46 was "likely to raise the cost of capital and otherwise create additional costs for companies that may flow through to shareholders" (KPMG 2003, 6).

In short, firms that consolidated and/or disclosed VIEs potentially incurred costs associated with increased estimation risk, significant restructuring and/or adoption costs, and more reported leverage than before. Further, firms avoiding consolidation of VIEs upon adoption of the standard often incurred additional costs to restructure or divest their VIEs, and financial institutions that arranged/held VIEs often sought to avoid FIN 46 requirements.[5] FIN 46 adoption could affect future cash flows by constraining debt contracting options and forcing firms to incur additional costs, thereby raising investor concern about firms' information risk.[6] Each of these can increase the cost of capital.

Given the arguments, the economic impact of FIN 46 on investors' perceptions and firms' cost of capital is an empirical question. Taken together, the theory and potential economic effects lead to the following question: Did FIN 46 influence firms' cost of capital? In other words, prior to the implementation of FIN 46, were investors bearing the cost of hidden risk through overpriced equities? We find, relative to a control group of firms disclosing no material effect from FIN 46, affected firms experienced an increased cost of equity following adoption of the standard, with firms consolidating VIEs generally experiencing the greatest impact.

Our study contributes to the literature on the cost of capital implications of financial disclosure, the impact of mandated accounting change, and the use of SPVs. Although prior theoretical and empirical research (e.g., Healy and Palepu 2001; Barth and Schipper 2008) suggests increased disclosure lowers firms' costs of capital, we present a context where increased disclosure results in higher cost of equity capital, similar to Ashbaugh-Skaife et al. (2009). Our findings are also consistent with Lambert et al.'s (2007) argument that mandated disclosures affect firms differentially, depending on the ramifications for future cash flows. It may also be argued that the pre-FIN 46 reporting paradigm enabled some firms to reduce their cost of capital and FIN 46 rectified this understatement.[7] Finally, we provide new evidence on the consequences of SPV use. Feng et al. (2009) document prevalent use of those activities and link their use to earnings management; we show the implications for the cost of capital, which may be of concern to both managers and regulators. Furthermore, our findings may inform the ongoing discussion of regulatory issues related to off-balance sheet structures.[8]

Section II next provides background on FIN 46 and develops hypotheses. Section III describes our sample. Section IV describes our research design. Section V describes the results and section VI concludes.

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[5] Bens and Monahan (2008) present evidence that the volume of asset-backed commercial paper (ABCP) began to decline when FIN 46 was first proposed, and this decline is primarily attributable to a reduction in North American banks' sponsorship of ABCP. They also demonstrate North American banks entered into "costly" restructuring arrangements to avoid consolidation of their conduits according to the new accounting standards, suggesting FIN 46 appears to have real effects on investment activity and product-market competition that are theoretically linked to firms' cost of capital.

[6] By "information risk," we mean the likelihood that firm-specific information that is pertinent to investor pricing decisions is of poor quality (Francis et al. 2005b, 29).

[7] We thank an anonymous reviewer for illustrating this point with the Enron case. It is plausible that FIN 46 requirements would have affected Enron's disclosures. Perhaps it would have increased Enron's cost of capital in the short term (i.e., lowered its stock price), but this could have had the longer-term impact of decreasing the cost of capital by avoiding a financial collapse.

[8] For example, consider the recent issuance of SFAS 166, *Accounting for Transfers of Financial Assets—An Amendment of FASB Statement No. 140*, SFAS No. 167, *Amendments to FASB Interpretation No. 46(R)*, and the in-process joint project of the IASB and FASB, *Consolidation: Policy and Procedures*.

II. BACKGROUND AND DEVELOPMENT OF HYPOTHESES

FASB Interpretation Number 46 (FIN 46)

FIN 46 defined "variable interest entities" as thinly capitalized entities with equity interests insufficient to finance the operations of the entity and not possessing the usual relationship between ownership, control, and risk.[9] The Interpretation established a new consolidation model based on sharing of economic risk that applied to a wide array of previously unconsolidated entities (PricewaterhouseCoopers 2004). Under this new model, the firm exposed to the majority of expected losses or residual rewards is deemed the primary beneficiary and must consolidate the entity, regardless of equity ownership (FASB 2003, ¶14). FIN 46 also established new disclosure requirements for all owners of significant variable interest in VIEs, including primary beneficiaries (FASB 2003).[10] The standard, however, did not apply to all firms or even all SPVs. Among others, it specifically excluded qualifying special purpose entities (QSPEs) subject to the reporting requirements of FAS 140 (FASB 2003, ¶4).[11]

Impact of FIN 46 on Cost of Capital (H1)

For firms with SPVs, FIN 46 could result in no change, a decrease, or an increase in the firm's cost of capital. Theoretical and empirical support exists for each of these outcomes. Theoretically, in perfect and frictionless markets, a mandated accounting change will have no effect on the cost of capital unless the change shifts a firm's risk profile and expected cash flows. Proponents of this "no effect" outcome argue that informed investors were aware of SPVs prior to adoption of FIN 46.[12] Further, some prior research suggests that the market can see through the structure of off-balance sheet items and price associated risk efficiently (Bauman 2003; Jin et al. 2006). Thus, it is possible that firms with SPVs experienced no change in cost of capital post-FIN 46.

In contrast, increased transparency required by FIN 46 may *decrease* firms' cost of capital following adoption. Increased transparency afforded by increased disclosure could decrease the adverse selection problem and improve liquidity, thereby lowering cost of equity through greater demand for a firm's securities and/or reduced transaction costs (Amihud and Mendelson 1986; Diamond and Verrecchia 1991; Easley and O'Hara 2004). Further, the estimation risk literature predicts disclosure leads to increased transparency, generating lower cost of equity (e.g., Barry and

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[9] Specifically, these are structures in which "the total equity investment at risk is not sufficient to permit the entity to finance its activities without additional subordinated financial support" (FASB 2003, ¶5a); at-risk equity holders do not have the usual rights and obligations of shareholders (e.g., voting rights, obligations to absorb losses, and right to receive residual returns) (FASB 2003, ¶5b); or "the voting rights of some investors are not proportional to their obligations to absorb the expected losses of the entity, their rights to receive the expected residual returns, or both" (FASB 2003, ¶5c).

[10] Primary beneficiaries are required to disclose the nature, purpose, size, and activities of VIEs and information about assets pledged as collateral for the VIE's obligations. All other firms with significant interests in VIEs are also required to disclose the nature, purpose, size, and activities of the VIEs, the nature of their involvement with the VIE, and their maximum exposure to loss.

[11] QSPEs were granted this scope exception largely because of the passive, pass-through structure mandated by FAS 140. However, as the credit crisis unfolded through 2007 and 2008, many of these structures began to require active management, in particular through restructuring of associated loans. In discussing the subsequent removal of the QSPE exception, FASB Chairman Herz noted in 2008, "QSPEs were originally intended as passive entities but morphed into something different. Unfortunately, it seems that some folks used Qs like a punchbowl to get off-balance sheet treatment while spiking the punch. That has led us to conclude that now it's time to take away the punchbowl" (Herz 2008, 11). As the credit crisis revealed, the scope exception granted to QSPEs proved to be an important issue. For instance, Blundell-Wignall et al. (2009) estimated over $3 trillion in QSPEs remained off-balance for the 19 U.S. Supervisory Capital Assessment Program (SCAP) banks at the end of 2008.

[12] Certain structures had standard disclosure (e.g., synthetic leases reported as operating leases) and firms must list subsidiaries, including many SPVs, in Exhibit 21 of the SEC 10-K.

Brown 1985; Lambert et al. 2007; Barth and Schipper 2008).[13] Several empirical studies likewise link expanded disclosure policy to lower cost of equity capital (Botosan 1997; Botosan and Plumlee 2002; Francis et al. 2005a).

Finally, FIN 46 could potentially *increase* firms' costs of capital through the direct and indirect effects described in Lambert et al. (2007). They show higher quality disclosures directly affect the firm's assessed covariances with other firms' cash flows and corresponding cost of capital. Further, these effects can either increase or decrease costs of capital, depending on the expected value of the firm's future cash flows and the covariance of those cash flows with the sum of all cash flows in the market.

A change in disclosure requirements can also indirectly affect the cost of capital by changing a firm's real decisions. Some firms that are the primary beneficiaries of VIEs under FIN 46 incurred significant FIN 46 implementation costs and other firms chose to incur costs to dispose of, restructure, or terminate VIEs to avoid consolidation (KPMG 2003; Credit Suisse 2003). Such restructuring charges may represent continuing costs because one solution to preserve off-balance sheet treatment was to shift VIE consolidation to a third party, with corresponding, ongoing costs for such services (Bens and Monahan 2008). Consolidating off-balance sheet structures also affected financial ratios, thereby constraining debt capacity and in some cases triggering renegotiation of debt covenants (Credit Suisse 2003). Thus, FIN 46 could change management decisions and affect future cash flows by restricting future contracting and financing options. In similar circumstances in connection with changes required by SFAS 94,[14] Khurana (1991) argues that consolidation of non-homogeneous subsidiaries could interfere with the ability of the firm to raise debt capital in the future and also reduce firms' opportunity sets for accounting, financing, production, and investment decisions. Similarly, Livdan et al. (2009) show that financing constraints inhibit firms' ability to smooth dividend streams when faced with aggregate shocks. We therefore test the following hypothesis:

> **H1:** Firms with VIEs experienced a change in cost of capital after FIN 46 compared to firms not materially affected by the standard.

Differential Impact of Consolidating or Divesting VIEs on Cost of Capital (H2)

We next analyze whether actions that firms took with respect to their VIEs could differentially affect cost of capital post-FIN 46. FIN 46 generally requires the primary beneficiary of the VIE to consolidate the previously off-balance sheet entity; however, some firms responded by divesting, restructuring, or otherwise terminating relationships with those structures. By keeping VIEs off-balance sheet, firms could preserve greater financial flexibility and avoid violating debt covenants, thereby limiting the impact of FIN 46. For example, Bens and Monahan (2008) show that banks generally sought to avoid consolidation of asset-backed commercial paper conduits by reducing the number of new commercial paper SPVs and by restructuring existing VIEs so that separate entities held the residual interests. Such actions prevented an adverse impact on accounting ratios and credit ratings post-FIN 46. Importantly, those firms that take actions to preserve off-book treatment of their VIEs imply there is some economic advantage to keeping these structures off-book. However, any restructuring required to retain this off-book treatment may be costly, reducing the benefits associated with the VIE in question. Conversely, firms that consolidated VIEs following FIN 46 presumably did so because the VIEs are economically profitable for the firm, despite any corresponding increase in reported liabilities and associated effects.

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[13] In our context, "estimation risk" is investor uncertainty about the parameters of the cash-flow-generating process (e.g., Lewellen and Shanken 2002). This uncertainty is closely related to information risk, which is the quality of firm-specific information pertinent to investor pricing decisions (e.g., Francis et al. 2005b).

[14] SFAS 94 (*Consolidation of All Majority-Owned Subsidiaries: An Amendment of ARB No. 51 with Related Amendments of APB Opinion No. 18 and ARB No. 43*) concerns the consolidation of non-homogenous subsidiaries, while SPEs often involve activities central to the operations of the parent firm (Maines et al. 2003). Thus, although the impact of these two standards is not expected to be identical, evidence from the impact of SFAS 94 should be generally informative about the market treatment of off-balance sheet structures.

The preceding considerations suggest two potential effects on the cost of capital: (1) an indirect effect that these economically necessary VIEs have on future cash flows, and (2) a direct effect due to increased disclosure required by FIN 46, including the potentially greater transparency afforded by consolidation. Regardless of FIN 46's impact on firms' cost of capital, we expect the impact to vary across firms consolidating VIEs compared to those that otherwise kept those structures off their books.

Barth et al. (2003) show that recognized and disclosed amounts contribute to different levels of price informativeness, which may affect estimation risk. Consolidation may also constrain future financing and contracting ability (Khurana 1991) and ability to smooth cash flows (Livdan et al. 2009). We therefore expect that FIN 46 affects the cost of capital for consolidating firms differently than for firms that did not consolidate, suggesting our second hypothesis:

> **H2:** FIN 46 affected the cost of capital differently for firms that consolidated information pertaining to VIEs compared to firms that divested or restructured VIEs.

III. SAMPLE SELECTION AND DESCRIPTION

Identifying Firms With and Without VIEs

We gathered our sample from disclosures made within firms' annual filings using procedures similar to, but broader in scope than Zechman (2010). Given the relative lack of disclosure about VIEs prior to adoption of FIN 46, we also restricted our hand-gathered sample using the procedures of Feng et al. (2009).

First, we identified firms specifically making disclosure(s) related to FIN 46. To gather data on such firms, we used 10-K Wizard to search annual reports filed in 2004, primarily for the fiscal year ending 2003.[15] We searched for all occurrences of "FIN 46" and "variable interest entity/entities" in these filings. This process produced 551 FIN 46 mandated disclosures from 352 firms. We classified these disclosures by whether the firm reported the VIE on a consolidated basis after FIN 46 adoption and by the purpose of each VIE. Table 1 provides a breakdown of this group, and sample disclosures are provided in Appendix A. As part of this data gathering procedure, we also identified 1,753 firms that reported "no material effect" or "no effect" from FIN 46 or FIN 46(R), but provided no further details about associated VIEs.[16]

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[15] Since FIN 46 was adopted in January 2003, the first 10-K reports referencing FIN 46 were filed with the SEC in early 2003. FIN 46 was subsequently superseded by FIN 46(R), which was issued in December 2003. FIN 46(R) changed a number of requirements but did not alter the basic thrust of FIN 46. Although our primary sample of interest is composed of firms that adopted FIN 46/FIN 46(R) for fiscal years ending in 2003, FIN 46(R) allowed staggered effective dates; as a result, not all public firms had adopted the new provisions for VIEs for the fiscal year ending in 2003. Specifically, while firms had to adopt either FIN 46 or FIN 46(R) for the first reporting period ending after December 15, 2003, some entities have year-ends earlier than December 15. Further, if firms applied FIN 46 for fiscal years ending in 2003, they had an extended deadline to apply FIN 46(R) in the first reporting period ending after March 15, 2004. Small business issuers had a further extended deadline to apply FIN 46(R) in periods ending after December 15, 2004.

[16] While it is not possible to know with certainty why firms would disclose no material effect from a recent standard, professional guidance provides two possibilities. Given the staggered adoption dates of FIN 46 and FIN 46(R) (see footnote 15), it is possible that firms had not fully adopted the standard; in fact, some indicate this is the case. For standards issued but not yet adopted, SAB 74 states, "In those instances where a recently issued standard will impact the preparation of, but not materially affect, the financial statements, the registrant is encouraged to disclose that a standard has been issued and that its adoption will not have a material effect on its financial position or results of operation" (footnote 3). Professional auditing guidance likewise indicates, "If an accounting change has no material effect on the financial statements in the current year, but the change is reasonably certain to have substantial effect in later years, the change should be disclosed in the notes to the financial statements (AU Section 420.20)."

TABLE 1

Description of Sample Firms, Types of VIEs, FIN 46 Compliance, and Industry Affiliations

Panel A: Counts of Firms by Category Based on Use of VIEs and Compliance with FIN 46

Category	Firms Identified (n)	Firms in Final Sample (n)
1. Firms complying with FIN 46 (*F46*)	352	244
2. Firms reporting no material FIN 46 effect (*No_F46_Impact*)	1,753	1,145
Totals	2,105	1,389

Panel B: Types of VIEs and Actions Taken to Comply with FIN 46

	Action Taken					
	On Books		Off Books		Total	
Types of VIEs	n	Percent	n	Percent	n	Percent
Financial VIEs	48	33%	33	34%	81	33%
Operating VIEs	63	43%	52	53%	115	47%
Other VIEs	3	2%	2	2%	5	2%
Multiple Types	32	22%	11	11%	43	18%
Totals	146	100%	98	100%	244	100%

Panel C: Counts of Firms by Industry and FIN 46 Firm Categories

Industry	Description	*No_F46_ Impact* Firms	*F46 Firms*
1	Food	27	7
2	Mining and Minerals	10	1
3	Oil and Petroleum Products	43	7
4	Textiles, Apparel, and Footwear	26	2
5	Consumer Durables	29	7
6	Chemicals	12	6
7	Drugs, Soap, Perfumes, and Tobacco	38	4
8	Construction and Construction Materials	29	16
9	Steel Works, etc.	16	1
10	Fabricated Products	7	2
11	Machinery and Business Equipment	170	21
12	Automobiles	13	4
13	Transportation	44	12
14	Utilities	17	16
15	Retail Stores	81	19
16	Banks, Insurance Companies, and Other Financials	199	54
17	Other	384	65
Total		1,145	244

(continued on next page)

TABLE 1 (continued)

Panel A shows the counts of firms in each firm category: (1) firms that complied with FIN 46 according to disclosures in their 10-K reports; and (2) firms reporting that FIN 46 had no material effect. The number of firms in the final sample reflects firms with at least one implied cost of equity capital measure.

Panel B shows the counts of F46 firms that consolidated VIEs or otherwise brought VIEs onto their books and firms that took actions to keep VIEs off their books (e.g., restructuring, terminating) and the counts in each of four types of VIEs: (1) Financial (lease or finance VIEs); (2) Operating (alliances, affiliates, or joint ventures); (3) Other (trust-preferred securities or multiple unspecified bundles of VIEs), and (4) Firms reporting multiple types of VIEs.

Panel C recaps the number of firms in each firm category by industry. We use Fama and French industry definitions. Firms are categorized as described in Panel A of this table.

Next, for the sample, we used Feng et al.'s (2009) search process to verify the presence or absence of likely VIEs or SPVs in Exhibit 21 of Form 10-K. Feng et al. (2009) document the widespread use of SPVs by utilizing filings made with the SEC related to affiliates.[17,18] Feng et al. (2009) identify likely SPVs as those subsidiaries and affiliates listed in Exhibit 21 with names that contain "Limited Partnership," "L.P.," "LP," "LLC," "L.L.C.," or "trust," since SPVs are typically organized as one of those legal structures. Given inconsistent disclosure before FIN 46, it may be that VIEs organized via one of these common legal structures were more likely to be identified by market participants prior to adoption of FIN 46.[19] Absent voluntary disclosure, VIEs structured by other means may have been difficult to identify prior to adoption of FIN 46. As a result, we further restrict our hand-gathered sample by requiring that firms disclose at least one likely VIE in Exhibit 21 before adoption of FIN 46.[20] By placing this constraint on our sample, we bias against finding a change in cost of capital post-FIN 46, as our main sample includes only those firms with the most obvious, and therefore most likely understood, VIEs before FIN 46.

Categories of Firms in the Sample

We then used firm Central Index Keys (CIKs) to gather corresponding Compustat data and data necessary to estimate valid proxies for implied cost of equity capital as discussed in the next

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[17] SEC regulations require that firms list subsidiaries and affiliates in Exhibit 21 to their annual 10-K. Specifically, U.S. Federal Regulations (Regulation S-X) 17CFR 229.601(a) specify that public filers submit Exhibit 21, Subsidiaries of the Registrant, as part of the annual 10-K. U.S. Federal Regulations (Application of Regulation S-X) 17 CFR part 210.1-02(x) define a subsidiary as "an affiliate controlled by such person directly, or indirectly through one or more intermediaries" with control defined in 17 CFR part 210.1-02(g) as "the possession, direct or indirect, of the power to direct or cause the direction of the management and policies of a person, whether through the ownership of voting shares, by contract, or otherwise." In other words, subsidiaries in Exhibit 21 may include entities not consolidated under the provisions of U.S. GAAP pre-FIN 46.

[18] Based on our sample-gathering procedures, the Feng et al. (2009) sample appears to include at least some structures that were not VIEs affected by FIN 46, (e.g., exempted structures such as QSPEs or entities such as active LPs and LLCs that are not SPVs).

[19] For example, upon its adoption of FIN 46, ConAgra Foods, Inc. disclosed detailed information about multiple VIEs impacted by the standard. A search of the firm's prior-year filing, however, reveals no obvious indication of off-balance sheet structures (e.g., keywords such as "special purpose entity" "off-balance sheet" or "synthetic lease" do not appear within the report), but four likely SPVs (following the Feng et al. [2009] criteria) appear in Exhibit 21.

[20] To replicate the search process of Feng et al. (2009), we use the proximity search feature within directEDGAR© to identify which firms listed in Exhibit 21 subsidiaries or affiliates with names containing the terms identified by Feng et al. (2009). Additionally, but consistent with their intent, we also searched for Exhibit 21 entries using the terms "limited," "ltd.," "ltd," "ltda," or "limitada" both to broaden our search and capture similar terms in Spanish and Portuguese. After the proximity search, we used tools in directEDGAR© to manually examine each Exhibit 21 to confirm the results. Because Feng et al. (2009) note that firms sometimes list subsidiaries in Exhibit 22 rather than 21, we also included this exhibit in our search. As such, this component of our procedure represents only a refinement to the process of Feng et al. (2009) in that we expand the keyword set used.

section. After identifying sample firms as described above and verifying data availability, we formed two categories as follows:

1. *F46* firms are those 244 firms that disclosed an effect from FIN 46 and were verified to have likely SPVs disclosed in Exhibit 21; and
2. *No_F46_Impact* firms are those 1,145 firms that disclosed that FIN 46 had no material effect on their financial results.[21]

Panel A of Table 1 describes our final sample of 1,389 firms. Panel C of Table 1 reports the sample firm groups according to Fama and French industry classifications. As this panel shows, firms across both sample categories are concentrated in the Fama and French "other" category. However, in the "machinery and business equipment" group, more firms report no effect from FIN 46 than disclose an effect from FIN 46.

Categories of F46 Firms According to the Compliance Action Taken

To examine H2, as reported in Panel B of Table 1, we further categorized *F46* firms according to the action taken in response to the standard. We categorize firms by whether they (1) brought the VIEs onto their books (*on books*) by consolidation, purchase of assets, etc. or (2) did not bring VIEs onto their books (*off books*) because the firm was not the primary beneficiary, restructured its interest in the entity, etc. Panel B shows that 146 *F46* firms brought VIEs onto their books and 98 firms did not.

Categories of VIEs for FIN 46 firms

Panel B of Table 1 also shows the breakdown of the *F46* firms according to the type of VIE reported. Approximately one-third of firms reported financial VIEs, almost half the F46 firms reported operating VIEs, and 18 percent of *F46* firms reported multiple types of VIEs. We separately categorize firms with multiple types, since it is difficult to determine the dominant VIE type for those firms. In general there is little difference in the types of VIEs brought onto books compared to those kept off books. However, firms with multiple types were more likely to bring at least one VIE onto their books.

IV. RESEARCH METHODS, IMPLIED COST OF EQUITY PROXIES, AND EMPIRICAL MODELS

Defining the Pre- and Post-FIN 46 Periods

Our goal is to examine changes in implied cost of equity capital following the adoption of FIN 46. However, for many firms that adoption took place over several months or even over multiple years, as FIN 46 was issued and subsequently revised. Thus, there was likely some delay while analysts incorporated information into their estimates.

Firms issued the first financial statements under FIN 46 early in calendar year 2003, and we expect the full effect of FIN 46 to be reflected in sample firms' cost of equity capital measures within 36 months as firms take actions to consolidate VIEs, make required disclosures, or reduce

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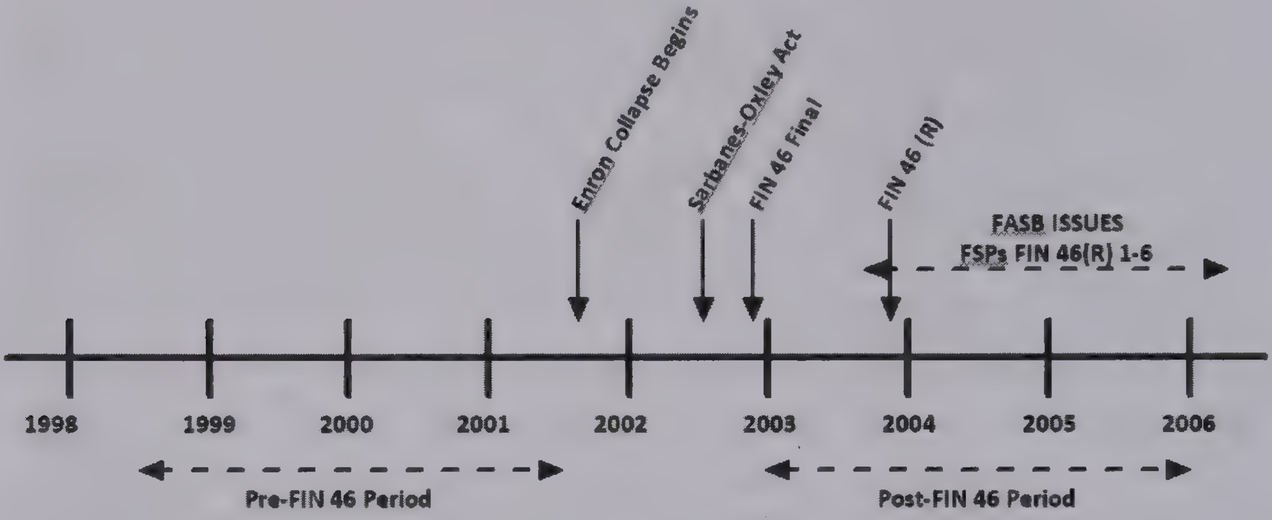
[21] While it is entirely possible that this group restructured VIEs so that FIN 46 would have no material effect on the firm, it is not possible to determine this from the disclosure provided within the annual report. We separate these firms because, considering the regulatory guidance discussed above, by making disclosures of this type, it is possible that these firms expect their use of VIEs may have a material effect in future periods, thus altering the current risk position.

FIGURE 1
Timeline



This figure presents a timeline of important and relevant events leading up to and following the issue of FIN 46, and it shows our control period, the pre-FIN 46 time period, and our focus for this research, the post-FIN 46 time period. We define the pre-FIN 46 period as the 3rd calendar quarter of 1998 through the 2nd calendar quarter of 2001 and the post-FIN 46 period as the 1st calendar quarter of 2003 through the 4th calendar quarter of 2005. We exclude the transitory period beginning the 3rd calendar quarter of 2001 and ending the 4th calendar quarter of 2002. FSP indicates FASB Staff Position document.

their participation in VIEs. We therefore define the post-FIN 46 time period as the 36-month period from the first calendar quarter of 2003 through the fourth quarter of 2005.

As a benchmark, we compare firms' cost of equity capital against a similar pre-FIN 46 period.[22] Since Enron Corporation's collapse began in the Fall 2001 and the FASB's initial development of FIN 46 took place during 2002, those events could affect the cost of equity capital for firms that participated in VIEs and confound any comparison. Thus, we exclude from analysis firm-quarter observations for the 18 months beginning in the third calendar quarter of 2001 and ending in the fourth calendar quarter of 2002.[23] We therefore define the pre-FIN 46 time period as the 36 months immediately preceding that transitory period, beginning in the third calendar quarter of 1998 and ending in the second calendar quarter of 2001. We expect that measures of cost of equity capital during this time period were untainted by the notoriety generated by Enron's collapse and subsequent actions by the FASB to change treatment of special purpose entities. Figure 1 graphically portrays important events, such as the collapse of Enron and the adoption dates of FIN 46 and FIN 46(R), in relation to the pre- and post-FIN 46 periods.

Proxies for the Implied Cost of Equity Capital

We employ multiple accounting-based proxies for firms' implied cost of equity capital that are widely used in recent finance and accounting literature (see Botosan and Plumlee [2005] for a detailed discussion). We select these accounting-based proxies because they are forward-looking measures of expected returns. Furthermore, these proxies rely on analysts' estimates of future firm performance, and given the complexity of these structures and the limited disclosure about firms' involvement with VIEs prior to FIN 46, analysts should be well positioned to assess the impact of VIEs on firm performance both before and after FIN 46.

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[22] We also tested shorter (one-year and two-year) pre- and post-FIN 46 periods with consistent results.

[23] The implications of our analyses are generally unchanged if we include these 18 quarters. However, we find substantial variation in estimates of implied cost of equity capital during this period consistent with the level of uncertainty connected with the Enron scandal and the prospective regulation of VIEs.

Consistent with prior research (e.g., Li 2010; Hail and Leuz 2006, 2009; Dhaliwal et al. 2007), our primary dependent variable is the average of four proxies. Ogneva et al. (2007), Chen et al. (2010), and Hail and Leuz (2006) all advocate the use of multiple measures to ensure that results are robust to method choice; for this reason we also present results for multiple measures.[24]

First, we use the proxy based on Easton's (2004) PEG ratio since Botosan and Plumlee (2005) find this measure to be predictably and consistently related to other risk measures. Additionally, we use the proxy based on Ohlson and Juettner-Nauroth (2005), since Gode and Mohanram (2003) find that this proxy is reliably related to other risk measures as well as *ex post* returns. We also use proxies developed by Gebhardt et al. (2001) and Claus and Thomas (2001), which are based on residual income valuation models (e.g., Ohlson 1995) and widely used in other accounting research (e.g., Li 2010; Dhaliwal et al. 2007; Daske 2006; Hail and Leuz 2006, 2009; Hribar and Jenkins 2004). For each of the cost of equity capital measures, we use the last analyst earnings per share and long-term growth estimates from I/B/E/S in the calendar quarter and market prices as of the end of the quarter, consistent with Dhaliwal et al. (2007). We describe the calculation of each of these proxies in Appendix B.

Defining Control Variables for Analysis of Implied Cost of Equity Capital

Prior research examining implied cost of equity capital employs a broad set of control variables.[25] Until recently, little theory has been available to guide the choice of control variables. However, Hughes et al. (2009) demonstrate the difference between the implied cost of equity capital and expected returns is a function of beta and cash flow volatility, leverage, and growth. They conclude that studies employing implied cost of equity capital as a proxy for expected return should control for those factors. Thus, we control for the following:[26]

ubeta = coefficient from a single factor value-weighted market model measured during the last month of each quarter using the previous 60 months' returns including firms with at least 24 observations in that period, unlevered by dividing by (1 + long-term debt/market value of equity);

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[24] We acknowledge the difficulty in identifying a reliable measure of the cost of equity capital, reflected in discussion in the literature about deficiencies in existing *ex ante* measures of the implied cost of equity capital (Easton and Monahan 2005; Guay et al. 2005; Botosan et al. 2010; Easton and Monahan 2010). These limitations apply to our study as well as to all other studies that examine accounting-based cost of capital measures.

[25] For example, Dhaliwal et al. (2007) control for factors suggested by Gebhardt et al. (2001), as well as Fama and French (1996) factors. Ogneva et al. (2007) and Ashbaugh-Skaife et al. (2009) also control for firm characteristics, such as mergers, percentage of losses, z-score decile, extent of foreign operations, and number of segments. To the extent data for these controls are available quarterly, we also estimated our models using the controls proposed by these referenced studies and found results consistent with those reported here.

[26] Because Hughes et al. (2009) did not specify empirical measures of constructs such as cash flow volatility and growth, we examined several alternative measures using, for example, free cash flow and return on equity. We also acknowledge these control variables likely do not fully correct for all problems with the underlying cost of capital measures. Use of these controls, however, should improve our analysis compared to other available sets of controls. To the degree these are related to use of SPVs, use of these controls should bias against our hypothesized results. Further, although we report results using the control variables defined here, the results are also robust to alternate sets of control variables including controls relating to performance, firm structure and governance, compensation effects, and information environment.

sd_ocf = standard deviation of operating cash flows over the previous five years including firms with at least three annual observations in that period, log transformed;[27]
sd_beta = standard deviation of the four beta values over the previous year for each firm, log transformed;
leverage = long-term debt divided by the market value of equity;
oi_growth = current quarterly operating income before depreciation and amortization divided by the previous quarter's value; and
size = market value of equity (in millions of dollars) at the end of the previous fiscal year, log transformed. Inclusion of this control is consistent with work such as Kothari et al. (2009) given the known relation between size and equity returns (Berk 1995).

Descriptive Statistics for Sample Firms Pre- and Post-FIN 46

Panel A of Table 2 presents descriptive statistics for the full sample of 1,389 firms for the pre-FIN 46 for three periods: (1) the pre-FIN 46 period, (2) the excluded time period (post-Enron scandal and during FIN 46 development), and (3) the post-FIN 46 period. We limited the sample to those firm-quarter observations with non-missing data for all variables. The mean (median) market capitalization ranges from $7.2 billion ($705 million) pre-FIN 46 to $6.7 billion ($1 billion) post-FIN 46. While leverage decreased over the 1998 through 2005 period, the average implied cost of equity capital (*re_ave*) likewise decreased from a mean value of 9.9 percent pre-FIN 46 to 9.0 percent post-FIN 46 consistent with Dhaliwal et al. (2007), who examined changes in implied cost of equity capital related to the Jobs and Growth Tax Relief Reconciliation Act of 2003.[28]

Tests Investigating Differences among Firm Categories

Panel B of Table 2 compares descriptive statistics for *F46* and *No_F46_Impact* firms. These groups have almost identical average implied cost of capital (*re_ave*), although *F46* firms have lower *re_peg* values and higher *re_ct* values. *F46* firms also have lower unlevered beta (*ubeta*), and lower cash flow volatility (*sd_ocf*) than the *No_F46_Impact* firms. *F46* firms are also generally larger and have higher leverage than *No_F46_Impact* firms. All measures taken together suggest that *F46* firms present lower risk profiles than the *No_F46_Impact* firms.

Figure 2 compares average implied cost of equity capital (*re_ave*) for the two groups of firms by quarter, adjusted for the mean quarterly cost of capital for all firms with available implied cost of equity capital measures. Thus, zero on the horizontal axis represents average implied cost of capital for all public firms for which implied cost of capital measures could be calculated each quarter. The lines indicate changes for *No_F46_Impact* and *F46* firms relative to a broad universe of firms. Clearly, *F46* firms have lower costs of equity capital pre-FIN 46; however, that advantage disappears by the end of 2001 and the *F46* firms have consistently higher average implied cost of equity capital values post-FIN 46.

Correlations among Implied Cost of Equity Capital Proxies and Other Risk Measures

Table 3 presents correlations among the cost of capital proxies and related control variables. As expected, the proxies for implied cost of equity capital are highly correlated. Those proxies are also significantly related to a variety of risk measures, consistent with prior research. The average cost of equity capital measure (*re_ave*) is significantly and negatively related to firm size and positively

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[27] We added 1 to the standard deviation of cash flows and beta figures before log transformation.

[28] Although not shown in the table, interest rates on five-year and ten-year Treasury bonds also decreased from over 5 percent pre-FIN 46 to 4 percent or less post-FIN 46, reflecting economy-wide decreases in the cost of capital.

TABLE 2

Descriptive Statistics

Panel A: Descriptive Statistics by Period Pre- and Post-FIN 46 (n = 20,350 Firm-Quarter Observations)

Variable	Mean	Std. Dev.	Q1	Median	Q3	n
1998 Qtr 3 to 2001 Qtr 2 (Pre-FIN 46 Period)						
re_ave	0.099	0.029	0.079	0.097	0.116	5,117
re_ct	0.092	0.030	0.070	0.088	0.108	5,117
re_gls	0.102	0.031	0.080	0.100	0.122	5,117
re_peg	0.100	0.048	0.068	0.091	0.122	5,117
re_oj	0.104	0.035	0.079	0.100	0.124	5,117
ubeta	0.852	0.727	0.367	0.684	1.147	5,117
sd_ocf	0.059	0.055	0.027	0.045	0.072	5,117
sd_beta	0.200	0.108	0.124	0.180	0.253	5,117
leverage	0.372	0.571	0.008	0.143	0.483	5,117
oi_growth	1.088	0.546	0.862	1.042	1.221	5,117
size ($mm)	7,205.6	34,628.7	260.3	705.0	2,346.4	5,117
2001 Qtr 3 to 2002 Qtr 4 (Excluded Period—post-Enron scandal and during FIN 46 development)						
re_ave	0.093	0.022	0.078	0.090	0.105	2,844
re_ct	0.079	0.025	0.062	0.076	0.091	2,844
re_gls	0.097	0.024	0.081	0.096	0.111	2,844
re_peg	0.102	0.046	0.072	0.093	0.121	2,844
re_oj	0.093	0.028	0.075	0.091	0.108	2,844
ubeta	0.834	0.854	0.284	0.584	1.097	2,844
sd_ocf	0.057	0.049	0.026	0.044	0.070	2,844
sd_beta	0.157	0.088	0.096	0.141	0.200	2,844
leverage	0.338	0.520	0.008	0.142	0.420	2,844
oi_growth	1.081	0.554	0.841	1.026	1.222	2,844
size ($mm)	8,252.7	36,371.5	385.8	969.4	2,844.4	2,844
2003 Qtr 1 to 2005 Qtr 4 (Post-FIN 46 Period)						
re_ave	0.090	0.019	0.077	0.087	0.099	6,602
re_ct	0.072	0.022	0.057	0.070	0.082	6,602
re_gls	0.091	0.022	0.077	0.090	0.104	6,602
re_peg	0.100	0.039	0.075	0.093	0.117	6,602
re_oj	0.095	0.030	0.076	0.092	0.108	6,602
ubeta	1.007	0.906	0.383	0.736	1.341	6,602
sd_ocf	0.060	0.064	0.025	0.044	0.070	6,602
sd_beta	0.069	0.081	0.000	0.043	0.119	6,602
leverage	0.264	0.425	0.001	0.112	0.323	6,602
oi_growth	1.096	0.534	0.869	1.047	1.233	6,602
size ($mm)	6,736.2	26,398.0	403.7	1,017.3	3,021.5	6,602

(continued on next page)

related to leverage, consistent with Botosan and Plumlee (2005). It is also significantly and positively related to measures of operating cash flow and beta volatility (*sd_ocf* and *sd_beta*), consistent with Hughes et al.'s (2009) arguments that implied cost of equity capital is a function of beta and cash flow volatility, as well as leverage and growth. Similar to prior research, all the implied cost of capital measures, except *re_peg*, are negatively related to beta.

TABLE 2 (continued)

Panel B: Descriptive Statistics by Category of Firm

	1. *No_F46_Impact* Firms		2. *F46 Firms*		Comparison
Variable	**Mean**	**Median**	**Mean**	**Median**	**Prob(1 equals 2)**
re_ave	0.094	0.090	0.094	0.091	ns
re_ct	0.079	0.075	0.084	0.080	**
re_gls	0.096	0.093	0.097	0.094	ns
re_peg	0.102	0.094	0.094	0.088	***
re_oj	0.097	0.094	0.100	0.097	ns
ubeta	0.976	0.727	0.733	0.551	***
sd_ocf	0.063	0.048	0.043	0.035	***
sd_beta	0.134	0.121	0.127	0.118	ns
leverage	0.271	0.102	0.464	0.225	***
oi_growth	1.096	1.041	1.073	1.041	**
size ($mm)	6,904.1	705.3	8,155.9	2,184.2	***
n	11,152		3,411		

, * Indicate significance at 5 percent and 1 percent levels, respectively.

ns indicates not significant.

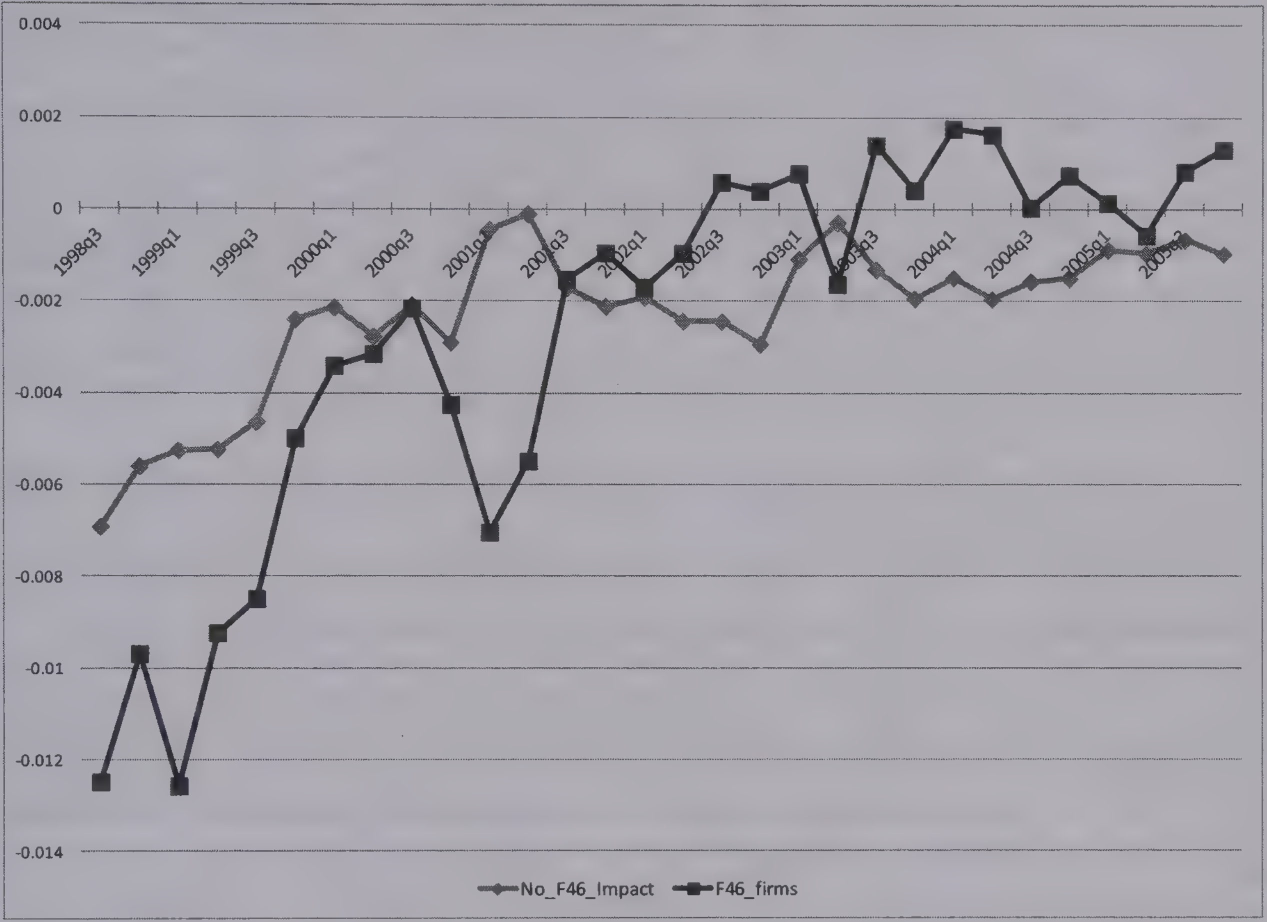
Panel A presents descriptive statistics for sample firms by period: (1) Pre-FIN 46 period is the 12 calendar quarters beginning with the 3rd quarter 1998 and ending with the 2nd quarter 2001, (2) the excluded period is the six quarters beginning with the 3rd quarter 2001 and ending with the 4th quarter 2002, and (3) the Post-FIN 46 period is the 12 quarters beginning with the 1st quarter 2003 and ending with the 4th quarter 2005. We limit the sample to those firm-quarter observations for which all the variables are available.

Panel B presents mean and median values for sample firms across all three time periods according to firm category: (1) *No_F46_Impact* firms are those firms that indicated that FIN 46 had no meaningful effect on their financial results, and (2) *F46 firms* are those firms that reported complying with FIN 46. The Comparison column indicates whether there is a significant difference between values for *No_F46_Impact firms* and *F46 firms* using two-tailed tests adjusting for intrafirm correlation with clustered standard errors.

Variable Definitions:

re_ave = average of other four measures of implied cost of capital;
re_ct = implied cost of capital based on Claus and Thomas (2001);
re_gls = implied cost of capital based on Gebhardt et al. (2001);
re_peg = implied cost of capital based on Easton (2004) PEG ratio;
re_oj = implied cost of capital based on Ohlson and Juettner-Nauroth (2005); *re* values are winsorized 1 percent at the high end of the distribution to limit the influence of extreme observations;
ubeta = unlevered beta, calculated by dividing beta by (1 + long-term debt/market value of equity); beta values are based on a single factor (value-weighted) market model based on monthly observations for firms with at least 30 observations in the previous 60 months;
sd_ocf = standard deviation of operating cash flows over previous five years plus 1, log transformed;
sd_beta = standard deviation of beta values over previous year plus 1, log transformed (proxy for beta volatility as suggested by results of Hughes et al. [2009]);
leverage = long-term debt divided by market value of equity, winsorized 1 percent at each end of the distribution;
oi_growth = operating income this quarter divided by operating income in the previous quarter, winsorized 1 percent at each end of the distribution; and
size = market value of equity ($mm) at the end of the previous fiscal year.

FIGURE 2
Quarterly Average Implied Cost of Capital (Mean-Adjusted) from 1998 to 2005 by Firm Category



This chart shows the average, quarterly mean-adjusted, implied cost of equity capital calculated quarterly over the period 1998 to 2005 for categories of firms defined as follows: (1) *No_F46_Impact* firms are those firms that indicated that FIN 46 had no meaningful effect on their financial results; and (2) *F46* firms are those firms that disclosed an effect from FIN 46. Quarterly mean values are based on all firms with available implied cost of equity capital measures for the quarter.

Models for Multivariate Tests of Implied Costs of Equity Changes Post-FIN 46 (H1)

To test H1, we examine the change in implied cost of equity capital post-FIN 46 for two different categories of firms: (1) the *No_F46_Impact* group with VIEs or SPVs not materially affected by FIN 46, and (2) the *F46* firms that complied with FIN 46, as described earlier, using the following basic model (firm and time subscripts suppressed):

$$re = b_0 + b_1 post46 + b_2 F46 + b_3 post46 \times F46 + b_4 ubeta + b_5 sd\_ocf + b_6 sd\_beta + b_7 leverage + b_8 oi\_growth + b_9 size + \varepsilon. \quad (1)$$

In Equation (1) *re* is one of the four implied cost of equity capital measures or the average measure and *post46* indicates the change in regime from pre- to post-FIN 46. The interaction term coefficient, b_3, indicates whether cost of capital changed for *F46* firms relative to *No_F46_Impact* firms, after controlling for other factors expected to affect cost of equity capital.

TABLE 3

Correlations

	re_ave	***re_ct***	***re_gls***	***re_peg***	***re_oj***	***ubeta***	***sd_ocf***	***sd_beta***	***leverage***	***oi_growth***	***size***
re_ave	1	0.764	0.758	0.700	0.665	−0.187	0.048	0.134	0.345	−0.065	−0.327
re_ct	0.789	1	0.735	0.229	0.502	−0.292	−0.036	0.178	0.380	−0.069	−0.183
re_gls	0.769	0.778	1	0.370	0.294	−0.151	0.043	0.181	0.335	−0.081	−0.351
re_peg	0.737	0.292	0.377	1	0.312	0.064	0.147	0.059	0.138	−0.009	−0.341
re_oj	0.667	0.458	0.284	0.269	1	−0.247	−0.082	−0.013	0.197	−0.069	−0.050
ubeta	−0.183	−0.290	−0.167	0.047	−0.225	1	0.368	0.032	−0.512	0.042	−0.014
sd_ocf	0.026	−0.038	0.000	0.110	−0.040	0.390	1	0.124	−0.361	0.039	−0.296
sd_beta	0.136	0.171	0.166	0.093	−0.005	0.079	0.086	1	0.027	−0.001	−0.158
leverage	0.368	0.391	0.374	0.212	0.164	−0.351	−0.190	0.059	1	−0.030	0.013
oi_growth	−0.035	−0.036	−0.049	0.011	−0.048	0.026	0.022	0.002	−0.013	1	−0.021
size ($mm)	−0.349	−0.218	−0.371	−0.348	−0.071	−0.025	−0.228	−0.153	−0.087	−0.042	1

Table 3 presents Pearson correlations below the diagonal and Spearman correlations above the diagonal. Absolute correlations > 0.02 significant at 1 percent. n equals 14,563 firm-quarter observations. Variables are as defined in Table 2, except *size* is log transformed for this table.

Models for Multivariate Tests of Implied Costs of Equity Changes by VIE Action (H2)

To test H2, we divided *F46* firms into two subcategories based on the action they took in response to FIN 46: (1) *F46_on_books* firms brought VIE assets and liabilities onto the firm's balance sheet, and (2) *F46_off_books* firms kept VIE assets and liabilities off the firm's balance sheet.[29] We test changes in implied cost of equity capital using the following model where variables other than firm action categories are as previously defined:

$$\begin{aligned} re = {} & b_0 + b_1 post46 + b_2 F46\_off\_books + b_3 F46\_on\_books + b_4 post46 \times F46\_off\_books \\ & + b_5 post46 \times F46\_on\_books + b_6 ubeta + b_7 sd\_ocf + b_8 sd\_beta + b_9 leverage \\ & + b_{10} oi\_growth + b_{11} size + \varepsilon. \end{aligned} \tag{2}$$

Econometric Considerations

Our sample is an unbalanced panel of 1,389 firms over 24 quarters. Peterson (2009) compared alternative approaches for dealing with serial correlation in panel datasets and concluded that, in the presence of unobserved firm effects, clustered standard errors are unbiased and produce correctly sized confidence intervals while controlling for those unobserved effects. We therefore present results estimated using OLS with clustered standard errors adjusted for intrafirm correlation.[30]

Because firms chose their policies following FIN 46, self-selection bias may arise.[31] However, it is reasonable to assume that firms selected actions to minimize the adverse impact of FIN 46. Thus, firm choice should act to reduce the impact of FIN 46, counter to our hypotheses that FIN 46 affected the cost of equity capital.

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[29] Some firms with multiple VIEs took both actions and we classified those firms as *F46_on_books* firms.

[30] We also analyze our models using the dual controls suggested by Gow et al. (2010) to account for cross-sectional and time-series dependency. While this method does cause small changes in the standard errors, it does not change the results reported in the following sections.

[31] Firms also chose to participate in off-balance sheet activities, but that choice should not affect post-FIN 46 cost of capital.

V. RESULTS

Results for Tests of Implied Cost of Equity Changes by Firm Category (H1)

Table 4 presents results comparing implied cost of equity capital pre- and post-FIN 46 by firm category. Panel A presents univariate tests of average implied cost of equity capital (*re_ave*) values for the pre- and post-FIN 46 periods. Both *F46* and *No_F46_Impact* firms experienced, on average, significantly lower cost of equity capital post-FIN 46 compared to pre-FIN 46. However, costs for *No_F46_Impact* firms decreased, on average, 1.1 percent, while *F46 firms* decreased only 0.6 percent and these differences are significantly different from one another ($p < 0.01$, one-tailed test). As shown in Figure 2, across adoption of FIN 46, cost of capital for *F46* firms changed from less than to greater than that of *No_F46_Impact* firms.

Panel B of Table 4 presents results from the multivariate analysis using Equation (1). The *post46* variable indicates the change in regime from pre- to post-FIN 46 and is significantly negative. The interaction term, *post46* × *F46* firms, then captures the post-FIN 46 change for *F46* firms relative to the *No_F46_Impact* firms. Focusing on results for the average implied cost of equity capital (*re_ave*), the coefficient on *post46* × *F46* is positive and highly significant ($p < 0.01$). Consistent with prior research (Botosan and Plumlee 2005), leverage is significant and positively related to *re_ave*, while *size* is significant and negatively related to *re_ave*, as well as to all of the other cost of equity capital proxies. Unlevered beta (*ubeta*) is significantly negatively related to *re_ave*. Among the control variables suggested by Hughes et al. (2009) and shown in Equation (1), only beta volatility (*sd_beta*) is not significantly related to *re_ave*, although it is significantly related to *re_gls*, *re_peg*, and *re_oj*. Overall, these results support H1. Cost of equity capital increased for firms with VIEs subject to FIN 46 relative to firms that reported no material effect from FIN 46.

Results for Tests of Implied Cost of Equity Changes by VIE Action (H2)

To examine the impact of actions taken to comply with FIN 46, we divided the *F46* firms into two categories according to whether they kept VIEs off (*F46_off_books*) or brought VIEs onto (*F46_on_books*) their books. Figure 3 shows that pre-FIN 46, the cost of equity capital moves together for those two groups; however, post-FIN 46, the average cost of equity capital diverges and *F46_on_books* firms have higher adjusted cost of equity capital than *F46_off_books* firms.

Panel A of Table 5 presents univariate results comparing the implied cost of equity capital pre- and post-FIN 46 for *No_F46_Impact*, *F46_off_books*, and *F46_on_books* firms. Both *No_F46_Impact* and *F46_off_books* firms experienced significant decreases in *re_ave* from pre- to post-FIN 46, but *F46_on_books* firms did not. Furthermore, the *F46_on_books* firms experienced a significantly smaller decrease than the *No_F46_Impact* firms ($p < 0.01$, one-tailed test) as well as the *F46_off_books* firms ($p < 0.05$, one-tailed test).

Panel B of Table 5 presents results from the multivariate analysis using Equation (2). Again, the *post46* variable indicates the change in regime from pre- to post-FIN 46 and is significantly negative. The interaction terms *post46* × *F46_off_books* and *post46* × *F46_on_books* reflect the relative post-FIN 46 change for those two firm categories. Focusing on results for the average implied cost of equity capital (*re_ave*), the *post46* × *F46_off_books* coefficient is not significant, but the *post46* × *F46_on_books* coefficient is positive and highly significant ($p < 0.01$). In untabulated tests the difference in coefficients between *post46* × *F46_on_books* and *post46* × *F46_off_books* is marginally significant ($p < 0.10$, F-test). The results for control variables are similar to those in Panel B of Table 4.[32]

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[32] We also separately examine for each group the residuals (e.g., unexplained cost of equity) remaining after controlling for the Hughes et al. (2009) control variables; both univariate and multivariate analyses of these residuals are consistent with the results reported in Tables 4 and 5.

TABLE 4

Analysis of Implied Cost of Capital Pre- and Post-FIN 46 (H1)

Panel A: Univariate Analysis of the Average Implied Cost of Capital Pre- and Post-FIN 46

Categories	Pre-FIN 46	n	Post-FIN 46	n	Change Pre-Post	Prob (pre < > post)	Prob (> *No_F46_Impact*)
No_F46_Impact	0.100	3,903	0.089	5,109	−0.011	***	
F46	0.097	1,214	0.091	1,493	−0.006	***	***
Total	0.099	5,117	0.090	6,602	−0.009	***	

*** Indicates significance at 1 percent, two-tailed test, correcting for intrafirm correlation with clustered standard errors.

Panel B: Multivariate Analysis of Implied Cost of Capital Pre- and Post-FIN 46 (H1a and H1b)

		Dependent Variables				
	Pred. Sign	*re_ave*	*re_ct*	*re_gls*	*re_peg*	*re_oj*
post46	+/−	−0.007	−0.018	−0.007	0.006	−0.011
		(7.02)***	(15.62)***	(5.80)***	(3.09)***	(6.64)***
F46	+/−	−0.002	−0.001	−0.001	−0.006	−0.000
		(1.23)	(0.43)	(0.49)	(2.08)**	(0.13)
post46 × *F46*	+/−	0.005	0.007	0.006	0.005	0.004
		(2.81)***	(3.33)***	(3.03)***	(1.44)	(1.36)
ubeta	+	−0.003	−0.006	−0.003	0.004	−0.008
		(5.57)***	(10.64)***	(4.35)***	(4.08)***	(9.91)***
sd_ocf	+	0.022	0.025	0.002	0.030	0.029
		(2.50)**	(2.89)***	(0.22)	(1.95)*	(2.67)***
sd_beta	+	0.001	−0.002	0.010	0.019	−0.025
		(0.12)	(0.33)	(1.85)*	(2.32)**	(3.72)***
leverage	+	0.014	0.015	0.016	0.020	0.006
		(10.72)***	(10.67)***	(11.24)***	(7.85)***	(3.32)***
oi_growth	−	−0.002	−0.002	−0.003	−0.000	−0.003
		(4.80)***	(3.80)***	(6.94)***	(0.61)	(4.51)***
size	−	−0.004	−0.003	−0.005	−0.008	−0.002
		(15.00)***	(8.82)***	(15.22)***	(15.02)***	(3.64)***
Constant		0.128	0.111	0.136	0.140	0.125
		(45.57)***	(37.88)***	(41.45)***	(28.57)***	(31.72)***
Observations		11,719	11,719	11,719	11,719	11,719
Adj R^2		0.27	0.30	0.28	0.18	0.09

*, **, *** Indicate significance at 10 percent, 5 percent, and 1 percent levels, respectively.

Robust standard errors adjusted for intrafirm correlation with clustered standard errors are in parentheses.

Panel A presents mean values for average implied cost of equity capital (*re_ave*) during the pre- and post-FIN 46 periods for firms reporting no material FIN 46 effect (*No_F46_Impact*), and for firms subject to FIN 46 (*F46*). The Prob(pre < > post) column indicates whether the change in *re_ave* is significantly different from zero, and the Prob(> *No_F46_Impact*) column indicates whether the change in *re_ave* was significantly greater than the change for *No_F46_Impact* firms (one-tailed test) correcting for intrafirm correlation with clustered standard errors.

(continued on next page)

TABLE 4 (continued)

Panel B presents OLS regressions testing H1 using the following model to compare firms' implied cost of capital between the pre-FIN 46 period (1998 Qtr 3 to 2001 Qtr 2) and the post-FIN 46 period (2003 Qtr 1 to 2005 Qtr 4), excluding observations from 2001 Qtr 3 to 2002 Qtr 4, according to whether the firm had VIEs and complied with FIN 46:

$$re = b_0 + b_1 post46 + b_2 F46 + b_3 post46 \times F46 + b_4 ubeta + b_5 sd\_ocf + b_6 sd\_beta + b_7 leverage + b_8 oi\_growth + b_9 size + \varepsilon.$$

Variable Definitions:
re = indicates one of the following accounting-based measures of implied cost of capital:
re_ave = average of other four measures of implied cost of capital;
re_ct = implied cost of capital based on Claus and Thomas (2001);
re_gls = implied cost of capital based on Gebhardt et al. (2001);
re_peg = implied cost of capital based on the Easton (2004) PEG ratio;
re_oj = implied cost of capital based on Ohlson and Juettner-Nauroth (2005);
re values are winsorized 1 percent at the high end of the distribution to limit the influence of extreme observations;
post46 = 1 if calendar quarter is during 2003 Qtr 1 to 2005 Qtr 4, 0 if calendar quarter is during 1998 Qtr 3 to 2001 Qtr 2;
F46 = 1 if firm reported complying with FIN 46, 0 otherwise;
post46 × *F46* = interaction term with value of 1 if both *post46* and *F46* firms equal 1, 0 otherwise;
ubeta = unlevered beta based on single factor market model;
sd_ocf = standard deviation of operating cash flows over previous five years plus 1, log transformed;
sd_beta = standard deviation of beta values over previous year plus 1, log transformed (proxy for beta volatility as suggested by Hughes et al. [2009]);
leverage = long-term debt divided by market value of equity, winsorized 1 percent at each end of the distribution;
oi_growth = operating income this quarter divided by operating income in previous quarter, winsorized 1 percent at each end of the distribution; and
size = log of market value of equity ($mm) at the end of the previous fiscal year.

Overall, Table 5 provides support for H2. The impact of FIN 46 on firms' cost of equity capital depends on the action that firms took to comply with FIN 46. Firms that reported consolidation of VIEs experienced the largest relative change in implied cost of equity capital post-FIN 46, an increase of approximately 70 basis points for the *re_ave* measure. In summary, the results show that FIN 46 affected *ex ante* measures of cost of equity capital for firms with VIEs impacted by FIN 46 relative to firms with no impact from the standard. Firms that consolidated VIEs (*F46_on_books* firms) following FIN 46 saw the largest relative increase in implied cost of equity capital. These results suggest that by restricting off-balance sheet options, the potential effect of FIN 46 on firms' future cash flows outweighed any benefit from more transparent disclosures.

Supplementary Analysis

In untabulated analysis[33] consistent with the methodology in Table 5, we also considered whether different types of VIEs modify the effects documented here. First, we categorized *F46_on_books* VIEs as described in Panel B of Table 1 and performed univariate analysis of average implied cost of capital (*re_ave*) for *F46_on_books* firms by VIE type, compared to the *No_F46_Impact* and *F46_off_books* firms. Although multiple VIE firms had changes in *re_ave* similar to the *No_F46_Impact* firms, *F46_on_books* firms (categorized by VIE type) generally had significantly smaller decreases than firms not consolidating VIEs under the standard ($p < 0.05$). Next, in untabulated multivariate analysis we further examined the effect of VIE type on *re_ave*.

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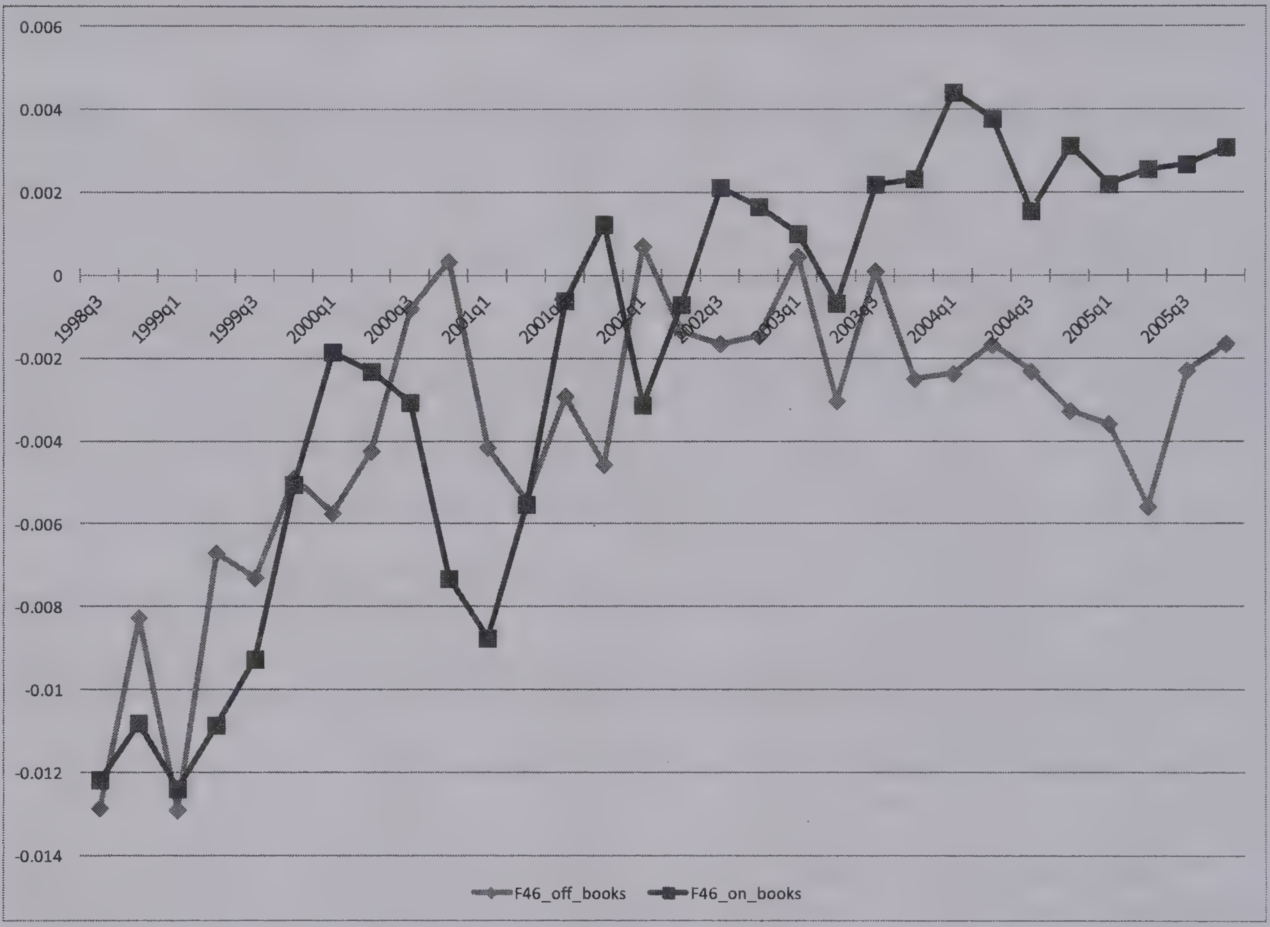
[33] The results discussed in this section are untabulated but are available upon request.

FIGURE 3

Quarterly Average Implied Cost of Capital (Mean-Adjusted) from 1998 to 2005 for FIN 46 Off-Books and On-Books Firms



This chart shows the average, quarterly mean-adjusted, implied cost of equity capital calculated quarterly over the period 1998 (quarter 3) to 2005 (quarter 4) for F46 firms according to the action taken with their VIEs post-FIN 46: keeping off books (*F46_off_books*) or bringing them onto books (*F46_on_books*). Quarterly mean values are based on all firms with available implied cost of equity capital measures for the quarter.

The *post46* interaction coefficients terms for both financial and other VIEs are significantly positive at the 1 percent level (coefficients of 0.013 and 0.025, respectively) and the other VIE interaction coefficient is the largest, but there are few firms in that group. Overall, these results suggest type of VIE does not materially affect the impact of FIN 46 on implied cost of capital.

We also examined an additional control sample of firms unaffected by FIN 46 (e.g., those firms making no disclosures about FIN 46 and reporting no likely SPVs within Exhibit 21). The majority of this sample of 2,296 firms (which we term *No_VIE* firms) did not file an Exhibit 21. These firms serve as an additional benchmark against which to evaluate our *F46* and *No_Impact_F46* groups. Across adoption of FIN 46, we found the decrease in *re_ave* for *No_VIE* firms to be significantly greater than the decrease for both *No_F46_Impact* and *F46* firms ($p < 0.01$, two-tailed test), and the decrease for *No_F46_Impact* firms was also significantly greater than that for *F46* firms ($p < 0.01$, two-tailed test). Untabulated multivariate analysis including this group yields stronger, but consistent results compared to those reported here.

Considering Botosan and Plumlee (2005), we also examined a cost of equity estimate based on Value Line forecasts ($r_{DIVPREM}$). This untabulated analysis finds very little difference across firm

TABLE 5

Analysis of Implied Cost of Capital Pre- and Post-FIN 46 According to the Action Taken with Reported VIEs (H2)

Panel A: Univariate Analysis of the Average Implied Cost of Capital Pre- and Post-FIN 46

Categories	Pre-FIN 46	n	Post-FIN 46	n	Change Pre-Post	Prob (Pre < > Post)	Prob (> *No_F46_Impact*)	Prob (> *F46_off_books*)
No_F46_Impact	0.100	3,903	0.089	5,109	−0.011	***		
F46_off_books	0.097	489	0.088	577	−0.009	***	ns	
F46_on_books	0.096	725	0.093	916	−0.003	ns	***	**
Total	0.099	5,117	0.090	6,602	−0.009	***		

, * Indicate significance at 5 percent and 1 percent levels, respectively.
ns indicates not significant.

Panel B: Multivariate Analysis of Implied Cost of Capital Pre- and Post-FIN 46

		Dependent Variables				
	Pred. Sign	*re_ave*	*re_ct*	*re_gls*	*re_peg*	*re_oj*
post46	+/−	−0.007	−0.018	−0.007	0.006	−0.011
		(7.02)***	(15.64)***	(5.79)***	(3.08)***	(6.64)***
F46_off_books	+/−	−0.001	−0.001	−0.003	−0.001	0.000
		(0.42)	(0.47)	(1.03)	(0.17)	(0.13)
F46_on_books	+/−	−0.003	−0.000	0.000	−0.010	−0.001
		(1.38)	(0.16)	(0.10)	(2.86)***	(0.22)
post46 × *F46_off_books*	+/−	0.002	0.004	0.006	−0.001	−0.000
		(0.84)	(1.24)	(2.03)**	(0.22)	(0.01)
post46 × *F46_on_books*	+/−	0.007	0.008	0.006	0.008	0.007
		(3.04)***	(3.59)***	(2.51)**	(2.11)**	(1.67)*
ubeta	+	−0.003	−0.006	−0.003	0.005	−0.008
		(5.53)***	(10.59)***	(4.34)***	(4.09)***	(9.86)***
sd_ocf	+	0.021	0.024	0.001	0.030	0.029
		(2.44)**	(2.81)***	(0.17)	(1.95)*	(2.61)***
sd_beta	+	0.001	−0.002	0.010	0.019	−0.025
		(0.14)	(0.31)	(1.87)*	(2.30)**	(3.70)***
leverage	+	0.014	0.015	0.016	0.020	0.006
		(10.56)***	(10.53)***	(11.15)***	(7.79)***	(3.21)***
oi_growth	−	−0.002	−0.002	−0.003	−0.000	−0.003
		(4.83)***	(3.86)***	(6.99)***	(0.58)	(4.53)***
size		−0.005	−0.003	−0.006	−0.008	−0.002
		(14.94)***	(8.86)***	(15.24)***	(14.86)***	(3.71)***
Constant		0.128	0.112	0.136	0.139	0.125
		(45.39)***	(37.97)***	(41.58)***	(28.32)***	(31.65)***
Observations		11,719	11,719	11,719	11,719	11,719
Adj R^2		0.27	0.31	0.28	0.18	0.09

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*, **, *** Indicates significance at 10 percent, 5 percent, and 1 percent levels, respectively.
Robust standard errors adjusted for intrafirm correlation with clustered standard errors are in parentheses.

(continued on next page)

TABLE 5 (continued)

Panel A presents mean values for average implied cost of equity capital (*re_ave*) during the pre- and post-FIN 46 periods for firms reporting no material FIN 46 effect (*No_F46_Impact*) and for firms subject to FIN 46 (*F46* firms) subdivided according to whether the firms kept the VIEs off their books (*F46_off_books*) or brought the VIEs onto their books (*F46_on_books*). The Prob(pre < > post) column indicates whether the change in *re_ave* is significantly different from zero, the Prob(> *No_F46_Impact*) column indicates whether the change in *re_ave* was significantly greater than the change for *No_F46_Impact* firms (one-tailed test), and Prob(> *F46_off_books*) column indicates whether the change in *re_ave* for *F46_on_books* firms is significantly greater than the change for *F46_off_books* firms (one-tailed tests correcting for intrafirm correlation with clustered standard errors).

Panel B presents OLS regressions testing H2 using the following model to compare firms' implied cost of capital between the pre-FIN 46 period (1998 Qtr 3 to 2001 Qtr 2) and the post-FIN 46 period (2003 Qtr 1 to 2005 Qtr 4), excluding observations from 2001 Qtr 3 to 2002 Qtr 4, according to the action taken by the firm with respect to the reported VIEs:

$$\begin{aligned} re = {} & b_0 + b_1 post46 + b_2 F46\_off\_books + b_3 F46\_on\_books + b_4 post46 \times F46\_off\_books \\ & + b_5 post46 \times F46\_on\_books + b_6 ubeta + b_7 sd\_ocf + b_8 sd\_beta + b_9 leverage + b_{10} oi\_growth + b_{11} size + \varepsilon. \end{aligned}$$

Variable Definitions:
re = indicates one of the following three accounting-based measures of implied cost of capital:
re_ave = average of other four measures of implied cost of capital;
re_ct = implied cost of capital based on Claus and Thomas (2001);
re_gls = implied cost of capital based on Gebhardt et al. (2001);
re_peg = implied cost of capital based on the Easton (2004) PEG ratio;
re_oj = implied cost of capital based on Ohlson and Juettner-Nauroth (2005);
re values are winsorized 1 percent at the high end of the distribution to limit the influence of extreme observations;
post46 = 1 if calendar quarter is during 2003 Qtr 1 to 2005 Qtr 4, 0 if calendar quarter is during 1998 Qtr 3 to 2001 Qtr 2;
F46_off_books = 1 if firm reported complying with FIN 46 but kept VIEs off their books, 0 otherwise;
F46_on_books = 1 if firms reported complying with FIN 46 and consolidated or otherwise brought the VIEs onto their books, 0 otherwise;
post46 × *F46_off_books* = interaction term;
post46 × *F46_on_books* = interaction term;
ubeta = unlevered beta based on single factor market model, winsorized 1 percent at each end of the distribution;
sd_ocf = standard deviation of operating cash flows over previous five years, log transformed;
sd_beta = standard deviation of beta values over previous year (proxy for beta volatility as suggested by results of Hughes et al. [2009]);
leverage = long-term debt divided by market value of equity, winsorized 1 percent at each end of the distribution;
oi_growth = operating income this quarter divided by operating income in previous quarter, winsorized 1 percent at each end of the distribution; and
size = log of market value of equity ($ mm) at the end of the previous fiscal year.

type. However, Value Line documentation indicates that Value Line forecasts after 2003 are largely based on pre-FIN 46 data and, therefore, the $r_{DIVPREM}$ measure would reflect only changes due to FIN 46, if any, with substantial delay.[34]

Additionally, we considered whether the impact of FIN 46 on debt ratios affected firms' cost of equity capital. We formed three portfolios for *F46* firms based on debt ratios prior to FIN 46 and additional liabilities recorded due to consolidating VIEs. In untabulated results we found that firms with high debt ratios and the highest increase in liabilities from VIEs experienced a 220 basis point increase in average cost of equity capital (*re_ave*). This change was significantly greater than for firms in the other two categories, both of which experienced changes similar to the *No_F46_Impact* firms. Firms in the high debt category also had the lowest relative cost of equity capital pre-FIN 46.

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[34] Specifically, the documentation says, "The 3–5 year average price is determined by applying a 'normalized' P/E ratio to projected 3–5 year earnings per share." Value Line estimates their EPS growth from the recent average growth rate ("The annual compounded growth rate using the average of the three latest base years.").

Finally, consistent with previously discussed theory and evidence, we also examined changes in cost of debt surrounding adoption of FIN 46. In untabulated multivariate analysis of credit ratings using Compustat S&P ratings and controls previously established in the literature (e.g., Jorion et al. 2005; Jiang 2008), we find long-term issuer credit ratings did not significantly change after FIN 46, although we observe more downgrades for firms associated with VIEs.[35] Further, the results suggest *F46_on_books* firms had better credit ratings prior to FIN 46 than did *No_F46_Impact* and *F46_off_books* firms. Credit ratings in general deteriorated after FIN 46, significantly so for the *No_F46_Impact* and *F46_on_books* firms; however, in the multivariate analysis, we did not observe significant increases in cost of debt for either *F46_off_books* or *F46_on_books* firms.

VI. CONCLUSIONS

This study explores whether adoption of FASB Interpretation No. 46/R (FIN 46), *Consolidation of Variable Interest Entities—An Interpretation of ARB No. 51*, in 2003 changes the cost of equity capital for affected firms. Using comparative analysis on a broad sample of 11,719 firm-quarter observations for 1,389 firms during the period 1998 to 2005, we find evidence that FIN 46 significantly increased the cost of equity capital for firms with affected VIEs by an average of 50 basis points. Further, firms that consolidated these formerly off-balance sheet structures experienced the largest increase. Additional analysis finds that the type of VIE does not materially affect the impact of FIN 46 on implied cost of equity capital. Further, the cost of debt as proxied by debt credit ratings did not significantly change after FIN 46, although debt downgrades increased.

Consistent with warnings from some financial analysts that the changes initiated by FIN 46 could adversely affect loan covenants, credit ratings, and regulatory capital requirements, our results indicate firms with VIE structures were, in general, negatively affected by FIN 46. Taken together, these results suggest FIN 46 reduced the opportunity for firms to use off-balance sheet structures to reduce their cost of equity capital. Our findings imply the FASB's stated purpose in enacting FIN 46 was at least partially satisfied.

This study is subject to limitations. We tested the cost of capital implications of FIN 46 using a variety of *ex ante* measures of cost of equity capital, all widely used in prior research. We compared values for individual, as well as the average, *ex ante* measures of implied cost of equity capital for a three-year period pre-FIN 46 against a similar period post-FIN 46 for firms materially impacted by FIN 46 against those that were not. We recognize the ongoing debate about the validity of implied cost of equity capital measures, but our use of these measures is consistent with current academic literature and appropriate for understanding the impact of FIN 46, given the theoretical asset-pricing literature to date.

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[35] *F46_on_books* firms are likely to experience more downgrades than the *No_VIE* control firms in both the pre- and post-FIN 46 periods ($p < 0.01$, ordered probit with clustered standard errors). *F46_off_books* firms are likely to experience more downgrades in the post-FIN 46 period than the *No_VIE* control firms. Both *F46_on_books* and *F46_off_books* categories are likely to experience more downgrades post-FIN 46 than pre-FIN 46 ($p < 0.10$, ordered probit with clustered standard errors).

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APPENDIX A

Exhibit 1: On-Books Financial VIE—Black Hills Corp (12/31/2003)

FIN 46-R

In January 2003, the FASB issued Interpretation No. 46, *Consolidation of Variable Interest Entities* (FIN 46). In December 2003, the FASB issued FIN No. 46 (Revised) (FIN 46-R) to address certain FIN 46 implementation issues. The Company's subsidiary, Black Hills Wyoming, has an agreement with Wygen Funding, Limited Partnership, an unrelated variable interest entity (VIE) to lease the Wygen plant. Under the new accounting interpretation, as amended, the Company consolidated the VIE effective December 31, 2003. The effect of consolidating the VIE into the Company's Consolidated Balance Sheet was an increase in total assets of $129.0 million, of which $121.5 million, net of accumulated depreciation of $3.0 million, is included in Property, Plant, and Equipment and an increase in long-term debt in the amount of $128.3 million. Prior to the December 31, 2003 consolidation, the Company recorded lease expense on the Wygen plant. Lease payments began upon completion of the plant in February 2003. During 2003, lease payments were $2.7 million and are included in Operations and Maintenance on the accompanying 2003 Consolidated Statement of Income. The net effect of consolidating the income statement of the VIE on December 31, 2003 was to recognize a cumulative effect charge for $2.5 million (net of $1.4 million of income taxes), which represents the depreciation and interest expense that would have been recorded had the VIE been consolidated at inception. The net effect on future results will be to recognize depreciation and interest expense in place of recognizing lease expense, which is estimated to have an approximate $3.0 million negative annual effect to after-tax net income.

Exhibit 2: On-Books Operational VIE—Cisco Systems, Inc. (07/31/2004)

Acquisition of Andiamo Systems, Inc.

In April 2001, the Company entered into a commitment to provide convertible debt funding of approximately $84 million to Andiamo, a privately held storage switch developer. This debt was convertible into approximately 44 percent of the equity of Andiamo. In connection with this investment, the Company obtained a call option that provided the Company the right to purchase Andiamo. The purchase price under the call option was based on a valuation of Andiamo using a negotiated formula. On August 19, 2002, the Company entered into a definitive agreement to acquire Andiamo, which represented the exercise of its rights under the call option. The Company also entered into a commitment to provide nonconvertible debt funding to Andiamo of approximately $100 million through the close of the acquisition. Substantially all of the convertible debt funding of $84 million and nonconvertible debt funding of $100 million has been expensed as research and development costs.

The Company adopted FIN 46(R) effective January 24, 2004. The Company evaluated its debt investment in Andiamo and determined that Andiamo was a variable interest entity under FIN 46(R). The Company concluded that the Company was the primary beneficiary as defined by FIN 46(R) and, therefore, accounted for Andiamo as if the Company had consolidated Andiamo since the Company's initial investment in April 2001. The consolidation of Andiamo from the date of the Company's initial investment required accounting for the call option as a repurchase right. Under FASB Interpretation No. 44, *Accounting for Certain Transactions Involving Stock Compensation*, and related interpretations, variable accounting was required for substantially all Andiamo employee stock and options because the ending purchase price was primarily derived from a revenue-based formula.

Effective January 24, 2004, the last day of the second quarter of fiscal 2004, the Company recorded a noncash cumulative stock compensation charge of $567 million, net of tax (representing the amount of variable compensation from April 2001 through January 2004). This charge was reported as a separate line item in the Consolidated Statements of Operations as a cumulative effect of accounting change, net of tax. The charge was based on the value of the Andiamo employee stock and options and their vesting from the adoption of FIN 46(R) pursuant to the formula-based valuation.

On February 19, 2004, the Company completed the acquisition of Andiamo, exchanging approximately 23 million shares of the Company's common stock for Andiamo shares not owned by the Company and assuming approximately 6 million stock options, for a total estimated value of $750 million, primarily derived from the revenue-based formula, which after stock-price-related adjustments resulted in a total amount recorded of $722 million, as summarized in the table below.

Subsequent to the adoption of FIN 46(R), changes to the value of Andiamo and the continued vesting of the employee stock and options resulted in an adjustment to the noncash stock compensation charge. The Company recorded a noncash variable stock compensation adjustment of $58 million in the third quarter of fiscal 2004 to the cumulative stock compensation charge recorded in the second quarter of fiscal 2004 to account for the additional vesting of the Andiamo employee stock and options and changes in the formula-based valuation from January 24, 2004 until February 19, 2004. This noncash adjustment was reported as operating expense in the Consolidated Statements of Operations, as amortization of deferred stock-based compensation in the Consolidated Statements of Cash Flows, and as an increase to additional paid-in capital in the Consolidated Statements of Shareholders' Equity. In addition, upon completion of the acquisition, deferred stock-based compensation of $90 million was recorded in the Consolidated Balance Sheets to reflect the unvested portion of the formula-based valuation of the Andiamo employee stock and options. The amount of deferred stock-based compensation was fixed at the date of acquisition and will be amortized over the remaining vesting period of Andiamo employee stock and options of approximately two years.

A summary of the accounting of the consolidation under FIN 46(R) and the subsequent purchase of Andiamo, after stock price related adjustments, is as follows (in millions):

	Amount
Cumulative effect of accounting change, net of tax benefit of	$567
Variable stock-based compensation	58
Deferred stock-based compensation	90
Net assets	7
Total	$722

Exhibit 3: Both On-Books and Off-Books Other VIE—Aetna, Inc. (12/31/2003)

Accounting for Variable Interest Entities ("VIE")

In January 2003, the FASB issued Interpretation No. 46, *Consolidation of Variable Interest Entities* (FIN 46). This interpretation requires the Company to consolidate a VIE if the entity meets certain criteria and the Company is considered the primary beneficiary of the VIE (such as where the Company has a direct or indirect ability to make significant decisions for the entity or the obligation to absorb a majority of the entity's expected losses or gains). FIN 46 also requires additional disclosure of the Company's relationship with a VIE regardless of whether the Company is the primary beneficiary.

In December 2003, the FASB issued a revision to Interpretation No. 46, *Consolidation of Variable Interest Entities* (FIN 46-R). FIN 46-R amended certain provisions of FIN 46 and delayed implementation for entities that are not considered special purpose entities until the first quarter of 2004. As of December 31, 2003, the Company has no investments that are considered special purpose entities.

The Company does not have any material relationships with VIEs that would require consolidation. The Company does have relationships with certain real estate partnerships that are considered VIEs. However, the Company would not be considered the primary beneficiary. The Company records the amount of its investment in these partnerships as an asset on its Consolidated Balance Sheet and recognizes its share of partnership income or losses in earnings. The Company's maximum exposure to loss as a result of its investment in these partnerships is its investment balance at December 31, 2003 of approximately $125 million and the risk of recapture of tax credits previously recognized, which the Company does not believe is significant. The partnerships construct, own, and manage low-income housing developments and had total assets of approximately $1.3 billion as of December 31, 2003.

Effective September 15, 2003, the Company's leasing program with an independent third party grantor trust (primarily for the lease of a corporate aircraft and certain office furniture), accounted for as an operating lease under previous accounting standards, was terminated, and the Company purchased those assets for cash. The Company would have been required to consolidate this grantor trust in accordance with the requirements of FIN 46.

APPENDIX B

Easton (2004) *re_peg* Measure

Easton (2004) develops a proxy for the cost of equity capital implied by the price-earnings-growth ratio (*re_peg*):

$$re\_peg = sqrt((eps_2 - eps_1)/price_0). \tag{1}$$

In Equation (1) eps_1 and eps_2 are the last mean earnings per share forecasts in the calendar quarter for fiscal years $t+1$ and $t+2$, respectively, and $price_0$ equals the market price at the end of the calendar quarter. The *re_peg* measure requires earnings growth to be positive. For the *re_peg* measure, we follow Francis et al. (2005a) and Botosan and Plumlee (2005) in assuming zero dividends per share.[36]

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[36] We also calculated a version of the *re_peg* measure including dividends and found similar results in all analyses.

Ohlson and Juettner-Nauroth (2005) *re_oj* Measure

Our second proxy for the cost of equity capital was developed by Ohlson and Juettner-Nauroth (2005) and examined by Gode and Mohanram (2003):

$$re\_oj = A + sqrt(A^2 + (eps_1/price_0)(g_2 - (\gamma - 1)). \tag{2}$$

Again, eps_1 equals analyst forecasts of earnings per share for the following fiscal year, $price_0$ is the final price for the current calendar quarter, g_2 is the earnings growth percentage $((eps_2 - eps_1)/eps_1)$, and $\gamma{-}1$ is long-term growth rate set to the risk-free rate less 3 percent (Hribar and Jenkins 2004), and A equals:

$$A = \left(\frac{1}{2}\right)((\gamma - 1) + dps_1/price_0), \tag{3}$$

where dps_1 is the dividends per share for the following year assumed to continue at the same level as in the current year. We use ten-year U.S. Treasury bond rates as the risk-free rate (e.g., Hribar and Jenkins 2004; Dhaliwal et al. 2005). The long-term growth rate, $\gamma{-}1$, sets a lower bound on the *re_oj* measure.

Gebhardt et al. (2001) *re_gls* Measure

Our third cost of equity capital measure is based on Gebhardt et al. (2001). They use a version of the residual income valuation model (Ohlson 1995; Feltham and Ohlson 1995) to derive their proxy for the cost of equity capital:

$$P_t = B_t + \frac{FROE_{t+1} - r_e}{(1 + r_e)} B_t + \frac{FROE_{t+2} - r_e}{(1 + r_e)^2} B_{t+1} + TV, \tag{4}$$

where P_t equals the market price at the end of calendar quarter, B_t equals the book value per share at the beginning of the quarter, r_e equals the cost of equity capital, *FROE* equals the forecasted return on equity (forecasted eps_{t+i}/B_{t+i-1} for the $t{+}i$ period), and *TV* is a terminal value estimate calculated as follows:

$$TV = \sum_{i=3}^{T-1} \frac{FROE_{t+i} - r_e}{(1 + r_e)^i} B_{t+i-1} + \frac{FROE_{t+T} - r_e}{(1 + r_e)^{T-1}} B_{t+T-1}. \tag{5}$$

In Equations (4) and (5), we calculate $FROE_{t+3}$ by multiplying forecast eps_{t+2} by 1 plus the I/B/E/S long-term growth rate forecast.[37] We set $T = 12$ in Equation (5) and calculated the terminal value assuming that *ROE* linearly reverts to the industry median *ROE* over periods 4 to 12.[38] We calculate the industry median ROE using ten-year averages for Fama and French's (1997) 48 industry classifications, excluding loss firms from the calculation (Dhaliwal et al. 2005).[39] Assuming clean surplus accounting, we calculate $B_{t+1} = B_{t+1-1} + (1 - payout\ ratio) \times eps_{t+1}$, where *payout ratio* equals current dividends per share divided by current earnings per share.[40] In cases where the earnings per share are negative, we calculate the payout ratio as current dividends divided by 6 percent of total assets (Hribar and Jenkins 2004; Dhaliwal et al. 2005, 2007).[41]

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[37] We constrained the maximum long-term growth rate to less than or equal to 50 percent, since long-term growth rates exceeding 50 percent are highly unlikely.

[38] We winsorized future *ROE* values at 1 percent on the high end of the distribution to limit the influence of untenable rates for forecast return on equity and also constrained *ROE* to be greater than zero.

[39] We also estimated *re_gls* values without removing loss firms from the calculation. The implied cost of equity capital measures was lower on average but the overall results were unaffected.

[40] We constrained the payout ratio to less than or equal to 1.

Claus and Thomas (2001) *re_ct* Measure

Claus and Thomas (2001) base their cost of equity capital measure on an alternate form of the residual income valuation model. In particular, they assume that abnormal earnings grow at a constant rate beyond the fifth year, in contrast to reverting to the industry median for the *re_gls* measure:

$$P_t = B_t + \frac{AE_{t+1}}{(1+r_e)} + \frac{AE_{t+2}}{(1+r_e)^2} + \frac{AE_{t+3}}{(1+r_e)^3} + \frac{AE_{t+4}}{(1+r_e)^4} + \frac{AE_{t+5}}{(1+r_e)^5} + \frac{AE_{t+5}(1+g_{ae})}{(r_e - g_{ae})(1+r_e)^5}, \tag{7}$$

where AE_{t+1} equals expected abnormal earnings for year $t+i$ calculated as forecasted $eps_{t+1} - r_e(B_{t+i-1})$, and g_{ae} is the growth rate on abnormal earnings equal to the yield on ten-year Treasury bonds (Hribar and Jenkins 2004; Dhaliwal et al. 2005, 2007). We estimate AE_{t+3} to AE_{t+5} by multiplying the prior-year forecast by the long-term growth forecast.[42] Again, we estimate B_{t+i} assuming clean surplus accounting and constant payout ratios as with the *re_gls* measure.[43]

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[41] We calculated the implied cost of capital iteratively beginning with a value for *re* of 0.0001 and incrementing that value by 0.0001 until the calculated price is within 1 percent of the actual price. See Guay et al. (2005) for issues with respect to selecting the appropriate market price and adjusting the price for changes in shares outstanding.

[42] Again, where an I/B/E/S long-term growth rate was not available, we used $(eps_2 - eps_1)/eps_1$, and we constrained the maximum long-term growth rate to less than or equal to 50 percent, since long-term growth rates exceeding 50 percent are highly unlikely. I/B/E/S long-term growth rate estimates are not available for approximately 10 percent of the sample.

[43] For both the *re_gls* and *re_ct* measures, we calculate the implied cost of capital iteratively beginning with a value of 0.0001 and incrementing that value by 0.0001 until the calculated price is within 1 percent of the actual price. For the *re_ct* measure, the long-term growth in abnormal earnings, g_{ae}, presents a lower bound on the cost of equity capital.

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Lost in Translation: The Effects of Incentive Compensation on Strategy Surrogation

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ABSTRACT: To facilitate managers' decision-making, firms develop strategic performance measurement systems that translate strategy into performance measures. Ideally, managers see measures for what they are—imperfect proxies for intangible strategic constructs. However, managers may fail to fully appreciate the fact that measures are merely representations of the strategic constructs, and act as though the measures are the construct of interest—a phenomenon we label *surrogation*. In this paper, we investigate whether and how the use of strategically linked performance measures for compensation purposes affects managers' propensity to exhibit surrogation. In accordance with the attribute substitution framework (Kahneman and Frederick 2002), we predict incentive compensation exacerbates surrogation, and that this effect is more prevalent when managers are compensated on a single measure of a strategic construct than when managers are compensated on multiple measures of a strategic construct. Via two experiments, we find support for these hypotheses. Our paper contributes to the literature on strategic performance measurement systems by highlighting the tendency of managers to use measures as surrogates for strategy. More

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generally, we identify a by-product of contracting on imperfect performance measures not previously considered in extant literature, and establish when consideration of costs of this by-product are likely to be critical.

Keywords: *strategy surrogation; strategic performance measurement systems; incentive compensation; attribute substitution.*

I. INTRODUCTION

Strategic performance measurement systems facilitate managers' decision-making by translating strategy into performance measures. A critical feature of these measurement systems is that the strategically linked performance measures be transparent (Kaplan and Norton 1996), allowing managers to "see through" the measures back to the strategy. This transparency allows managers to infer the firm's desired course of action, gauge the appropriateness of the strategy, and adjust the strategy as deemed necessary. Ideally, managers see measures for what they are—imperfect proxies for intangible, over-arching strategic constructs. However, managers may fail to fully appreciate the fact that measures are merely representations of the true constructs of interest, ultimately acting as if the imperfect measures *are* the constructs of interest. This behavior—which we label *surrogation*—potentially hinders managers' ability to make appropriate strategy-related judgments and decisions. The purpose of this paper is to investigate managers' propensity to exhibit surrogation when using a strategic performance measurement system.

Surrogation occurs in a wide variety of circumstances. For example, university administrators accustomed to focusing on teaching evaluations during faculty reviews may lose sight of the strategic construct of "teaching effectiveness" and behave as though the evaluations are the construct of interest (Brickley and Zimmerman 2001; Flinn and Crumbley 2009). Similarly, a manager charged with "delighting the customer" when customer satisfaction surveys gauge strategic success may begin to see maximizing survey results as the strategy, and behave accordingly (Grizzle 2002). In a market setting, investors may use a firm's financial statements repeatedly, and in the process lose sight of the construct(s) of interest that these statements represent (Ijiri 1967, 1975).

Under some circumstances, the phenomenon of surrogation is likely to be less important. For instance, losing sight of strategic constructs that underlie performance measures may be acceptable for low-level employees who do not make strategic judgments and decisions. Additionally, the phenomenon of surrogation is moot when contracts are complete or when measures perfectly capture underlying constructs. However, complete contracts are rarely seen in practice (Williamson 1985), and firms often make trade-offs between contract *completeness* and contract *complexity*. This occurs because managers often face settings in which constructs are intangible, ill-defined, and complex, necessitating the use of imperfect measures of the constructs as part of a firm's strategic performance measurement system.

Many factors are likely to influence managers' propensity toward surrogation. This study focuses specifically on the use of measures as a basis of compensation. Specifically, we ask whether tying incentive compensation to one or more performance measures affects managers' propensity to use measures as surrogates for strategic constructs.

We rely on literature in psychology to develop our theory. In particular, we leverage Kahneman and Frederick's (2002) notion of attribute substitution, which occurs when an individual has a judgment to make regarding a complex "target attribute," and instead relies on a more easily accessible "heuristic attribute" in their judgment process. Using this framework, we develop two hypotheses regarding the influence of incentive compensation on a manager's surrogation of strategy. Our first hypothesis predicts that basing a portion of a manager's compensation on a single measure of a strategic construct exacerbates surrogation of that construct, relative to when the manager receives a fixed wage. Specifically, we argue that the incentive compensation directs a manager's focus toward the compensated measure as a means of achieving higher compensation. Such attention-directing effects increase the accessibility of the compensated measure as a potential substitute for the construct of interest, increasing the likelihood that surrogation occurs, relative to when fixed-wage-compensation is used. Our second hypothesis predicts that incentive compensation based on multiple measures of a single strategic construct leads to less surrogation of that construct relative to when compensation is based on a single measure of that construct. In particular, we posit that managers are more likely to consider any single measure's capacity to represent the strategic construct when compensated on multiple measures of a strategic construct rather than on a single measure of that construct. Such consideration highlights the limitations of any one measure as a representation of the strategic construct, and according to attribute substitution theory, decreases the likelihood that managers use imperfect proxies as surrogates for the higher-level construct.

Investigating the effect of compensation on surrogation using archival or field data is challenging because of the difficulty of distinguishing surrogation from wealth-maximizing behavior. For example, a salesperson whose compensation is based on customer satisfaction survey scores may focus on maximizing customer satisfaction surveys both because the salesperson responds to incentives to maximize wealth *and* also because of surrogation whereby they view survey results as the strategic construct of interest. Determining where wealth maximization ends and where surrogation begins in such field settings is generally not possible. Therefore, we use an experiment to distinguish between surrogation and wealth maximization. To strengthen the test of our theory and hypotheses, we investigate our research question in a setting that, by necessity, does not map perfectly into the real-world settings in which we are most interested. This approach is similar to prior experimental work in accounting (Libby and Tan 1999; Tayler and Bloomfield 2011). Specifically, participants in our main experiment earn incentive compensation in one task, then move to a separate, similar task where incentive compensation is absent, allowing us to measure surrogation in isolation. In a follow-up experiment, we again induce surrogation via compensation manipulations in one task and measure surrogation in a separate task. However, in the second experiment we also provide compensation in the second task to help address alternative explanations for our results.

In the first task in our experiments, graduate business student participants in our study use the computer game *Spore* (Electronic Arts 2008; Lawton 2008) to design a virtual creature following a specified strategy. We provide four measures of a single strategic construct to participants in real-time as they design their creatures. We manipulate compensation tied to these measures at three levels: fixed-wage-compensation, incentive compensation based on a *single* measure of the creature's abilities, and incentive compensation based on *multiple* (three of the four) measures of the creature's abilities. After designing their creature, participants move to the next task where we measure their tendency to use measures as surrogates for the strategic construct. Each participant views a series of 18 virtual creatures and makes choices regarding the design of these creatures. We designed these choices to allow us to evaluate the extent to which incentive compensation influences participants' tendency to use compensated measures in the first task as surrogates for the strategy in the second task. Importantly, our main experiment does not tie these 18 design choices to compensation in any way, allowing us to view behavior absent the influence of incentive compensation. Based on these choices, we compute participants' "surrogation score," our key dependent variable.

We find evidence consistent with our hypotheses across both of our experiments. Participants compensated on a single measure of a strategic construct were more likely to exhibit strategy surrogation than participants who received a fixed wage. We observe this result even when participants had to incur an opportunity cost to make a choice reflecting surrogation. Further, we find evidence that participants compensated on multiple measures of a strategic construct were less likely to exhibit surrogation than participants compensated on a single measure of a strategic construct.

Our study contributes to the literature on strategic performance measurement systems by highlighting the tendency of managers to use measures as surrogates for strategy. While prior research identifies the potential for such behavior to arise (Ijiri 1967, 1975), little evidence exists to help explain *why* managers use performance measures as surrogates for strategic constructs. Not only do we provide a theory-based explanation for such behavior, but also we document how an important factor—the number of measures on which managers are compensated—affects surrogation. We also contribute to literature relating to the adage "you get what you measure" (Kerr 1975; Goldratt 2004) and the concept of "performance measure myopia" (Lambert 2001, 38), providing evidence relating to why individuals may engage in such behavior even when they have no direct financial incentive to do so. More generally, we identify a by-product of contracting on imperfect performance measures not previously considered in extant literature (Bushman and Indjejikian 1993; Feltham and Xie 1994; Budde 2007), and establish when consideration of costs of this by-product are likely to be critical.

The remainder of this paper proceeds as follows. Section II provides the background and hypotheses. Section III describes our experiment and related procedures. Section IV presents results. Section V concludes.

II. BACKGROUND AND HYPOTHESES

Strategic Performance Measurement Systems

A fundamental purpose of accounting is to provide a means by which decision-makers can grapple with abstract and complex constructs of interest (Ijiri 1967, 1975). Ideally, decision-makers could work directly with these constructs. However, given the nature of these constructs, accounting measures are useful media through which managers make decisions related to the constructs.

This fundamental purpose of accounting is embodied in the context of firms' strategic performance measurement systems. Prior research uses various labels for strategic performance measurement systems, including performance measurement models (Malina et al. 2007) and balanced scorecards (Kaplan and Norton 1992). In this paper, we refer to any system with an explicit link between strategy and performance measurement as a strategic performance measurement system (Epstein and Manzoni 1997; Webb 2004). A key feature of these systems is the mapping of relationships among strategic constructs and performance measures. These outlines are commonly referred to as strategy maps (Kaplan and Norton 2000), value driver maps (Ittner and Larcker 2003), or causal chains of performance (Tayler 2010) (see Figure 1).[1]

Prior research highlights the importance of linking firm strategy and performance measures, noting at least three key functions of such alignment (Langfield-Smith 1997; Ittner and Larcker 1998, 2001; Chenhall 2003). First, translating strategy into a set of measures facilitates *communication* of strategy throughout the firm (Malina and Selto 2001, 2004). That is, performance measures signal to managers potential actions to take, and inform them of how their actions affect others within the firm, as well as the firm as a whole. Second, aligning strategy with performance measures enables *evaluation* of strategy. Performance measures provide tangible evidence that managers use to determine the extent to which the firm is accomplishing its strategy (Bromwich 1990), and to test and validate the strategy (Campbell et al. 2008). Third, tying measures to strategy facilitates the *development* of new strategies (Simons 1991, 1994). Performance measure information can help managers to discover new strategic constructs and/or new strategies. Key to these functions is the transparency of measures, which allows managers to "see through" the measures back to the strategy (Kaplan and Norton 1996).

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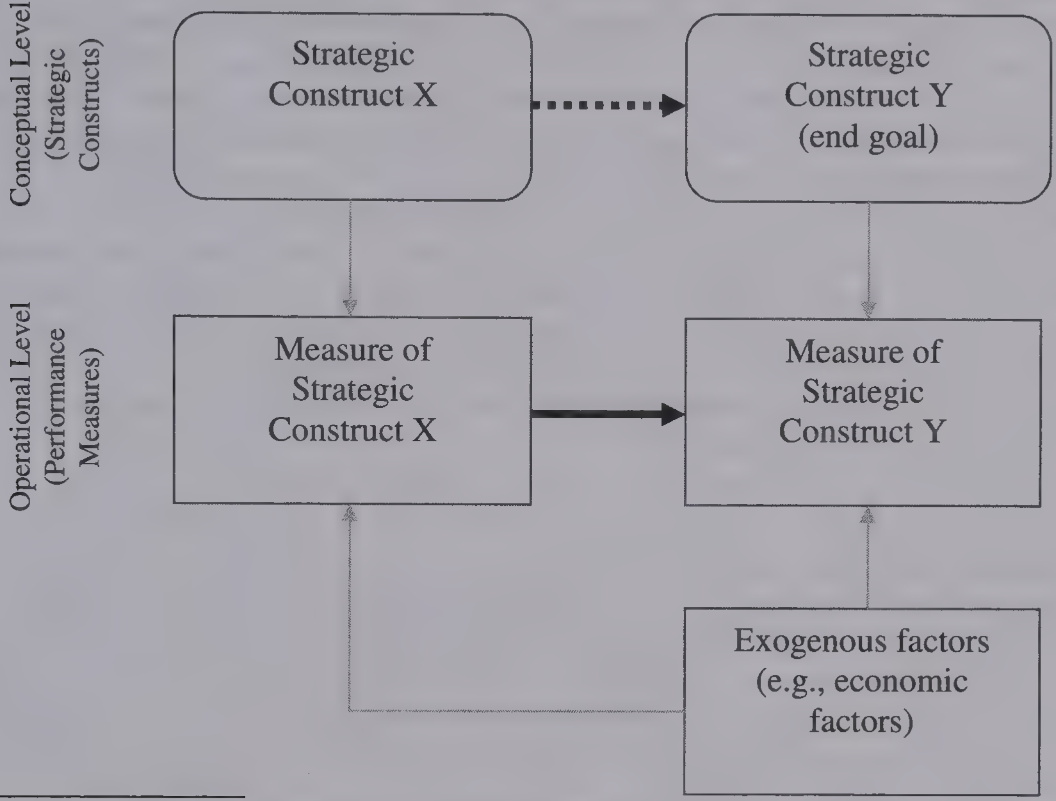
[1] Strategic constructs, which define the purpose of actions taken, are often referred to as strategic objectives (Kaplan and Norton 1996). A collection of causally connected strategic constructs define the strategy for an organization.

FIGURE 1
Linking Strategy to Measures of Performance


Conceptual Level (Strategic Constructs)
Operational Level (Performance Measures)
Strategic Construct X
Strategic Construct Y (end goal)
Measure of Strategic Construct X
Measure of Strategic Construct Y
Exogenous factors (e.g., economic factors)

Strategic performance measurement systems facilitate managers' actions by mapping relationships among strategic constructs and performance measures. Above is a simplified example of this mapping based, in part, on the predictive validity framework (Runkel and McGrath 1972; Libby 1981). Horizontal linkages at the *Conceptual Level* represent the hypothesized cause-and-effect relationships between strategic constructs (also called strategic objectives [Kaplan and Norton 1996]). These linkages form "strategy maps" (Kaplan and Norton 2000), also called "value driver maps" (Ittner and Larcker 2003) or "causal chains of performance" (Tayler 2010). Downward links between *Conceptual Level* strategic constructs and *Operational Level* measures reflect the role of measures as imperfect representations of higher-level strategic constructs. Horizontal linkages at the *Operational Level* represent testable relationships between measures (based on the hypothesized relationships of the associated strategic constructs at the *Conceptual Level*). Figure 1 also captures the potential effect of exogenous factors, such as economic conditions, that influence the realization of performance measures.

Surrogation

Notwithstanding the many benefits of aligning a firm's strategic constructs with performance measures, this process can also lead managers to focus on performance measures to the point that they lose sight of the strategic constructs that the measures represent. Although intended as proxies for higher-level constructs, Ijiri (1967, 1975) identifies the potential for measures to replace the construct as the focus of interest. Ijiri (1967, 1975) provides an example of how investors use a firm's accounting measures to learn about the firm's economic viability (the construct of interest).[2] When strategic objectives are the constructs of interest, this phenomenon involves managers failing to fully appreciate the role of measures as mere representations of strategy, a phenomenon we label *surrogation*.

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[2] A specific example of this phenomenon is seen in the relationship between accounting earnings and Hicksian income (Hicks 1939; Schipper and Vincent 2003). A firm's stakeholders rely on accounting earnings reported on the income statement to learn about the firm's Hicksian income. However, through repeated use of the earnings measure, investors may begin to act as though earnings, and not Hicksian income, is the construct of interest. Strong market responses to firms just missing earnings benchmarks (e.g., Burgstahler and Dichev 1997) is consistent with surrogation at the market level, and suggest that surrogation has non-trivial implications.

In some scenarios, surrogation is of little consequence. For example, if a construct is perfectly represented by a single measure or set of measures (Luft and Shields 2003; Farrell 2009), then the notion of surrogation is moot. However, because strategic constructs are typically ill-defined and complex, measures of constructs will be imperfect. That is, no single measure or set of measures perfectly captures the strategic construct in all scenarios to which the construct applies (Baker 1992, 2000; Bushman and Indjejikian 1993).

Surrogation may also be moot when complete contracts are in place. If a contract is designed such that a wealth-maximizing response yields optimal results in terms of the underlying constructs of interest, then employee beliefs about the true objective of the firm (maximize measure versus maximize construct) may be irrelevant. However, a growing body of work suggests that complete contracts are rarely seen in practice. Indeed, because of human limitations, firms must typically make trade-offs between contract completeness and contract complexity, such that the optimal contract is incomplete (Williamson 1985).

Notwithstanding these exceptions, surrogation is potentially detrimental in many scenarios, especially those in which the use of strategic performance measurement systems is most beneficial. For instance, suppose that a manager is charged with evaluating whether a customer-satisfaction focus increases financial performance. If the manager has used customer satisfaction survey scores as surrogates for customer satisfaction, and those scores suggest that satisfying customers does not improve financial results, then the manager may recommend moving away from a customer focus, rather than first evaluating the validity of the survey results. To the extent that the survey scores are an incomplete or imperfect proxy for the strategic construct of customer satisfaction, the decision to abandon the strategy could be suboptimal. Moreover, a manager who defines a strategic construct via an inherently imperfect measure may be less likely to recognize when the measure becomes obsolete, or may not perceive the benefits of a new measure that better captures the strategic construct in a changing business environment. Regardless of its "net" effect, the potential for the costs associated with surrogation to mitigate the effectiveness of the strategic performance measurement system highlights the need for a deeper understanding of the surrogation phenomenon.

Research in psychology provides a theoretical explanation for *why* managers use performance measures as surrogates for strategic constructs. In particular, Kahneman and Frederick (2002) discuss the notion of *attribute substitution*, which occurs when an individual has to make a judgment about a complex "target attribute," and instead relies on a more easily accessible "heuristic attribute" during the judgment process (see also Schkade and Kahneman 1998; Kahneman et al. 2006).[3]

Kahneman and Frederick (2002, 54) discuss three conditions necessary for attribute substitution to occur. First, the target attribute must be relatively inaccessible. Second, the heuristic attribute must be highly accessible. Third, the substitution of the target attribute with the heuristic attribute must not be consciously rejected. Strack et al.'s (1988) study highlights these three conditions. Strack et al. (1988) ask college students "How many dates did you have last month?" and "How happy are you with your life in general?" and manipulate the order of these questions between subjects. When students answered the happiness question first, the correlation between their answers to the two questions was negligible. However, when students answered the dating question first, the correlation jumped to 0.66. In this example, answering the happiness question first requires making a judgment about the relatively inaccessible attribute of overall happiness. When answering the happiness question second, students appeared to rely on the highly accessible and quantifiable number of dates they had last month to a greater extent than students answering the happiness question first. Though students may have been aware of their weighting of their dating success when assessing their overall happiness, their conscious processing did not reject this substitution.

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[3] Kahneman and Frederick (2002) characterize attribute substitution as a general framework that includes multiple heuristics, including representativeness (Tversky and Kahneman 1974), availability (Tversky and Kahneman 1973), and recognition (Gigerenzer and Goldstein 1996). While surrogation fits within the same attribute substitution framework as the heuristics above, it is also distinct from these other phenomena. Although we rely on the attribute substitution framework to develop our theory, we retain the surrogation label to differentiate our phenomenon of interest from those noted above, and to more consistently reflect Ijiri's (1967, 1975) original discussion of how accounting measures originally developed to represent a construct to eventually replace the construct itself.

In the context of strategic performance measurement systems, the first of the three necessary conditions for attribute substitution is usually satisfied because strategic constructs are typically abstract, ill-defined, and complex. In this study, we consider how incentive compensation, a feature common to strategic performance measurement systems, influences the degree to which the *second* and *third* attribute-substitution conditions are met.[4]

Incentive Compensation

A large body of literature addressing the incentives-performance relationship suggests that a variety of mechanisms determine how incentives affect performance (Bonner and Sprinkle 2002). While no one theory provides a comprehensive explanation for how incentives ultimately influence performance, this research suggests that incentive compensation has attention-directing effects, which influence an individual's experience with performance measures (Atkinson et al. 1997; Lambert 2001). In this study, we investigate how linking managers' compensation to performance measures affects their tendency to use measures as surrogates for strategic constructs.

Incentive Compensation on a Single Performance Measure of a Strategic Construct

Prior research shows that firms often use multiple performance measures to represent a single strategic construct (Banker et al. 2000; Chen et al. 2010) and must decide on the number of strategically linked performance measures on which to provide financial incentives. This decision is non-trivial because firms must balance potentially conflicting factors such as contract completeness and contract complexity in their decisions (Ittner and Larcker 1998). As such, firms often choose to base incentives on only a subset of the available measures.

We first consider the basic context in which incentive compensation depends on a single measure of a strategic construct. Psychology literature demonstrates that individuals have limited attention, and that task structure can affect the attention given to features of the environment (James 1890; Shiffrin 1988). Consistent with this research, we posit that incentive compensation directs managers' limited attention toward the compensated measure as the means by which managers' actions affect their compensation, thereby influencing their strategy-implementation experience (Frederickson et al. 1999). These attention-directing effects of incentive compensation heighten the accessibility of the heuristic attribute, which is here the compensated measure, thereby increasing the likelihood of meeting the second necessary condition for attribute substitution. Further, although managers may recognize their use of a heuristic attribute in their assessment, we expect at least some of them not to reject this substitution. Thus, relative to a scenario in which measures are not used for compensation purposes, incentive compensation on a single measure of a strategic construct increases managers' propensity to exhibit surrogation. This leads to the following hypothesis:

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[4] Kaplan and Norton (1996, 219) advocate tying employee compensation to strategic performance measurement systems, but caution that doing so can be problematic when measures "are not perfect surrogates for the strategic objectives."

> **H1:** Basing compensation on a single measure of a strategic construct leads managers to use the measure as a surrogate for the strategic construct more than they would under a fixed-wage-compensation scheme.

Incentive Compensation with Multiple Performance Measures of a Strategic Construct

Like compensation based on a single measure of a strategic construct, compensation based on multiple measures of a single strategic construct can also lead managers to view those measures as a means by which their actions determine their compensation. Thus, regardless of the number of compensated measures, a manager's attention is directed toward compensated measures, increasing the availability of these heuristic attributes. As with compensation based on a single measure, this heightened attention increases the likelihood of meeting the second necessary condition for attribute substitution. However, managers' attention toward, and experience with, multiple measures may be markedly different relative to when compensation is based on a single measure of a strategic construct. We focus on whether managers' experience with multiple measures will increase the likelihood that they will reject the substitution of the heuristic attribute for the target and, hence, fail to satisfy the third necessary condition for attribute substitution (Kahneman and Frederick 2002).

To the extent that multiple measures are relatively independent, managers compensated on multiple measures confront a greater number of action trade-offs that influence their compensation than managers compensated on a single measure (Wood 1986; Campbell 1988; Bonner 1994). To simplify the action-choice task (Payne et al. 1993) managers likely use similarity assessments, which facilitate object classification, concept formation, and related judgments and decisions (Tversky 1977; Fiske and Taylor 1991). Consistent with this theory, we posit that managers compensated on multiple measures of a single strategic construct will consider the similarities of these measures. In doing so, managers are likely to obtain greater awareness of the overall strategic construct and the extent to which the measures represent the strategic construct. This mechanism occurs in two ways. First, comparing the compensated measures increases the likelihood that managers will realize that all of the compensated measures represent the same strategic construct (Johnson 1988; Markman and Medin 1995; Shah and Oppenheimer 2011). Second, comparing the compensated measures highlights the limited ability of any one measure to completely represent the corresponding strategic construct (Zhang and Markman 2001). Thus, by performing these similarity assessments, managers will attend more to the representational role of the measures as proxies for a related strategic construct relative to a setting with a single compensated measure. This increases the likelihood that managers will reject the tendency to replace the strategic construct with performance measures.

Compensation on multiple measures could increase the tendency to use simplifying heuristics and thus increase the accessibility of heuristic attributes, which affects the second necessary condition for attribute substitution. However, such compensation also brings conscious operations into play that increase the likelihood that the substitution of the target attribute with the heuristic attribute will be rejected, reducing the likelihood that the third necessary condition for attribute substitution will be satisfied (Chaiken 1980; Schwarz 2004; Alter et al. 2007). Therefore, we hypothesize that managers compensated on multiple measures of a strategic construct are less likely to exhibit surrogation than those compensated on a single measure of a strategic construct.

> **H2:** Basing compensation on multiple performance measures of a single strategic construct leads managers to use compensated measures as surrogates for the strategic construct less than they would under a single-measure compensation scheme.

III. METHOD

We tested our hypotheses in a laboratory experiment in which participants used computers to work through self-guided instructions and perform various tasks. Participants in our main experiment were 79 students recruited from graduate business courses at a highly ranked business school in the United States. On average, participants were 29 years old, and 61 percent were male.

Experimental Task

After an introduction to the study, participants designed a virtual creature using the *Creature Creator* module of the computer game *Spore* (Electronic Arts 2008; Lawton 2008). Instructions indicated that participants should use the following strategy for designing their creature:

> Design a creature that can socialize with other life forms such that it will become the dominant species on the planet.[5]

Figure 2 provides a sample screen similar to what participants viewed. At the outset of this first task in the experiment, participants received an endowment of 500 "DNA points," with which to purchase various body parts for their creature.[6] Each part added one or more abilities to the creature. Participants could see what abilities each part added before purchase, as well as measures of each ability in real-time as they added each part. When a part was added to the creature, the cost of the part was automatically deducted from the participant's DNA point endowment. If a participant removed a part from the creature, then the program immediately adjusted the creature's ability measures and refunded the participant's DNA points.

While designing their creature, participants saw numerous measures of their creature's abilities, including four measures tied to their creature's ability to "socialize," the strategic construct of interest.[7] These quantitative measures reflected the creature's ability to *sing*, *dance*, *charm*, and *pose* (each on a scale of 0 to 5). Instructions indicated that each of these abilities was equally important to their creature's ability to socialize.

All participants received compensation based on a post-experimental assessment of their creature's ability to achieve global domination via socialization. This compensation allowed us to hold constant, across all conditions, the incentive to design a creature in line with the provided strategy. We calculated participant compensation after the completion of the experimental session. A research assistant placed each participant's creature individually in a *Spore* environment, gave simple commands to the creature to befriend neighboring creatures, and recorded the time needed to befriend two creatures.[8] Within each experimental condition, we compared the performance of each

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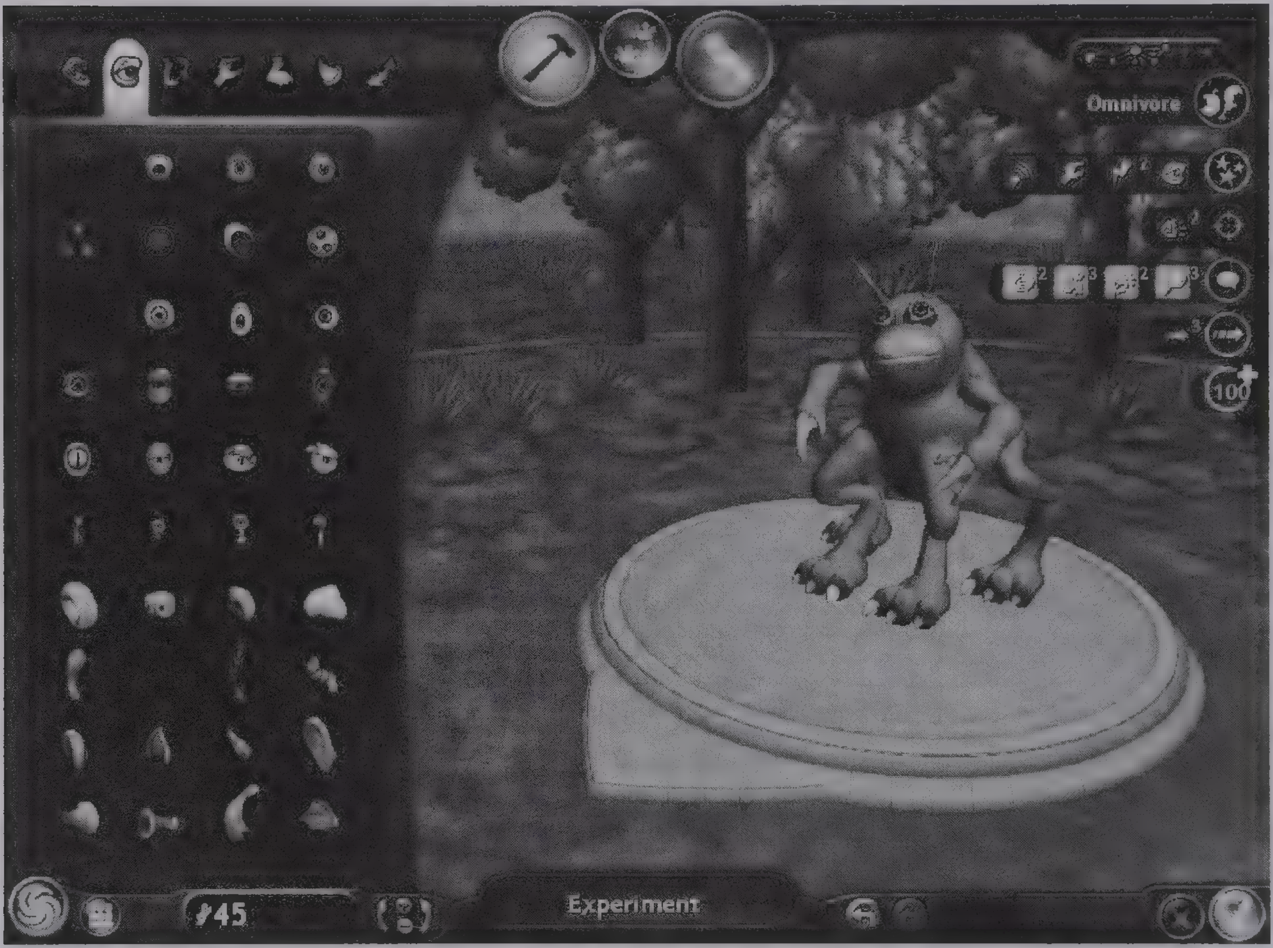
[5] The concepts of "socializing" and becoming "dominant" in this strategy statement may seem at odds with each other. To clarify, we provided participants with a general introduction to the game of *Spore*, which highlighted two strategies for becoming the dominant species: "You can become the dominant species by making friends with other species, or by overpowering them." We provided this information to participants to mitigate any confusion over features of the *Spore* software unrelated to the experimental task. To further ensure that participants understood the general *Spore* introduction and the specific instructions for the experimental task, we required all participants to correctly answer a question indicating that social abilities were most important to their creature's ability to achieve global domination in the task. Any confusion that may have persisted should work against our finding support for our directional predictions.

[6] This budget limit (500 DNA points) served to ensure that participants had enough resources to design a creature in line with the provided strategy, but not without making trade-offs. That is, given this resource constraint, participants had to choose among multiple ways of implementing the stated strategy.

[7] Measures of other abilities inherent to the *Spore* software, but unrelated to our experimental task, were also visible to participants. To avoid confusion, all available measures were described in the instructions. Availability of all measures was held constant across all experimental conditions.

[8] A creature's total social-ability level (i.e., the sum of a creature's *sing*, *dance*, *charm* and *pose* levels) is a key determinant of its time to befriend, as creatures with lower measure levels take longer to befriend other creatures.

FIGURE 2
Screenshot of Initial Creature Design



Above is a sample screenshot of the *Creature Creator* module of the computer game *Spore*. The left side of the screen displays various body parts that can be added to the creature. The upper-right corner of the screen displays various measures of the creature's abilities. The third row of boxes on the upper right displays the four social-ability measures: *sing* (level = 2), *dance* (level = 3), *charm* (level = 2), and *pose* (level = 3). During the experiment, these measures were constantly available and were adjusted real-time as participants added and removed parts from their creatures. "DNA points," used as currency to purchase parts, are displayed in the bottom-left corner of the screen (currently DNA = 45). Buttons at the top center of the screen allow participants to paint their creature and to see their creature move in a simple environment.

participant's creature to that of nine other randomly selected participants' creatures, and this relative performance ranking determined one portion of their compensation. The top performer in each group earned \$20, the second best performer received \$18, etc. Providing compensation tied directly to successful implementation of the strategy helped reinforce the equal importance of all four measures in minimizing the time to befriend two creatures. By heightening participants' awareness of all four social-ability measures, including this compensation works against finding evidence of surrogation in our experiment.

Note that neither "time to befriend" nor the related tournament-based compensation serve as our dependent variable. Although these variables may indirectly capture the extent to which participants use measures as surrogates for the strategy, they also reflect participants' desire to maximize wealth. Consequently, using these variables introduces a wealth-maximization confound that would limit our ability to test the effect of incentives on surrogation. For this reason, we constructed an alternative dependent variable that does not suffer from this confound, which we describe following the discussion of our independent variable.

Independent Variable

In addition to the compensation tied to the participant's success at strategy implementation, some participants received incentive compensation tied directly to measures of their creature's social-ability. This compensation is our independent variable, which we manipulate at three levels: no social-ability compensation (*fixed-wage-compensation*), compensation tied to a single measure of the creature's social-ability (*single-measure-compensation*), and compensation tied to three of the four social-ability measures (*multiple-measure-compensation*). In the fixed-wage-compensation condition, participants received \$10 regardless of their creature's social-ability measures. In the single-measure-compensation condition, participants received \$2 per level of their creature's *sing* measure. For example, a participant who designed a creature with *sing* level 4 would receive 4 × \$2 = \$8. In the multiple-measure-compensation condition, participants received \$1 per level of their creature's *sing*, *dance*, and *charm* measures. For example, a participant who designed a creature with *sing* level 3, *dance* level 2, and *charm* level 4, would receive (3 × \$1) + (2 × \$1) + (4 × \$1) = \$9.

Dependent Variables

We elicited our primary dependent measure using a second task after participants finished designing their virtual creature. To elicit this dependent measure, participants viewed 18 successive pictures of other virtual creatures. With each of these creatures, participants saw two potential combinations of the four social-ability measures (*sing*, *dance*, *charm*, and *pose*). We labeled these combinations as "modification packages." As they viewed each creature, participants selected the modification package that they believed would best implement the stated strategy from the initial creature design task of the experiment. In addition, the instructions indicated that participants should assume that the DNA cost was equal across each pair of modification packages. Participants viewed each of the 18 creatures one at a time, and made their modification-package selections as they viewed each creature. Once participants selected a modification package and moved on to the next creature, they could not go back and change earlier modification-package selections. Figure 3 depicts an example of what participants saw as they made these selections.[9]

We designed modification-package pairings *ex ante* to allow us to use participants' selections to evaluate the extent to which our compensation manipulation influenced participants' tendency to use compensated measures as surrogates for strategy. To illustrate, consider the sample pairing presented in Figure 3, which includes the following two modification-package options:

Modification package #1: *sing* = 2, *dance* = 4, *charm* = 1, and *pose* = 5.
Modification package #2: *sing* = 3, *dance* = 2, *charm* = 4, and *pose* = 2.

Given this pairing, if a participant selects modification package #2, then this selection is consistent with the goal of maximizing *sing*, because *sing* is 2 in modification package #1 and is 3 in modification package #2. Thus, selecting modification package #2 is consistent with surrogation with *sing*. Selecting modification package #2 is also consistent with the goal of maximizing *sing*, *dance*, and *charm*, because the sum of those three abilities is 7 in modification package #1 and is 9

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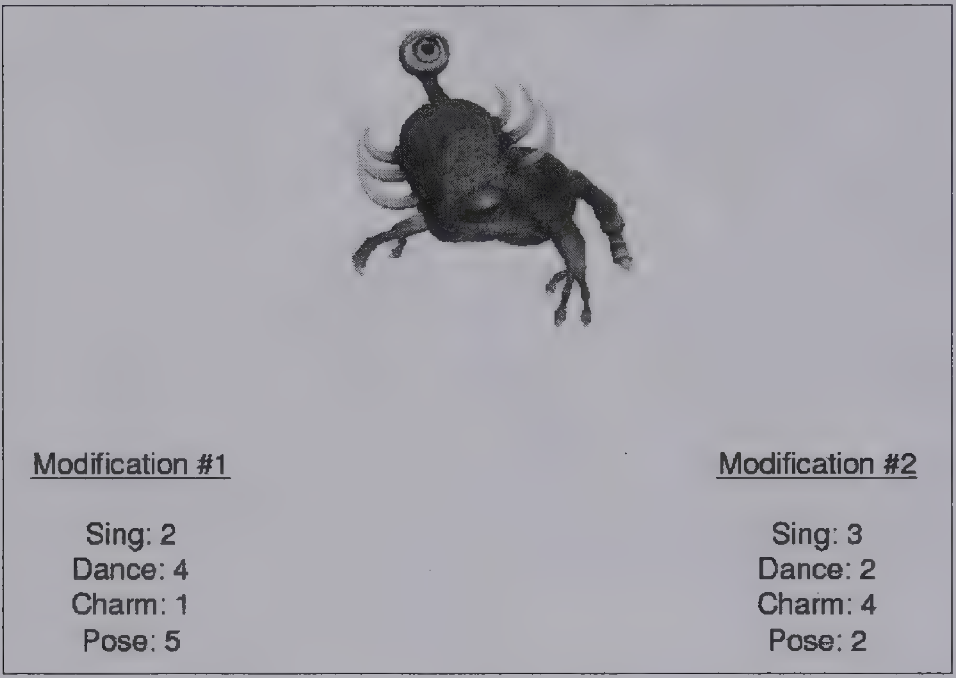
[9] To control for an order effect, we created four randomly determined orders for the 18 choices, and fully crossed these four order conditions with our three compensation conditions. The order in which choices are made does not alter our results. Thus, we ignore this variable in subsequent analyses and discussion.

FIGURE 3
Sample Modification-Package Choice



Modification #1

Sing: 2
Dance: 4
Charm: 1
Pose: 5

Modification #2

Sing: 3
Dance: 2
Charm: 4
Pose: 2

Above is a sample of one of the 18 modification-package pairings used to generate the primary dependent variable for the study. For each pairing, participants were shown a picture of a creature along with the two modification packages. Participants were asked to select the modification package that they believed, if added to the creature, would best enable them to implement the provided strategy. After making their selection, participants clicked "Next" to move on to the next creature and modification-package pairing.

in modification package #2. Thus, selecting modification package #2 is also consistent with surrogation with *sing*, *dance*, and *charm*.

To construct our primary dependent variable, we sum the number of selections a participant makes that are consistent with surrogation. In determining whether a selection is consistent with surrogation, we focus on behavior as it pertains to the social-ability measure(s) for which participants had previously earned compensation during the creature-design task of the experiment. Thus, for single-measure-compensation participants, we focus on their behavior as it pertains to the *sing* measure, and investigate whether participants' selections maximize *sing*. For multiple-measure-compensation participants, we focus on their behavior as it pertains to the *sing*, *dance*, and *charm* measures, and investigate whether participants' selections maximize the sum of *sing*, *dance*, and *charm*.

Although all participants saw the same 18 creatures and associated modification-package pairings, not all 18 pairings can be used to measure behavior consistent with surrogation. For example, a pairing in which *sing* is the same in both modification packages cannot be used to measure surrogation with *sing*. Similarly, a pairing in which the sum of *sing*, *dance*, and *charm* is the same in both modification packages cannot be used to measure surrogation with *sing*, *dance*, and *charm*. Thus, we divide the number of selections a participant makes that are consistent with surrogation in their experimental condition by the number of pairings that can be used to evaluate surrogation in that condition to come up with a participant's "comprehensive surrogation score."

TABLE 1
Modification-Package Pairings

Pairing	Surrogation-Consistent Modification Package[a] S	D	C	P	S+D +C	S+D+ C+P	Alternative Modification Package[a] S	D	C	P	S+D +C	S+D+ C+P	Pairing Used to Evaluate *sing* Surrogation?[b]	Pairing Used to Evaluate *sing*, *dance*, and *charm* Surrogation?[b]	Pairing Used to Evaluate Opportunity Cost?[c]
1	4	3	5	1	12	13	3	5	3	2	11	13	Yes	Yes	No
2	5	2	4	2	11	13	1	4	5	3	10	13	Yes	Yes	No
3	3	1	5	2	9	11	1	4	1	5	6	11	Yes	Yes	No
4	5	2	4	1	11	12	2	4	1	5	7	12	Yes	Yes	No
5	3	2	5	1	10	11	1	5	3	4	9	13	Yes	Yes	Yes
6	5	2	3	1	10	11	4	1	4	3	9	12	Yes	Yes	Yes
7	4	4	2	2	10	12	1	3	5	5	9	14	Yes	Yes	Yes
8	3	2	4	2	9	11	2	4	1	5	7	12	Yes	Yes	Yes
9	4	2	3	4	9	13	1	3	5	4	9	13	Yes	No	No
10	3	1	4	1	8	9	2	5	1	1	8	9	Yes	No	No
11	3	3	2	1	8	9	2	1	5	3	8	11	Yes	No	Yes
12	5	2	1	3	8	11	2	4	2	5	8	13	Yes	No	Yes
13	2	4	3	2	9	11	2	5	1	3	8	11	No	Yes	No
14	3	5	4	2	12	14	3	1	5	5	9	14	No	Yes	No
15	4	3	4	1	11	12	4	1	3	5	8	13	No	Yes	Yes
16	2	4	3	1	9	10	2	1	5	5	8	13	No	Yes	Yes
17[d]	5	1	4	1	10	11	4	3	2	1	9	10	No	No	NA
18[d]	3	3	2	2	8	10	3	4	1	3	8	11	No	No	NA

This table lists the 18 modification-package pairings provided to participants. Although pairings are numbered in the table, they were not numbered in the study, and were presented in four, predetermined, random orders (order is fully crossed with the compensation manipulation).

[a] The column labeled "Surrogation-Consistent Modification Package" gives the levels of *sing* (S), *dance* (D), *charm* (C), and *pose* (P) of the modification package within each pairing that represents an option consistent with surrogation with *sing*, surrogation with *sing*, *dance*, and *charm*, or both. The column labeled "Alternative Modification Package" gives the alternative to the surrogation-consistent option. Although not shown in the table, modification packages are labeled for each pairing as "Modification #1" or "Modification #2" (see Figure 3). In addition, the label on the surrogation-consistent modification package is randomly determined, and is held constant for all participants.

(continued on next page)

TABLE 1 (continued)

[b] The table also shows whether the pairing is used to evaluate surrogation with *sing* or surrogation with *sing*, *dance*, and *charm*. For example, in Pairing #9, a choice of the modification package listed in the "Surrogation-Consistent Modification Package" column is consistent with surrogation with *sing*, because it has a higher *sing* level than the alternative. However, this pairing is not used to evaluate surrogation with *sing*, *dance*, and *charm*, as the total of these three measures is equal across modification packages in this pairing. However, Pairing #5 can be used to evaluate both surrogation with *sing*, and with *sing*, *dance,* and *charm*, because one package is dominant on both *sing* and on the total of the *sing*, *dance,* and *charm* measures.

[c] The last column indicates whether there is an opportunity cost to selecting the surrogation-consistent Modification Package. *No opportunity cost* pairings are comprised of modification packages in which the total level of the four social-ability measures are equal between the two modification packages. Thus, a participant who chooses the modification package consistent with surrogation incurs no cost to do so. In contrast, *opportunity cost* pairings are comprised of modification packages in which the total level of the four social-ability measures are lower for the surrogation-consistent option than for the alternative.

[d] Pairings #17 and #18 are not used in any test of surrogation. These pairings are included in the experiment as distracters. For Pairing #17, the modification package with higher *sing*, and *sing*, *dance*, and *charm* levels also has a higher total across all four social-ability measures, confounding surrogation and optimal-card selection. For Pairing #18, both *sing* and the total of *sing*, *dance*, and *charm* are equal across packages, so there is no surrogation-consistent modification package for this pairing.

Twelve of the 18 pairings are relevant for evaluating participants' tendency to use *sing* as a surrogate for social-ability, the strategic construct. A different set of 12 pairings are relevant for evaluating participants' tendency to use *sing*, *dance*, and *charm* as surrogates for social-ability. Table 1 presents the 18 modification-package pairings, along with a breakdown of the relevant pairings for measuring behavior consistent with surrogation in each experimental condition.

Modification-package pairings differed according to the extent to which participants had to incur an opportunity cost to make a selection consistent with surrogation. We label pairings in which the total level of the four social-ability measures (*sing*, *dance*, *charm*, and *pose*) is equal between the two modification packages as *no opportunity cost* pairings. With these pairings, a participant selecting the modification package consistent with surrogation does not forgo a higher total social-ability (i.e., incurs no opportunity cost) to do so. In contrast, we label pairings in which the surrogation-consistent modification package has a lower total social-ability level as *opportunity cost* pairings. With these pairings, a participant selecting the surrogation-consistent modification package incurs an opportunity cost because a creature's total social-ability level is a key determinant of its ability to achieve the strategic objective of socializing with other creatures.[10] To illustrate, consider again the pairing shown in Figure 3. While modification package #2 maximizes the *sing* level because *sing* is 2 in modification package #1 and is 3 in modification package #2, modification package #1 has a higher total social-ability level because *sing*, *dance*, *charm*, and *pose* levels sum to 12 for modification package #1 and sum to 11 for modification package #2. Therefore, selecting the surrogation-consistent modification package in this example entails forgoing higher total social-ability, and is thus categorized as an opportunity cost pairing. The last column in Table 1 indicates the opportunity cost status of each of the surrogation-consistent modification package pairings.

In summary, we compute three measures of surrogation: *comprehensive*, *no opportunity cost*, and *opportunity cost*. As described above, we compute these three measures differently depending on how participants were compensated in the creature-design task. Specifically, we measure single-measure-compensation (multiple-measure-compensation) participants' propensity to use *sing* (*sing*, *dance*, and *charm*) as a surrogate for social-ability. We compute both versions of these measures for the fixed-wage-compensation participants to allow for baseline measures of surrogation against which the other conditions can be compared.

Session Timeline

As participants arrived at a session, we randomly assigned them to one of the three compensation conditions. Participants read computer-based instructions and watched a video tutorial on using the *Creature Creator* module of *Spore*. Participants then practiced building a virtual creature for 12 minutes. After practicing, participants read additional instructions that included information about participants' compensation and the strategy to be implemented, took a short quiz to ensure their understanding of the instructions, and then proceeded to design their virtual creatures. Next, participants viewed the 18 modification-package pairings, and made their selections for each pairing. Finally, participants completed a questionnaire to elicit demographic and process-related information. We debriefed and compensated participants after all sessions were completed.

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[10] Our use of the label "opportunity cost" is not intended to convey that participants incurred a direct financial cost in choosing one of the modification packages. Rather, our intention in our main experiment is to differentiate pairings in which a surrogation-consistent selection forgoes something of value in executing the strategy (i.e., a creature with a higher total social-ability level).

Additional Discussion of the Experimental Design

A number of issues relating to our experimental design warrant further discussion. First, each of the four social-ability measures completely captures non-overlapping dimensions of the social-ability construct. While at odds with our contention that no single measure or set of measures perfectly captures the strategic construct across all real-world settings in which the construct applies, this design feature allows for a clean test of our hypotheses. Importantly, this design choice highlights the incomplete nature of the compensation contract in all three experimental conditions. A complete contract in our setting would place positive and equal incentive weights on all four social-ability measures. However, as discussed earlier, our compensation schemes are meant to reflect real-world settings in which incomplete contracts are common (Williamson 1985).

Second, we provide no compensation for participants' modification-package selections. Single- or multiple-measure-based compensation similar to that provided to participants during the creature-design task of the experiment would introduce a "wealth maximization" confound, precluding us from observing the phenomenon of interest. Thus, we use an experimental setting that is admittedly different from many real-world settings involving incentives. Indeed, although real-world settings that parallel our experimental setting likely exist (e.g., see Malina and Selto 2004), our experimental setting is not meant to map into any specific real-world setting. Rather, our intent is to create a setting that yields a valid measure of surrogation and a clean test of our hypotheses. However, the results of our main study speak directly to settings where managers must make strategic decisions without having compensation tied directly to every aspect of decision-making.[11,12] Further, to address alternative explanations for our results, we conduct an additional experiment in which we compensate participants for strategically aligned modification-package selections. We discuss this follow-up experiment and associated results in Section IV.

Third, the single-measure-compensation scheme and the multiple-measure-compensation scheme differ not only in terms of the number of compensated measures (our manipulation of interest), but also in terms of the representativeness of the compensated measures. In general, the better the set of measures used for decision making represents strategy, the better the strategic decisions will be. However, our theory focuses on the effect of the number of compensated measures on *surrogation*, and not on the increase in the optimality of strategy-related decisions induced by greater representativeness. Thus, if we chose to measure surrogation behavior indirectly

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[11] One potential concern is that, absent incentives, participants in all conditions may exert minimal effort when making their modification-package selections. Notably, low effort works against finding support for our directional predictions, as low effort increases noise in our dependent measures across all conditions. Additionally, if participants exerted minimal effort and selected modification packages randomly, then they would select the surrogation-consistent option about half the time. In contrast, high effort implies a lower likelihood that participants select the surrogation-consistent option. In all three conditions, participants selected the surrogation-consistent option significantly *less* than half the time ($p < 0.01$ in all cases). Furthermore, within each condition, participants chose the surrogation-consistent option significantly more often for *no opportunity cost* pairings than for *opportunity cost* pairings ($p < 0.01$ in all cases), providing additional evidence of an effortful evaluation in this task.

[12] Relatedly, Holmstrom and Milgrom (1991) analyze a model wherein an employee is charged with performing multiple tasks. They show that compensating on only a subset of tasks induces the employee to allocate attention to the compensated tasks at the expense of those tasks that are not compensated, resulting in unbalanced task performance. Our analysis differs from Holmstrom and Milgrom (1991) in that compensation in our study is also expected to lead to an unbalanced interpretation of the strategic construct with the number of compensated measures moderating this effect. Further, our primary dependent variable goes beyond wealth-maximizing behavior to capture surrogation-related behavior induced by the attention-directing effects of incentive compensation. Thus, our study complements Holmstrom and Milgrom (1991) by suggesting additional, but separate, effects of tying compensation to a subset of available measures.

via a measure of decision optimality, then disentangling the effects of multiple-measure-compensation and the representativeness of compensated measures would not be possible (because both are expected to influence decision optimality). However, our dependent variable captures surrogation independent of decision optimality by measuring the tendency to ignore non-compensated measure(s). By constructing our dependent variable in this way, the greater degree of representativeness of the compensated measures in the multiple-measure-compensation condition works against finding support for our hypotheses. Specifically, surrogation in the single-measure-compensation condition involves attending to a smaller proportion of social-ability than surrogation in the multiple-measure-compensation condition. Consequently, to the extent that representativeness of the compensated measures influences participants' judgments and decisions, participants in the single-measure-compensation condition are more likely to recognize that the compensated measure is insufficient in terms of its representativeness, decreasing participants' propensity to use the compensated measures as surrogates for social-ability.

Fourth, our use of the computer game *Spore* for our experimental instrument helps protect against the possibility that participants' priors regarding appropriate surrogates for strategic constructs affected their judgments and decisions, because the setting is far removed from most participants' practical and educational experiences. Importantly, however, the setting still allows participants to "get their hands dirty" implementing the provided strategy and to deal with various associated measures.[13]

Finally, we took several steps to maximize the internal validity of our study. To ensure that the strategy statement was salient, we explicitly stated the strategy on two occasions, the latter of which occurred immediately before participants designed their creature. We also included in the task instructions an explicit statement that all four measures of social-ability are equally important in implementing the strategy and achieving global domination. Further, participants had to demonstrate thorough understanding of the various aspects of our research design via a quiz.[14]

IV. RESULTS

We begin with an analysis related to the effectiveness of our incentive compensation manipulation. To assess whether participants performed the initial creature design task consistently with the provided incentives, we asked each participant to "indicate how much attention you gave to the [*sing*, *dance*, *charm*, and *pose*] abilities, by allocating 100 points among the four abilities. Allocate more points to abilities to which you gave more attention." We compared the mean number of points allocated either to *sing*, or to *sing*, *dance*, and *charm* across conditions (reported in Table 2). Not surprisingly, single-measure-compensation participants allocated more points (i.e., paid more attention) to the *sing* measure ($n = 26$, mean $= 35.1$ percent) than fixed-wage-compensation participants ($n = 27$, mean $= 22.4$ percent) ($t = 3.44$, $p < 0.01$).[15] Likewise, multiple-measure-compensation participants allocated more points to the *sing*, *dance*, and *charm* measures ($n = 26$, mean = 86.2 percent) than fixed-wage-compensation participants (mean 81.1 percent) ($t = 1.98$, $p = 0.03$).[16,17] Collectively, these results suggest that our compensation manipulation was successful.[18] We now turn to our hypothesis tests and related supplemental analyses.

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[13] None of the participants reported any prior *Spore* experience. Further, experience playing video games is not a significant predictor of any of the results in our paper.

[14] Two quiz questions addressed the strategy statement and the equal importance of each ability measure. One multiple-choice question read, "Which of the following will help your creature achieve global domination most rapidly?" Options were "The ability to fly," "Social abilities," "Attack abilities," and "The ability to jump." Participants could not proceed until they gave the correct response ("Social abilities"). The other question read, "In *Spore*, which of the following is most important in determining your creature's ability to make friends with other creatures?" Options were *sing*, *dance*, *charm*, *pose*, and "All socializing abilities are equally important in determining a creature's ability to make friends." Again, participants could not continue until they gave the correct responses ("All socializing abilities are equally important in determining a creature's ability to make friends.").

[15] We report one-sided p-values for tests of directional predictions. Otherwise, we report two-sided p-values.

Primary Tests of Hypothesis 1

H1 predicts that compensation on a single measure of a strategic construct increases managers' propensity to use a measure as a surrogate for the strategic construct relative to a fixed-wage-compensation scheme. To test this hypothesis, we examine fixed-wage-compensation and single-measure-compensation participants' surrogation scores. H1 predicts that, relative to fixed-wage-compensation participants, single-measure-compensation participants are more likely to choose modification packages with higher levels of the *sing* measure, at the expense of other, equally important, social-ability measures. Such behavior would be consistent with surrogation.

To test H1, we first calculate participants' *comprehensive* surrogation scores—the percentage of each participant's modification-package choices that are consistent with surrogation with *sing* as described in Section III. Next, we compare the mean *comprehensive* surrogation score between fixed-wage-compensation and single-measure-compensation conditions. At the operational level, H1 is supported if single-measure-compensation participants' surrogation scores are higher than fixed-wage-compensation participants' surrogation scores. Participants' mean *comprehensive* surrogation score is 0.59 in the single-measure-compensation condition, but is only 0.39 in the fixed-wage-compensation condition (see Table 3, Panel A). As we report in Table 3, Panel B, the difference of 0.20 in *comprehensive* surrogation scores across these two conditions is statistically significant ($t = 4.56$, $p < 0.01$).

These results are robust to distinguishing surrogation scores in terms of whether there is an opportunity cost associated with selecting a surrogation-consistent modification package. Panel A of Table 3 presents participants' *opportunity cost* and *no opportunity cost* surrogation scores. As reported in Panel B of Table 3, mean *opportunity cost* and *no opportunity cost* surrogation scores

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[16] Participants in the single-measure-compensation condition designed creatures with a higher level of *sing* (mean = 3.81) than participants in the fixed-wage-compensation condition (mean = 2.96) ($t = 2.98$, $p < 0.01$). However, participants in the multiple-measure-compensation condition (mean = 8.88) did not design creatures with higher combined levels of *sing*, *dance*, and *charm* than participants in the fixed-wage-compensation condition (mean = 8.41) ($t = 0.80$, ns), though the means are directionally consistent with the expected influence of incentives. This latter result likely stems from parameters inherent to the *Spore* software that lead to a small difference between the sum of the *sing*, *dance*, and *charm* levels for a participant maximizing on those measures and a participant maximizing on all four social-ability measures. This constraint is one reason why we elicited participants' self-reported point (i.e., attention) allocations.

[17] Although our hope in using an abstract environment was to limit preconceived notions about appropriate surrogates for strategy, participants nonetheless judged *charm* to be especially important for their creatures' social-ability. On average, participants allocated more points to *charm* than any other social-ability. This works against our finding results, as it adds noise to every condition. It specifically works against our finding support for H2, as an emphasis on *charm* would increase, not decrease, the tendency to use measures as surrogates for the strategic construct in our multiple-measure-compensation condition.

[18] Participants' total compensation did not differ across conditions ($F = 0.97$, ns). We designed our experiment to maximize the likelihood that, on average, creature design compensation and the additional compensation were approximately equal. The proportion of total compensation stemming from the creature design compensation for the fixed-wage-compensation, single-measure-compensation, and multiple-measure-compensation conditions were 50.5 percent, 43 percent, and 48 percent, respectively. The lower proportion in the single-measure-compensation condition stems from the fact that the mean creature-design compensation was higher in both the multiple-measure-compensation (mean = \$8.88) and the fixed-wage-compensation (mean = \$10.00) conditions than in the single-measure-compensation condition (mean = \$7.62).

TABLE 2

Responsiveness to Incentive Compensation Manipulation

Panel A: Mean Creature Design Point Allocations[a] (Standard Deviation)

	Compensation Condition[b]		
Social-Ability Measures	**Fixed-Wage Compensation (n = 27)**	**Single-Measure Compensation (n = 26)**	**Multiple-Measure Compensation (n = 26)**
Sing	22.4 (7.6)	35.1 (17.6)	26.7 (7.3)
Dance	23.8 (8.6)	17.9 (7.0)	26.5 (9.6)
Charm	34.9 (16.9)	34.5 (19.1)	33.0 (14.5)
Pose	19.2 (8.9)	12.5 (7.5)	13.9 (9.5)

Panel B: Statistical Tests

	Estimate	**t**	**p-value[c]**
Points Allocated to *sing*: Fixed-Wage versus Single-Measure Compensation	12.7	3.44	< 0.01
Points Allocated to *sing*, *dance*, and *charm*: Fixed-Wage versus Multiple-Measure Compensation	5.1	1.98	0.03

[a] *Creature Design Point Allocations* represent participants' allocations of exactly 100 points across the four social-ability measures—*sing*, *dance*, *charm*, and *pose*—when asked to "indicate how much attention you gave to the [*sing*, *dance*, *charm*, and *pose*] abilities, by allocating 100 points among the four abilities. Allocate more points to abilities to which you gave more attention."

[b] This represents compensation tied directly to social-ability measures of the creatures initially designed by participants. We manipulated this source of compensation at three levels: no social-ability compensation (*fixed-wage-compensation*), performance-based compensation based on a single measure of the creature's social abilities (*single-measure-compensation*), and performance-based compensation based on three of the four measures of the creature's social abilities (*multiple-measure-compensation*). In the fixed-wage-compensation condition, participants received \$10 regardless of the state of their creature's social-ability measures. In the single-measure-compensation condition, participants received \$2 per level of their creature's *sing* measure (e.g., a level 4 *sing* measure resulted in compensation of \$8). In the multiple-measure-compensation condition, participants received \$1 per level of their creature's *sing*, *dance*, and *charm* measures (e.g., a creature with *sing* level 3, *dance* level 2, and *charm* level 5 resulted in compensation of \$10).

[c] All p-values are presented on a one-tailed basis, given the expected influence of incentive compensation.

are higher in the single-measure-compensation condition than in the fixed-wage-compensation condition (*opportunity cost*: $t = 2.49$, $p = 0.01$; *no opportunity cost*: $t = 4.15$, $p < 0.01$).[19]

Collectively, these results support our hypothesis that compensation on a single measure of a strategic construct increases managers' propensity to use the compensated performance measures as

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[19] As discussed earlier, our *opportunity cost* surrogation scores measure participants' tendency to sacrifice a high overall social-ability for higher levels of the compensated measures. However, participants could also have viewed low variance among social abilities as desirable. As such, opportunity cost surrogation may reflect sacrificing low social-ability variance to maximize the compensated measures. For both hypotheses, untabulated results investigating this possibility are inferentially identical to those of our main analyses.

TABLE 3

Tests of Hypotheses

Panel A: Mean Surrogation Scores (Standard Deviation)

	Compensation Condition[a]		
Surrogation Scores	**Fixed-Wage Compensation (n = 27)**	**Single-Measure Compensation (n = 26)**	**Multiple-Measure Compensation (n = 26)**
Surrogation Score: Comprehensive[b]	0.39[e] (0.12)	0.59 (0.19)	0.46 (0.25)
Surrogation Score: No Opportunity Cost[c]	0.62[e] (0.20)	0.84 (0.18)	0.63 (0.26)
Surrogation Score: Opportunity Cost[d]	0.15[e] (0.23)	0.34 (0.32)	0.29 (0.31)

Panel B: Hypothesis Tests

	Estimate	**t**	**p-value[f]**
H1: Fixed Wage versus Single-Measure			
Comprehensive	0.20	4.56	< 0.01
No Opportunity Cost	0.22	4.15	< 0.01
Opportunity Cost	0.19	2.49	0.01
H2: Single-Measure versus Multiple-Measure			
Comprehensive	−0.13	2.15	0.02
No Opportunity Cost	−0.21	3.38	< 0.01
Opportunity Cost	−0.05	0.58	ns

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[a] Compensation condition represents compensation tied directly to social-ability measures of the creatures initially designed by participants. We manipulated this source of compensation at three levels: no social-ability compensation (*fixed-wage-compensation*), performance-based compensation based on a single measure of the creature's social abilities (*single-measure-compensation*), and performance-based compensation based on three of the four measures of the creature's social abilities (*multiple-measure-compensation*). In the fixed-wage-compensation condition, participants received $10 regardless of the state of their creature's social-ability measures. In the single-measure-compensation condition, participants received $2 per level of their creature's *sing* measure (e.g., a level 4 *sing* measure resulted in compensation of $8). In the multiple-measure-compensation condition, participants received $1 per level of their creature's *sing*, *dance*, and *charm* measures (e.g., a creature with *sing* level 3, *dance* level 2, and *charm* level 5 resulted in compensation of $10).

[b] *Comprehensive* surrogation scores are the percentage of participants' modification-package choices that are consistent with surrogation with *sing* or with *sing*, *dance*, and *charm*, depending on the compensation condition.

[c] *No Opportunity Cost* surrogation scores are the percentage of participants' modification-package choices that are consistent with surrogation with *sing* or with *sing*, *dance*, and *charm*, depending on the compensation condition, when the total of all four social-ability measure levels does not differ between modification packages.

[d] *Opportunity Cost* surrogation scores are the percentage of participants' modification-package choices consistent with surrogation with *sing* or with *sing*, *dance*, and *charm*, depending on the compensation condition, given that such a choice means forgoing a higher total level of the four social-ability measures.

[e] Because H1 requires a comparison of surrogation with *sing* between the fixed-wage-compensation and single-measure-compensation conditions, the surrogation scores for the fixed-wage-compensation condition for this table are based on surrogation with the *sing* measure.

[f] All p-values are presented on a one-tailed basis, given the directional predictions of H1 and H2.

a surrogate for the strategic construct. Especially interesting is the robustness of these results when considering choices where surrogating with *sing* entails incurring an opportunity cost. Further, these results cannot be explained by a simple propensity toward wealth-maximization, because participants were not paid to maximize a subset of the available measures during this task of the experiment (see Section III).

Primary Tests of Hypothesis 2

H2 predicts that compensation on multiple measures of a strategic construct leads managers to use the compensated measures as surrogates for the strategic construct less than if they are compensated on a single measure. Operationally, this suggests that participants in the multiple-measure-compensation condition will exhibit lower surrogation scores than participants in the single-measure-compensation condition.

In calculating surrogation scores, we focus on the social-ability measures for which participants received compensation while designing their initial virtual creature. Thus, for single-measure-compensation participants, we use the same surrogation scores used to test H1, which are computed based on their surrogation with *sing*. For multiple-measure-compensation participants, we compute surrogation scores based on choices related to the sum of the *sing*, *dance*, and *charm* measure levels. Participants' mean *comprehensive* surrogation score is 0.59 in the single-measure-compensation condition, but is only 0.46 in the multiple-measure-compensation condition (see Table 3, Panel A). As we report in Table 3, Panel B, the difference of 0.13 is significant ($t = 2.15$, $p = 0.02$). These results are robust when considering the *no opportunity cost* surrogation score ($\text{mean}_{\text{single}} = 0.84$; $\text{mean}_{\text{multiple}} = 0.63$; $t = 3.38$, $p < 0.01$). With respect to the *opportunity cost* surrogation score, however, the results are not statistically significant ($\text{mean}_{\text{single}} = 0.34$; $\text{mean}_{\text{multiple}} = 0.29$; $t = 0.58$, ns). Importantly, the comparison of *no opportunity cost* surrogation scores provides the most conservative test of H2. That is, choices consistent with surrogation are *most* likely to be made when surrogation is *not* costly. Thus, our results suggest that multiple-measure-compensation participants are less likely than single-measure-compensation participants to make such choices, but only in the absence of opportunity costs. These results thereby provide some evidence consistent with H2.

A concern regarding our tests of H2 is that these tests entail using a different subset of modification-package pairings across compensation conditions. That is, the *comprehensive*, *no opportunity cost*, and *opportunity cost* surrogation scores are computed using different subsets of modification-package pairings across the single-measure- and multiple-measure-compensation conditions. To control for this inherent lack of consistency, we also test H2 using pairings that are used to compute surrogation scores for both of these compensation conditions (Pairings #1–8 in Table 1). Based on these alternative measures, untabulated results for the *comprehensive* and *no opportunity cost* surrogation scores are inferentially identical to those initially reported. For the alternative *opportunity cost* surrogation scores, however, we find that the difference between multiple-measure-compensation participants (mean = 0.28) and single-measure-compensation participants (mean = 0.40) is marginally significant ($t = 1.35$, $p = 0.09$). Thus, results holding the modification-package pairings constant across conditions provide additional support for H2.

Supplementary Analyses

As a follow-up analysis, we compare the surrogation scores of participants in the fixed-wage-compensation and the multiple-measure-compensation conditions. To serve as a relevant comparison with multiple-measure-compensation participants in this analysis, we calculate the fixed-wage-compensation condition surrogation scores based on modification-package choices that

TABLE 4

Comparison of Fixed-Wage and Multiple-Measure-Compensation Conditions

Panel A: Mean Surrogation Scores (Standard Deviation)

	Compensation Condition[a]	
Surrogation Scores	**Fixed-Wage Compensation (n = 27)**	**Multiple-Measure Compensation (n = 26)**
Surrogation Score: Comprehensive[b]	0.40[e] (0.14)	0.46 (0.25)
Surrogation Score: No Opportunity Cost[c]	0.63[e] (0.23)	0.63 (0.26)
Surrogation Score: Opportunity Cost[d]	0.16[e] (0.23)	0.29 (0.31)

Panel B: Statistical Tests

Fixed-Wage versus Multiple-Measure	**Estimate**	**t**	**p-value[f]**
Comprehensive	0.06	1.16	ns
Low Opportunity Cost	0.00	0.02	ns
High Opportunity Cost	0.13	1.70	0.10

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[a] This represents compensation tied directly to social-ability measures of the creatures initially designed by participants. We manipulated this source of compensation at three levels: no social-ability compensation (*fixed-wage-compensation*), performance-based compensation based on a single measure of the creature's social abilities (*single-measure-compensation*), and performance-based compensation based on three of the four measures of the creature's social abilities (*multiple-measure-compensation*). In the fixed-wage-compensation condition, participants received $10 regardless of the state of their creature's social-ability measures. In the single-measure-compensation condition, participants received $2 per level of their creature's *sing* measure (e.g., a level 4 *sing* measure resulted in compensation of $8). In the multiple-measure-compensation condition, participants received $1 per level of their creature's *sing*, *dance*, and *charm* measures (e.g., a creature with *sing* level 3, *dance* level 2, and *charm* level 5 resulted in compensation of $10).

[b] *Comprehensive* surrogation scores in this table are the percentage of participants' modification-package choices that are consistent with surrogation with *sing*, *dance*, and *charm*.

[c] *No Opportunity Cost* surrogation scores in this table are the percentage of participants' modification-package choices that are consistent with surrogation with *sing*, *dance*, and *charm* when the total of all four social-ability measure levels does not differ between modification packages.

[d] *Opportunity Cost* surrogation scores in this table are the percentage of participants' modification-package choices consistent with surrogation with *sing*, *dance*, and *charm* given that such a choice means forgoing a higher total level of the four social-ability measures.

[e] To compare surrogation with *sing*, *dance*, and *charm* between the fixed-wage-compensation and multiple-measure-compensation conditions, the surrogation scores for the fixed-wage-compensation condition in this table are based on surrogation with *sing*, *dance*, and *charm.*

[f] All p-values are presented on a two-tailed basis, given the lack of directional predictions.

capture surrogation with *sing*, *dance*, and *charm* (i.e., the compensated measures in the multiple-measure-compensation condition).

Participants' mean *comprehensive* surrogation score is 0.46 in the multiple-measure-compensation condition, and is 0.40 in the fixed-wage-compensation condition (see Table 4, Panel A). As we report in Table 4, Panel B, the difference of 0.06 is not significant (t = 1.16, ns, two-tailed). This result is similar for the *no opportunity cost* surrogation score ($\text{mean}_{\text{fixed-wage}}$ =

0.63; $\text{mean}_{\text{multiple}} = 0.63$; $t = 0.02$, ns, two-tailed). With respect to the *opportunity cost* surrogation score, the difference between the fixed-wage-compensation condition (mean = 0.16) and the multiple-measure-compensation condition (mean = 0.29) is marginally significant ($t = 1.70$, $p = 0.10$, two-tailed). These findings are consistent with the primary tests of H2, where both the *comprehensive* and *no opportunity cost* surrogation scores decreased when going from single- to multiple-measure-compensation, but the *opportunity cost* surrogation score did not reflect a significant decrease.

To further explore the surrogation phenomenon, we investigate participants' responses to a post-experimental question eliciting their allocation of attention among the four measures of sociability while making modification-package choices. Similar to the manipulation-check question regarding the creature-design task described earlier, we asked each participant to "indicate how much attention you gave to the [*sing*, *dance*, *charm*, and *pose*] abilities, by allocating 100 points among the four abilities, [allocating] more points to abilities to which you gave more attention" when making their modification-package selections. Our findings, reported in Table 5, suggest that single-measure-compensation participants allocated more attention to the *sing* measure when they selected modification packages than did fixed-wage-compensation participants (mean = 23.7 percent) ($t = 2.52$, $p < 0.01$). Importantly, multiple-measure-compensation participants *did not* allocate significantly more points to the *sing*, *dance*, and *charm* measures (mean = 83.7 percent) than did the fixed-wage-compensation participants (mean 80.0 percent) ($t = 1.57$, ns, two-tailed). This latter finding is particularly notable given evidence reported earlier that participants in the multiple-measure-compensation condition reported allocating *more* attention to the *sing*, *dance*, and *charm* measures than fixed-wage-compensation participants during the initial creature-design task, for which multiple-measure-compensation participants earned incentive compensation. This suggests that while participants in the multiple-measures-compensation setting responded in a wealth-maximizing manner to their incentives in the creature-design task, these incentives did not lead to a significant increase in surrogation for these participants.

Follow-Up Experiment

As discussed earlier, an important aspect of the internal validity of our main experiment relates to our choice not to compensate participants for their modification-package selections. While appropriate for our research purpose, this choice raises questions related to external validity. Therefore, we ran a follow-up experiment to help address this question. Specifically, we investigate whether our results hold if participants are compensated for their modification-package selections.

Our follow-up experiment was identical to our main experiment, with three exceptions. First, the follow-up experiment was 1 × 2, focusing only on the single-measure-compensation and the multiple-measure-compensation conditions, in which we expected surrogation to be most prevalent. Second, participants earned compensation for their modification-package selections. We informed participants that this additional compensation would be based on their ability to implement the strategy, and that this compensation would be determined in a similar manner to the compensation that participants earned for their creature's ability to achieve global domination. Specifically, each participant's performance during the modification-package selection task was compared to that of nine other randomly selected participants within the same experimental condition. The best performer within each ten-person group received $20, the next best performer received $18, etc. Third, we added a post-experimental question assessing participants' understanding of the compensation they earned for their modification-package selections. Participants answered a multiple choice question indicating whether their compensation for their modification-package selections was determined "Based on [their] ability to implement the strategy," "Based on [their] ability to maximize *sing*," or "Based on [their] ability to maximize *sing*, *dance*, and *charm*."

TABLE 5

Modification-Package Choice Point Allocations

Panel A: Mean Modification-Package Choice Point Allocations[a] (Standard Deviation)

Social-Ability Measures	Compensation Condition[b] Fixed-Wage Compensation (n = 27)	Single-Measure Compensation (n = 26)	Multiple-Measure Compensation (n = 26)
Sing	23.7 (6.4)	33.5 (19.2)	22.2 (10.3)
Dance	24.3 (7.6)	18.6 (8.7)	23.0 (13.7)
Charm	32.0 (14.1)	33.5 (20.5)	38.5 (24.8)
Pose	20.4 (7.7)	14.4 (9.0)	16.3 (9.4)

Panel B: Statistical Tests

	Estimate	t	p-value[c]
Points Allocated to *sing*: Fixed-Wage versus Single-Measure Compensation	9.8	2.52	< 0.01
Points Allocated to *sing*, *dance*, and *charm*: Fixed-Wage versus Multiple-Measure Compensation	3.7	1.57	ns

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[a] *Modification-Package Choice Point Allocations* represent participants' allocations of exactly 100 points across the four social-ability measures—*sing*, *dance*, *charm*, and *pose*—when asked to "indicate how much attention you gave to the [*sing*, *dance*, *charm*, and *pose*] abilities, by allocating 100 points among the four abilities, [allocating] more points to abilities to which you gave more attention" when making modification-package selections.

[b] This represents compensation tied directly to social-ability measures of the creatures initially designed by participants. We manipulated this source of compensation at three levels: no social-ability compensation (*fixed-wage-compensation*), performance-based compensation based on a single measure of the creature's social abilities (*single-measure-compensation*), and performance-based compensation based on three of the four measures of the creature's social abilities (*multiple-measure-compensation*). In the fixed-wage-compensation condition, participants received $10 regardless of the state of their creature's social-ability measures. In the single-measure-compensation condition, participants received $2 per level of their creature's *sing* measure (e.g., a level 4 *sing* measure resulted in compensation of $8). In the multiple-measure-compensation condition, participants received $1 per level of their creature's *sing*, *dance*, and *charm* measures (e.g., a creature with *sing* level 3, *dance* level 2, and *charm* level 5 resulted in compensation of $10).

[c] All p-values are presented on a one-tailed basis, except for comparisons involving the fixed-wage-compensation and multiple-measure-compensation conditions, which are presented on a two-tailed basis.

Table 6 presents the results of the additional experiment. As shown in Panel A, participants' mean *comprehensive* surrogation score is 0.61 (n = 24) in the single-measure-compensation condition, but is only 0.46 (n = 26) in the multiple-measure-compensation condition. As we report in Panel B, the difference of 0.15 is statistically significant ($t = 2.31$, $p < 0.01$). These results are robust when considering the *no opportunity cost* surrogation score ($\text{mean}_{\text{single}} = 0.84$; $\text{mean}_{\text{multiple}} = 0.66$; $t = 2.70$, $p < 0.01$). When considering the *opportunity cost* surrogation score, the difference between conditions is marginally significant ($\text{mean}_{\text{single}} = 0.38$; $\text{mean}_{\text{multiple}} = 0.25$; $t = 1.35$, $p = 0.09$). When we drop from our analyses six participants who incorrectly answered the question in the post-experimental questionnaire about the basis of their modification-package-selection

TABLE 6

Additional Experiment

Panel A: Mean Surrogation Scores (Standard Deviation)

	Compensation Condition[a]	
Surrogation Scores	**Single-Measure Compensation (n = 24)**	**Multiple-Measure Compensation (n = 26)**
Surrogation Score: Comprehensive[b]	0.61	0.46
	(0.24)	(0.23)
Surrogation Score: No Opportunity Cost[c]	0.84	0.66
	(0.22)	(0.26)
Surrogation Score: Opportunity Cost[d]	0.38	0.25
	(0.34)	(0.32)

Panel B: Statistical Tests

Single-Measure versus Multiple-Measure	**Estimate**	**t**	**p-value[e]**
Comprehensive	0.15	2.31	0.01
No Opportunity Cost	0.21	2.70	0.01
Opportunity Cost	0.13	1.35	0.09

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[a] This represents compensation tied directly to social-ability measures of the creatures initially designed by participants. We manipulated this source of compensation at two levels: performance-based compensation based on a single measure of the creature's social abilities (*single-measure-compensation*) and performance-based compensation based on three of the four measures of the creature's social abilities (*multiple-measure-compensation*). In the single-measure-compensation condition, participants received $2 per level of their creature's *sing* measure (e.g., a level 4 *sing* measure resulted in compensation of $8). In the multiple-measure-compensation condition, participants received $1 per level of their creature's *sing*, *dance*, and *charm* measures (e.g., a creature with *sing* level 3, *dance* level 2, and *charm* level 5 resulted in compensation of $10).

[b] *Comprehensive* surrogation scores are the percentage of participants' modification-package choices that are consistent with surrogation with *sing* or with *sing*, *dance*, and *charm*, depending on the compensation condition.

[c] *No Opportunity Cost* surrogation scores are the percentage of participants' modification-package choices that are consistent with surrogation with *sing* or with *sing*, *dance*, and *charm*, depending on the compensation condition, when the total of all four social-ability measure levels does not differ between modification packages.

[d] *Opportunity Cost* surrogation scores are the percentage of participants' modification-package choices consistent with surrogation with *sing* or with *sing*, *dance*, and *charm*, depending on the compensation condition, given that such a choice means forgoing a higher total level of the four social-ability measures.

[e] All p-values are presented on a one-tailed basis, given the directional predictions implied by our theory.

compensation, all results are inferentially identical, except that the *opportunity cost* results become more significant ($t = 2.14$, $p = 0.02$). These results support our theory and corroborate the findings from our main experiment.

V. CONCLUSION

This paper investigates the influence of incentive compensation on managers' propensity to use compensated measures as surrogates for strategic constructs. Our findings suggest that compensating managers based on a single measure of a strategic construct increases their propensity to use the compensated measure as a surrogate for the construct. Our results further suggest that this propensity to use compensated measures as surrogates for strategy is less pronounced when compensating managers on multiple measures of a strategic construct. Our paper contributes to a variety of research streams, including literature on firms' use of strategic performance measurement systems. Specifically, we highlight managers' tendency to use measures as surrogates for strategic constructs as a potentially detrimental response to performance measure information. Additionally, our study helps develop academic and practitioner understanding of factors that influence the extent to which firms incur this potential cost. Thus, our study is important to practicing managers and management accountants responsible for developing and implementing strategic performance measurement systems.

Notably, by finding support for both of our hypotheses, we are able to rule out a number of alternative explanations for our findings. For instance, some participants may have been confused by the incomplete compensation contracts, and believed that compensated measures were more important to their creature's ability to socialize. Such a belief would lead participants to make modification-package selections consistent with, but not driven by, surrogation. Another potential alternative explanation for results consistent with H1 is that some participants may have simply tried to make modification-package-pairing selections that were consistent with their behavior in the creature design task (Abelson et al. 1968). Motivated reasoning (Kunda 1990; Tayler 2010; Luft et al. 2011) and other psychology-based theories could also explain H1 results. Participants might identify reasons why surrogation-consistent modification packages, which also happen to be consistent with participants' emphasis when designing their creature, are superior. However, these alternative explanations cannot explain the full pattern of our findings. In particular, because they would predict high surrogation across both incentive compensation conditions, they cannot explain the increased surrogation of the strategic construct when compensation is tied to a single measure, and the *decreased* surrogation of the strategic construct when compensation is tied to multiple measures.

Our paper is subject to multiple limitations. First, while we view the rich, but non-business oriented setting as a strength of our design, the setting raises questions of boundary conditions and generalizability. For instance, we develop and test hypotheses referring to compensation based on a "single measure" or based on "multiple measures." While we operationalize these conditions using specific numbers of measures, we appreciate the continuous nature of the number of possible measures that may be included in a strategic performance measurement system. The number of measures at which surrogation begins to be mitigated is likely to depend on a variety of factors that are beyond the scope of this paper. Finally, while we draw on a vast, well-established literature relating to attention, our supplemental measures of attention are self-reported and potentially very noisy. Although we believe the goals of our study necessitate this approach, future research may benefit from utilizing more precise measures of attention.

While our focus in this paper has been the effects of compensation on strategy surrogation, future research could investigate other institutional factors that inhibit or exacerbate surrogation. Prior research identifies various measure attributes (e.g., congruence, precision, and sensitivity) that affect the optimal weighting of performance measure in the compensation contract. These attributes may also influence managers' propensity to use measures as surrogates for a strategic construct (Feltham and Xie 1994; Krishnan et al. 2005). In addition, managers' involvement in the choice of strategy may influence their propensity toward surrogation. Moreover, the extent to which surrogation is lower under multiple-measure-compensation than under single-measure-compensation may depend on the relationship among the multiple measures (e.g., extent of overlap, causal relationships). Finally, future research could explore specific implications of surrogation, such as how and under what circumstances costs of surrogation arise (e.g., managers' failure to update obsolete measures, sub-optimal strategic decisions), how the representativeness of a given measure influences the cost of surrogation with that measure, and whether strategy surrogation affects managers' evaluations of subordinates. Research addressing such issues would contribute greatly to our understanding of the surrogation phenomenon, as well as the influence of surrogation on the effectiveness of strategic performance measurement systems.

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Negative Special Items and Future Earnings: Expense Transfer or Real Improvements?

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ABSTRACT: Burgstahler et al. (2002) investigate the implications of special items for future earnings and report that firms use negative special items to accelerate the recognition of future expenses into the current period. That is, negative special items serve as an "inter-period transfer" device. We extend their analysis and find that earnings increase in post-special item quarters beyond the four quarters considered in Burgstahler et al. (2002). In particular, we find that future earnings increase over the subsequent 16 quarters by more than 130 percent of the reported negative special item. The earnings increases are greater for restructuring charges than for asset write-downs or goodwill impairment charges. Such patterns suggest that negative special items also signal real future performance improvements (i.e., performance improvement hypothesis) in addition to inter-period expense transfer (i.e., inter-period transfer hypothesis). Moreover, the real improvement effect appears to be driven by restructuring charges, the most prevalent type of special item.

Keywords: *negative special items; earnings management; restructuring.*

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I. INTRODUCTION

Burgstahler et al. (2002; hereafter, BJS) document that negative special items (NSIs) are followed by "earnings of the *opposite sign* in subsequent quarters" (BJS 2002, 587; emphasis in the original).[1] They conjecture that NSIs, on average, represent an "inter-period transfer" of future expenses into current period income. Moreover, their empirical evidence suggests that this inter-period transfer aspect of NSIs is quite substantial, equaling over 8 percent of the original NSI for each of the first three quarters and over 27 percent of the original NSI in the fourth quarter following special item recognition. Cumulatively, their analysis suggests that over 50 percent of an NSI is realized as increased earnings in the four quarters subsequent to recognition.[2] By documenting the future earnings increases associated with NSIs, BJS present compelling evidence that earnings management in the form of inter-period expense transfer is the primary motivating factor for managers in recognizing NSIs.

We address three aspects of the BJS findings to further our understanding of inter-period expense transfer as a basis for interpreting the future earnings implications of NSIs. First, we assess the BJS inter-period transfer hypothesis by considering its implications for changes in earnings for quarters beyond the first four quarters subsequent to the recognition of the NSI. Second, we consider the implication of NSIs for changes in operating cash flows that are not a part of the BJS analyses. The cash flow aspect of our investigation is motivated by Doyle et al. (2003) who report mixed evidence regarding the relation between special items and future cash flows.[3] Third, we disaggregate NSIs into sub-types that have predictable and varying implications for future earnings and cash flows. In particular, we examine the implications for changes in future earnings and operating cash flows for the current recognition of restructuring charges, asset write-downs, and goodwill impairment charges.[4]

While expense transfer certainly constitutes one possible source of the near-term future earnings increases documented in BJS, such increases are also consistent with the notion that the underlying events and actions prompting the recognition of a special item lead to real future performance improvements by the firm. For example, take the case of a firm that undertakes a restructuring of operations (a subclass of NSIs). If the restructuring includes a workforce reduction and plant closing, then the firm may realize future efficiency gains from reduced payroll expenses and operating expenses associated with running the plant. As long as the reduced expenses exceed the decline in revenue from the lost productive capacity, the firm will realize efficiency gains. However, unlike expense transfer driven gains, such real improvement effects need not decline and may well even strengthen in quarters beyond $t+4$ relative to the special item event quarter. By evaluating the implications of NSIs for future earnings and operating cash flows, our analysis allows us to gain insight into the particular roles of expense transfer and real improvements for the future performance consequence of NSIs. Further, by separately examining the implications of restructuring charges, asset write-downs, and goodwill impairment charges, we are able to focus on NSI sub-types that are most likely to be associated with efficiency improvements, expense transfer, and no future effect, respectively.

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[1] BJS extend prior research (e.g., Bernard and Thomas 1990; Ball and Bartov 1996) by documenting that security prices fail to fully reflect predictable elements of the relationship between current and future earnings. The primary focus of this aspect of their study concerns whether the market fully impounds the future income properties of special items. They report that prices better reflect the future implications of special items than the other components of earnings. This finding is consistent with the notion that the implications of transitory special items for future earnings are relatively clear compared to other components of earnings.

[2] The acceleration of future expense as part of a current NSI is simply a timing issue in that future expenses taken now will not be incurred in the future periods resulting in an increase to future net income. For example, an asset write-down recognized as an NSI in the current period represents the acceleration of future depreciation expense. Thus, earnings will be higher in future periods by the amount of the NSI (i.e., the accelerated depreciation).

[3] They report an insignificant relation between special items and operating cash flows, but a significantly positive relation with free cash flow (see Doyle et al. 2003, 160–161, Tables 3 and 4). Their analysis, however, does not partition special items into negative and positive amounts, whereas our entire focus is on the distinct impact of NSIs documented in BJS.

[4] We predict that restructurings will lead to efficiency improvements, asset write-downs are associated with expense transfer, and goodwill impairment charges will not be associated with efficiency gains or expense transfer. We discuss this in more detail later in the text.

An accrual-based income-transfer to *increase* current income borrows against future earnings by either deferring the recognition of expenses or accelerating the recognition of revenue. In contrast, NSI-based expense transfer *decreases* current income, thus transferring earnings to the future by accelerating the recognition of future expense. The result is lower current period earnings and higher future period earnings (BJS). This effect is naturally bounded by the amount of the special item, is inherently non-sustainable in perpetuity, and must at some point dissipate.[5] As time passes and the transfer effect declines, there is a necessary reversal in income *changes* since within each earnings difference, the expense transfer effect in the form of an increase in earnings is greater in the earlier earnings number than in the subsequent earnings number.[6] In this respect, the empirical implications of the expense-transfer hypothesis resemble the well-known future income reversal properties of accruals.[7] Hence, as is true for accrual-based earnings increases, NSI expense transfer will lead to expected income change declines in future periods as the aggregate amount of the transfer is consumed.

Our initial tests of the expense-transfer hypothesis center on determining whether any evidence of these predicted declines in the transfer effect exists in quarters $t+5$ through $t+16$ relative to the NSI event in quarter t. We find no evidence of these expected declines and, in fact, we find considerable evidence supportive of the proposition that NSIs in quarter t imply increases in income in quarters $t+5$ through $t+16$. Similar to the Francis et al. (1996) suggestion that positive price responses to restructuring charge announcements (one type of special item) signal improvements in future performance, we interpret this persistent income effect as evidence that NSIs in part reflect structural improvements in operations/strategy that favorably impact long-term profitability.[8]

Our interpretation of these patterns of post-NSI income increases is conditioned on the assumption that expense transfer effects must decline over time, which is necessarily true given a sufficiently long time horizon. However, it is possible that our horizon of 16 quarters is not sufficiently long enough to capture this necessary decline. Thus, an alternative explanation for our results is that the amount of the expense transfer is increasing over our 16 quarter time horizon rather than declining. For example, a firm that recognizes a $1,000 NSI in quarter t may be transferring future expense of \$100 from year +1, \$200 from year +2, \$300 from year +3 and \$400 from year +1. To address this possibility, we also investigate the relation between quarter *t* NSIs and future changes in operating cash flows. Since cash flows are less susceptible to manipulation (Doyle et al. 2003), NSI-induced increases in operating cash flows are more consistent with efficiency gains than expense transfer.

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[5] BJS report that more than 50 percent of the initial special item is transferred in the form of increased year +1 income; therefore, it follows that either this effect must decline in magnitude or it will naturally be exhausted in year +2.

[6] Figure 2, which we discuss in greater detail later in the text, demonstrates the effect of expense transfer on future earnings under the various expense transfer assumptions (i.e., declining, constant, and increasing) using a simple earnings expectation model.

[7] That is, when a period t accrual manipulation is used to increase period t income, a decline is predicted for income in period $t+1$ relative to period t (Kang and Sivaramakrishnan 1995; Sloan 1996). This decline occurs, in part, because future period income has been borrowed and must eventually be paid back. However, it also occurs because any accrual-driven manipulation of income in period t is inherently transitory. Hence, even if the borrowed earnings are replaced by borrowing again in period $t+1$, the expected income change remains negative unless one assumes that the borrowing can increase indefinitely over time, a notion inconsistent with Barton and Simko (2002).

[8] McVay (2006) documents that earnings are unusually low in special items quarters. Thus, an alternative explanation is that our results simply reflect the mean reversion in earnings over time. However, we do not believe that mean reversion explains these results. As discussed in the "Sensitivity Analyses" section, we specifically address this issue by controlling for prior firm performance. Our conclusions are robust to the inclusion of this additional control.

Overall, we find that NSI-induced earnings increases over the subsequent 16 quarters represent over 130 percent of the original charge.[9] Pure expense transfer naturally cannot exceed 100 percent of the amount of the NSI, so any cumulative effect over 100 percent cannot be the result of expense transfer alone. On average, it seems probable that most NSIs reflect current activity and are of a non-transferable nature (i.e., real costs related to current special item activity or classification shifting).[10] Thus, it is unlikely that inter-period expense transfer can plausibly account for the majority of the NSI-induced future earnings increases we document. For example, if a current NSI of \$1.00 consists of \$0.50 expense transfer, \$0.25 classification shifting, and \$0.25 true NSI current costs, then expense transfer is actually bounded at 50 percent of the NSI charge. Consistent with this conjecture, further empirical evidence suggests that NSIs result in substantial future efficiency gains. Examining the relation between NSIs and future operating cash flows, we find that almost a third of the special items-induced earnings increases are realized in the form of increases in operating cash flow.

We next examine three NSI sub-types, restructuring charges, asset write-downs, and goodwill impairment charges, which we predict as having differing implications for future earnings and operating cash flows. To the extent that NSIs are associated with future efficiency gains, we expect that restructuring charges will most likely lead to real improvements. On the other hand, we expect asset write-downs to be most closely associated with the type of charge that represents an acceleration of future expense (i.e., expense transfer) as an asset write-down is usually the acceleration of depreciation expense. We do not expect goodwill impairment charges to be associated with future earnings or operating cash flows.

Our results are generally consistent with these expectations. We find that restructuring charge-induced earnings (operating cash flow) increases accumulated over the subsequent 16 quarters represent over 500 (150) percent of the original NSI. In contrast, goodwill impairment charges have little empirical relation to future earnings or operating cash flows. With respect to asset write-downs, we find that future earnings (operating cash flow) increases following the asset write-down are less than one-third (half) of the corresponding amount that we find for restructuring charges. Taken together, our evidence suggests that expense transfer provides only a partial explanation for the BJS results. Real improvements in future profitability, especially those related to restructuring charges, appear to be at least as important as the expense transfer effects in explaining the BJS results.

Section II presents the base models for the BJS replication analysis and addresses the predictable future earnings implications of the expense-transfer hypothesis and the real improvements hypothesis. Section III details the sample selection and descriptive statistics. Section IV presents the BJS replication results and the results from further tests of the inter-period expense-transfer hypothesis and the real improvements hypothesis. Section V briefly summarizes and concludes.

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[9] The cumulative effect is subject to measurement error but follows directly from the BJS analysis. We attempt to avoid overstating the effect by measuring pre-tax earnings and special items as well as controlling for serial reporting of special items in sensitivity analyses discussed later.

[10] True current NSI activities can include such items as termination benefits for terminated employees or plant closing costs. Classification shifting occurs when firms report a recurring operating cost as part of an NSI (McVay 2006; Fan et al. 2010).

II. INTER-PERIOD EXPENSE TRANSFER VERSUS REAL IMPROVEMENTS

The BJS analysis focuses on the following equation for $k = 1,\ldots,4$:[11]

$$(E_{i,t+k} - E_{i,t+k-4}) = b_{0k} + b_{1k}NSI_{i,t} + b_{2k}(E_{i,t} - NSI_{i,t} - E_{i,t-4}) + e_{i,t}, \tag{1}$$

where:

$E_{i,t+k}$ = income before extraordinary items (Compustat data item IBQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,4$) divided by the market value of equity (MKVALTQ) in quarter $t-4$; and

$NSI_{i,t}$ = negative special items (SPIQ) for firm i in quarter t multiplied by 1 minus the top federal statutory tax rate divided by the market value of equity (MKVALTQ) in quarter $t-4$.

Equation (1) is based directly on the BJS analysis. The dependent measure in Equation (1) ($E_{i,t+k} - E_{i,t+k-4}$), the seasonal change in earnings for quarters $t+1$, $t+2$, $t+3$, and $t+4$ is incorporated in this model because it provides a way to test the effect of current NSIs and earnings before NSIs ($E_{i,t} - NSI_{i,t} - E_{i,t-4}$) on future earnings. That is, if there is a relationship between future earnings and NSIs, then the dependent measure in Equation (1) should capture that effect. This general notion also holds true for each of the dependent measures incorporated in all of the regression models in this study. BJS report that the coefficients on NSIs are significantly negative at lags 1, 2, and 3, and the coefficient at lag 4 is significantly less than −1. Taken together, BJS report evidence that suggests over 50 percent of the original NSI is recovered through increased earnings in the four subsequent quarters. BJS (2002, 598) conclude that their evidence is consistent with NSIs representing the "current recognition of expenses that would otherwise have been recognized in subsequent quarters, thereby decreasing income in quarter t and increasing income in subsequent quarters." An alternative explanation is that NSIs represent some combination of inter-period expense transfer and operational changes that result in real improvements to future performance.

Future Period Implications of the Expense-Transfer Hypothesis

The initial portion of our analysis addresses the empirical implications of the expense-transfer hypothesis for earnings changes beyond quarter $t+4$. In a simple two-year setting where quarters $t+1$ to $t+4$ correspond to the first four post-special item quarters (i.e., year +1) and quarters $t+5$ to $t+8$ correspond to the fifth through eighth post-special item quarters (i.e., year +2), expense transfer over this two-year period must take on one of three distinct forms: (1) *declining*—the earnings increase from expense transfer in year +2 is less than year +1; (2) *constant*— the earnings increase from expense transfer in year +2 is equal to year +1; or (3) *increasing*— the earnings increase from expense transfer in year +2 is greater than year +1. The specific implications for the NSI coefficient in Equation (1) of these transfer effects depends on two factors: (1) whether the lag between the independent and dependent earnings change is four quarters or fewer than four quarters (i.e., an interim quarter lag such as when the change in quarter t earnings is explained by the change in quarter $t-1$ earnings); and (2) whether the independent earnings change measure "straddles"[12] the *NSI* event quarter t (e.g., $E_{t+3} - E_{t-1}$) or reflects changes entirely based on post-special-items-event earnings (e.g., $E_{t+5} - E_{t+1}$).

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[11] This equation is a modified version of that in BJS. BJS focus on both positive and negative special items. We report descriptive statistics for all observations, but our main analyses focus solely on NSIs as positive special items are mainly transitory and do not exhibit the reversal properties of interest in this study or that of BJS. Results of all tests with positive special items are available on request.

[12] That is, the later earnings number in the change is for a post-special-item quarter while the earlier earnings number is for a pre-special-items quarter. Earnings for quarter $t+2$ less earnings for quarter $t-2$, where quarter t is the special item event quarter is an example of a straddle.

If the expense transfer is declining, then the transfer effect increases both components of post-special-items event dependent earnings changes (i.e., E_{t+k} and E_{t+k-4}). However, the transfer amount is greater in the earlier (E_{t+k-4}) earnings number than the later (E_{t+k}) earnings number. Earnings differences that do not straddle the special item quarter will be negative since the later earnings number has less transfer than the earlier earnings number (i.e., we expect that $E_{t+k} < E_{t+k-4}$). Thus, the declining transfer assumption predicts positive coefficients on *NSI* in Equation (1) beyond the straddle period. If the expense transfer is increasing, then earnings differences will be positive since the transfer amount is greater in the later (E_{t+k}) earnings number than the earlier (E_{t+k-4}) earnings number. Thus, the increasing transfer assumption predicts negative coefficients on *NSI* in Equation (1) for all periods. Beyond the straddle period, the constant transfer assumption predicts zero coefficients on *NSI* as the early and late transfer effects are equal in amount.[13]

Figure 1 summarizes the *NSI* coefficient predictions under the declining, constant, or increasing transfer patterns and Figure 2 illustrates the effect of each transfer pattern assumption for a simple earnings expectations model. Figure 2 shows that unless the transfer amounts are very small, the transfer will become exhausted very rapidly as it is necessarily bounded by the amount of the charge.[14]

Implications of the Real Improvements Hypothesis

While evidence of NSI-induced earnings increases beyond quarter $t+4$ is consistent with *increasing* special items expense transfer, it is also consistent with a very different perspective of the post-NSI income increases reported in BJS. Specifically, such increases potentially reflect the realization of future efficiency, operating, or strategic improvements that are either consequences of, or signaled by, NSIs. Moreover, it is possible for the income effects of such improvements to increase over time. Thus, while a pattern of steadily increasing income could be explained as an increasing expense transfer pattern, real improvements appear to provide a more plausible explanation. Conversely, a pattern of decreasing future income would appear to be better explained by declining expense transfer than by real changes in performance.

Additional Tests of Expense Transfer versus Real Improvements

Because future income patterns cannot completely resolve the question of whether and to what extent NSIs represent expense transfer versus real improvements, we conduct three additional empirical analyses to examine this issue further. We examine (1) the total earnings increases over the subsequent 16 quarters related to NSIs; (2) the extent to which NSIs influence future changes in operating cash flows; and (3) the total earnings and operating cash flow increases over the subsequent 16 quarters related to restructuring charges, asset write-downs, and goodwill impairment charges.

Given that NSI-induced expense transfer is bounded by the amount of the NSI, evidence that the total earnings increase related to NSIs exceeds 100 percent of the original NSI would suggest that NSIs lead to some level of real improvements. Hence, we estimate the cumulative transfer effect as reflected in earnings changes over the four years following the special item and compare this total to the 100 percent baseline. While the cumulative effect evaluation appears to address a

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[13] Prior drafts of this manuscript include an appendix containing a rigorous evaluation of all possible coefficient predictions under each of the transfer assumptions. This appendix, omitted for space constraints, is available from the authors upon request.

[14] For example, if a depreciable asset is written down and the asset has a remaining useful life of 15 years, the future depreciation transferred into the current NSI (i.e., the amount of the write-down) will not be completely exhausted until the asset is fully depreciated (i.e., 15 years from the current period).

FIGURE 1
Summary Predictions for Sign of Negative Special Items Coefficients in Future Earnings Regressions beyond Quarter *t*+4

	Inter-Period Expense-Transfer Hypothesis					
	Straddle Explanatory Earnings Changes[a]			**Post-Special-Item Period Explanatory Earnings Change**[b]		
Earnings Lag	**Declining**[c]	**Constant**[d]	**Increasing**[e]	**Declining**[c]	**Constant**[d]	**Increasing**[e]
Lag 4	?	–	–	+	0	–
Lag 1,2, & 3	+	+	–	+	0	–

All predictions in Figure 1 are with respect to the coefficient b_1 in the following equation:

$$(PTI_{i,t+k} - PTI_{i,t+k-4}) = b_{0(t+k,l)} + b_{1(t+k,l)}RNSI_{i,t} + b_{2(t+k,l)}(PTI_{t+k-l} - PTI_{t+k-l-4}) + e_{i,(t+k,l)},\ t+k > t+4.$$

$PTI_{i,t+k}$ is pre-tax income for firm *i* in quarter *t*+*k* divided by the market value of equity in quarter *t*–4; *l* is an index variable that equals 1, 2, 3, or 4 and reflects the lag in quarters between the dependent and independent earnings changes.

$RNSI_t$ is reported negative special items recognized in quarter *t* divided by market value of equity in quarter *t*–4.

[a] A "straddle" earnings change quarter occurs when the later earnings number in the earnings change is for a post-special-item quarter while the earlier earnings number is for a pre-special-items quarter.

[b] A post-special-items change quarter occurs when both earnings measures are values that occur after the NSI quarter.

[c] For a "declining" special items transfer, the amount of future income recognized as a negative special item in quarter *t* declines, on average, in (seasonal) amount over the time horizon examined. That is, the special item transfer benefits earlier future period income more than later future period income.

[d] A "constant" special items transfer is constant in (seasonal) amount in each quarter over the time horizon examined.

[e] An "increasing" special items transfer increases in (seasonal) amount over the time horizon examined.

sufficient condition for the presence of at least some level of real improvement in the post-special-item earnings changes, there are two issues with respect to this analysis. First, this analysis provides only a floor for measuring the extent of NSI-induced real improvements (i.e., the degree to which earnings increases exceed 100 percent of the NSI). Second, our measure of NSI-induced earnings increases is subject to measurement error. To address both of these potential design limitations, we investigate an additional measure that is more amenable to such inferences: changes in future operating cash flows. Operating cash flows are less susceptible to manipulation by management and, thus, NSI-induced increases in operating cash flows are more likely a result of efficiency gains than expense transfer.

Finally, special items are quite heterogeneous and various sub-types of special items have differing implications for future earnings and operating cash flows. Operational restructurings are generally undertaken to create future efficiency gains (Brickley and Van Drunen 1990; Atiase et al. 2004; Clement et al. 2007). However, prior research and anecdotal evidence also suggests that restructuring charges and related reversals may be associated with earnings management activity

FIGURE 2

Effects of Declining, Constant, and Increasing Transfer Assumptions assuming a $1.00 Negative Special Item in Quarter t[a]

Realized Earnings						Expected Earnings											
	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4	t+5	t+6	t+7	t+8	t+9	t+10	t+11	t+12
Quarterly Earnings	10.00	10.00	10.00	10.00													
Quarter t expected earnings before innovation					10.00												
Quarter t innovation (NSI_t)[b]					(1.00)												
Quarter t total earnings including innovation					9.00												
a) Earnings Expectations without quarter t innovation						10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00
b) **Declining Transfer Assumption**—transfer 0.15 first 4 quarters and 0.10 next 4 quarters for a total transfer of $1.00.						0.15	0.15	0.15	0.15	0.10	0.10	0.10	0.10	0.00	0.00	0.00	0.00
Expected total earnings (a) + (b)						10.15	10.15	10.15	10.15	10.10	10.10	10.10	10.10	10.00	10.00	10.00	10.00
Expected Seasonally Differenced Earnings ($E_{t+k} - E_{t+k-4}$)[c]						0.15	0.15	0.15	1.15	(0.05)	(0.05)	(0.05)	(0.05)	(0.10)	(0.10)	(0.10)	(0.10)
c) **Constant Transfer Assumption**—transfer 0.20 for first 5 quarters for a total transfer of $1.00.						0.20	0.20	0.20	0.20	0.20	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Expected total earnings (a) + (c)						10.20	10.20	10.20	10.20	10.20	10.00	10.00	10.00	10.00	10.00	10.00	10.00
Expected Seasonally Differenced Earnings ($E_{t+k} - E_{t+k-4}$)						0.20	0.20	0.20	1.20	0.00	(0.20)	(0.20)	(0.20)	(0.20)	0.00	0.00	0.00
d) **Increasing Transfer Assumption**—transfer 0.10 for first 4 quarters and 0.15 next 4 quarters for a total transfer of $1.00.						0.10	0.10	0.10	0.10	0.15	0.15	0.15	0.15	0.00	0.00	0.00	0.00
Expected total earnings (a) + (c)						10.10	10.10	10.10	10.10	10.15	10.15	10.15	10.15	10.00	10.00	10.00	10.00
Expected Seasonally Differenced Earnings ($E_{t+k} - E_{t+k-4}$)						0.10	0.10	0.10	1.10	0.05	0.05	0.05	0.05	(0.15)	(0.15)	(0.15)	(0.15)

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[a] The "declining transfer assumption" is consistent with a depreciable asset write-down where the asset is depreciated under the accelerated method of depreciation. The "constant transfer assumption" is consistent with a depreciable asset write-down where the asset is depreciated under the straight-line method of depreciation. We have no clear example of an NSI that fits the "increasing transfer assumption." That said, the "increasing transfer assumption" implies more expense is transferred from later in time quarters than quarters immediately adjacent to the NSI quarter.

[b] The quarter t innovation is the reported NSI.

[c] For example, under the declining transfer assumption the expected seasonally differenced earnings for quarter t+4 of $1.15 is the $10.15 earnings for quarter t+4 less the $9.00 earnings for quarter t.

(e.g., Smith and Lipin 1996; Levitt 1998; Moehrle 2002; Bens and Johnson 2009). Thus, changes in earnings induced by restructuring charges may potentially reflect real improvements or expense transfer. On the other hand, future changes in cash flows that result from restructuring activity will more likely be associated with real improvements since cash flows are less susceptible to intertemporal transfer.

Statement of Financial Accounting Standards 121 (SFAS 121), *Accounting for the Impairment of Long-Lived Assets*, indicates that an asset impairment exists if the asset's recoverable cost is lower than the book value of the asset. In this case, a firm must write the asset down to its fair market value. When a long-lived asset is written down to fair market value, the accounting effect of this event is to transfer future depreciation expense into to the current period as a special item. Unless an asset write-down is taken in conjunction with some other charge or event, the only effect a write-down can have is to transfer depreciation expense from the future into the current period. Consistent with other concurrent events being recognized with asset write-downs, Rees et al. (1996) report that firms with asset write-downs also record significant unexpected accruals in the year of the write-down. However, they also find that these unexpected accruals do not reverse in subsequent periods. Thus, our expectation is that when the NSI is an asset write-down, the most likely result will be pure NSI expense transfer.

The Financial Accounting Standards Board adoption of Statement of Financial Accounting Standards 142 (SFAS 142), *Goodwill and Other Intangible Assets*, in June of 2001 fundamentally changed the accounting for goodwill. Prior to the adoption of SFAS 142, goodwill was recognized as an asset and amortized over a period of not more than 40 years. However, SFAS 142 eliminated goodwill amortization. Under current rules, goodwill impairment is merely the write-off of the value of goodwill when its fair value is determined to be less than fair market value.

Since goodwill is not amortized, an impairment of goodwill does not accelerate future expense recognition. However, goodwill impairment implies lower future cash flows that necessitate recognition of the goodwill impairment charge. Thus, our expectation is that goodwill impairment charges will have little relation to future earnings, but may be associated with lower future cash flows. However, if goodwill impairment signals a change in the firm's future operational or strategic direction that will affect the value of goodwill, the charge may be associated with future performance improvements. Taken together, the expectation is that there will be no future earnings and lower cash flow effects from the impairment of goodwill. By investigating the relationship of the cumulative restructuring charge, asset write-down, and goodwill impairment effects on future earnings and operating cash flows, we can assess the extent of expense transfer versus real improvements via these special item sub-types that we predict will have real improvement, expense transfer, and zero transfer implications, respectively.

III. SAMPLE SELECTION AND DESCRIPTIVE STATISTICS

Our sample consists of quarterly earnings data from 2002–2009 drawn from Compustat quarterly data files. We eliminate observations using the same criteria as that applied by BJS (2002, 594).[15] We begin with a sample of 260,874 quarterly observations with non-missing earnings before extraordinary items (IBQ) and non-missing special items (SPIQ). We eliminate quarterly observations missing market value of equity (MKVALTQ) at quarter t–4. In addition, we eliminate observations (1) that do not have a minimum of nine contiguous quarters of earnings surrounding quarter t; and (2) where the absolute value of earnings for quarters t–4 to t+4 exceeds the market value of equity at t–4.[16] Our data requirements and sample selection criteria, summarized in Panel A of Table 1, result in a test sample of 163,628 firm-quarter observations (9,234 firms).

Panel B of Table 1 reports the frequencies of special items observations by quarter for the final sample of 163,628 observations. The data in this table show, consistent with BJS, a larger proportion of negative to positive special items (36,609 versus 13,252). We also find that the reporting of special items is relatively stable across the interim quarters (1–3); however, in the fourth quarter, the percentage of non-zero special items increases significantly relative to the interim quarters. In the fourth quarter, 30.9 and 10.2 percent of all observations report negative and positive special items, respectively. In the interim quarters, the average percentages are 19.8 and 7.5 percent, respectively. Panel B of Table 1 also reports the mean pre-tax special item scaled by the quarter t–4 market value of equity. Positive special items remain relatively stable across all quarters, but NSIs appear to be larger in the fourth quarter relative to interim quarters. In the fourth quarter, the mean scaled *NSI* is –0.036, while it ranges from –0.015 to –0.022 for the three interim quarters. Overall, these sample descriptive statistics are very similar to those reported by BJS.

Panel C of Table 1 reports mean market value of equity, pre-tax special items, pre-tax income, and operating cash flows. We report pre-tax income and operating cash flows for the year prior to the special item recognition as well as the four years subsequent to the special item recognition. Our evidence suggests that special item firms are larger than firms that do not recognize special items.

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[15] We incorporate the same sample selection criteria as BJS except their elimination of immaterial (less than 1 percent of market value) NSIs. For robustness, we rerun all of our tests excluding immaterial NSIs. All results are quantitatively and qualitatively unchanged.

[16] We apply these criteria to both income before extraordinary items and pre-tax income. BJS include only income before extraordinary items in their study.

TABLE 1

Sample Selection Criteria and Descriptive Statistics

Panel A: Sample Selection Criteria

	Total	*SI* Non-Zero[a]	Percent $SI < 0$[b]	Percent $SI > 0$[c]
Quarterly observations of E_t and SI_t for sample period 2002–2009	260,874	80,965	73.84	26.16
1. Market value unavailable for $t-4$	43,090			
	217,784	70,645	73.52	26.48
2. One or more contiguous E_{t-4} to E_{t+4} missing	44,094			
	173,690	53,757	72.95	27.05
3. One or more contiguous PTI_{t-4} to PTI_{t+4} missing	1,176			
	172,514	53,319	72.97	27.03
4. Less: Scaled absolute value of E_{t-4} to E_{t+4} and PTI_{t-4} to PTI_{t+4} greater than 1	8,886			
Final Sample	163,628	49,861	73.42	26.58

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[a] The number of quarterly special items reported by Compustat that are non-zero.

[b] The number of quarterly special items reported by Compustat that are less than zero divided by the total number of non-zero special items.

[c] The number of quarterly special items reported by Compustat that are greater than zero divided by the total number of non-zero special items.

Panel B: Final Sample Observations and Mean Positive and Negative Special Items by Fiscal Quarter

	Total		$SI = 0$		$SI > 0$			$SI < 0$		
Quarter	n	Percent of All Qtrs.	n	Percent of Row	n	Percent of Row	Mean Pre-Tax *SI*[a]	n	Percent of Row	Mean Pre-Tax *SI*[a]
1	44,441	27.16	33,481	75.34	3,183	7.16	0.022	7,777	17.50	−0.015
2	40,475	24.70	29,096	71.89	3,116	7.70	0.020	8,263	20.42	−0.020
3	39,263	23.98	27,931	71.14	2,950	7.51	0.021	8,382	21.35	−0.022
4	39,449	24.39	23,259	58.96	4,003	10.15	0.021	12,187	30.89	−0.036
Total	163,628	100.0%	113,767	69.53	13,252	8.10	0.021	36,609	22.37	−0.025

(continued on next page)

TABLE 1 (continued)

Panel C: Mean Annual Future Earnings, Cash Flows, Market Value of Equity, and Special Items[b]

			$\Sigma 4QPTI_{i,y}$					$\Sigma 4QCFO_{i,y}$				
	$MVAL_{i,t-4}$	Pre-Tax $SI_{i,t}$[a]	$y = -1$	$y = 1$	$y = 2$	$y = 3$	$y = 4$	$y = -1$	$y = 1$	$y = 2$	$y = 3$	$y = 4$
All Observations	2,997.25	—	−0.013	0.002	0.022	0.036	0.045	0.057	0.069	0.086	0.107	0.132
n	156,448		156,448	156,448	123,567	96,115	72,448	147,256	147,256	119,823	94,170	70,914
$SI = 0$[c]	2,045.37	—	−0.004	0.008	0.025	0.037	0.045	0.063	0.065	0.082	0.103	0.128
n	108,185		108,185	108,185	86,657	67,861	51,468	100,861	100,861	83,079	66,294	50,239
$SI < 0$[c]	5,378.50	−0.023	−0.036	−0.014	0.014	0.034	0.049	0.054	0.078	0.096	0.118	0.144
n	35,553	35,553	35,553	35,553	29,991	20,522	15,269	34,430	34,430	26,503	20,252	15,043
$SI > 0$[c]	4,438.48	0.019	0.035	−0.002	0.020	0.029	0.039	0.060	0.075	0.092	0.111	0.132
n	12,710	12,710	12,710	12,710	9,919	7,732	5,711	12,235	12,235	9,701	7,624	5,132

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[a] The mean pre-tax SI reported here is calculated as the reported special item in quarter t scaled by the market value of equity in quarter t–4.

[b] The observations for this table are further reduced from Panel B by absolute values of PTI_{t+k} and CFO_{t+k}, where $k = 1$ through 16, greater than 1.

[c] Reported special items in quarter t are equal to, less than, or greater than zero, respectively.

Variable Definitions:
$MVAL_{i,t-4}$ = market value of equity (Compustat data item MVALTQ) for firm i in quarter t–4;
$PTI_{i,t+k}$ = pre-tax income (PIQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,16$) divided by the market value of equity (MKVALTQ) in quarter t–4;
$CFO_{i,t+k}$ = cash flow from operating activities (OANCFQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,16$) divided by the market value of equity (MKVALTQ) in quarter t–4;
$\Sigma 4QPTI_{i,y} = (PTI_{i,t+k} - PTI_{i,t+k-4})$ summed for the four quarters where k equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$; and
$\Sigma 4QCFO_{i,y} = (CFO_{i,t+k} - CFO_{i,t+k-4})$ summed for the four quarters where k equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$.

We find that the mean market value of equity for negative and positive special item firms is \$5.4 and \$4.4 billion, respectively. Both are much larger than the mean market value of equity for zero-special item firms of \$2.0 billion.

On average, sample firms report increasing pre-tax income and operating cash flows as indicated by the generally positive values on $\Sigma 4QPTI_{i,y}$ and $\Sigma 4QCFO_{i,y}$. $\Sigma 4QPTI_{i,y}$ and $\Sigma 4QCFO_{i,y}$ are year-by-year, four-quarter summations of the seasonal differences in earnings and operating cash flows, respectively. However, it appears that the increases in $\Sigma 4QPTI_{i,y}$ and $\Sigma 4QCFO_{i,y}$ are most pronounced for NSI firms. In particular, we find that $\Sigma 4QPTI_{i,y}$ ($\Sigma 4QCFO_{i,y}$) increases for NSI firms from –0.036 (0.054) in the year prior to recognition to 0.049 (0.144) in the fourth year subsequent to the special item quarter. For zero special items firms, we find that $\Sigma 4QPTI_{i,y}$ ($\Sigma 4QCFO_{i,y}$) increases from –0.004 (0.063) in the year prior to the current quarter to 0.045 (0.128) in the fourth year subsequent to the current quarter. Overall, we find that the change in $\Sigma 4QPTI_{i,y}$ ($\Sigma 4QCFO_{i,y}$) for negative, positive, and zero special item firms from year -1 to year +4 is 0.085 (0.090), 0.004 (0.072), and 0.049 (0.065), respectively. Thus, our descriptive evidence suggests that NSI firms, on average, show improvements in performance substantially greater than positive or zero special item firms in the four years subsequent to recognition.

IV. EMPIRICAL RESULTS

Replication of BJS

We begin our analysis with a replication of the BJS earnings persistence regressions, estimating the coefficient relating seasonally differenced earnings to subsequent values of seasonally differenced earnings. Since the time period of our analysis differs, we replicate the BJS analysis to investigate whether the main results from their study still hold. Consistent with BJS, we estimate Equation (1) separately for quarters with positive, negative, and zero special items. However, since the focus of our investigation is NSIs, for brevity we only report the estimations for NSIs.[17]

The results from the estimation of Equation (1) are reported in Table 2. We report our results for the estimation of Equation (1) (Current Sample) as well as those reported by BJS (2002, 596, Table 2, Panel B). We find that the coefficients at all lags are negative and significant (two-tailed $p < 0.01$).[18] Moreover, the crucial fourth lag coefficient is -1.342 and significantly less than -1 (two-tailed $p < 0.01$). Overall, the results from the estimation of Equation (1) closely parallel those reported by BJS both quantitatively and qualitatively. Moreover, our Equation (1) results suggest an expense transfer of approximately 63 percent, which is consistent with the 56 percent transfer effect reported by BJS.[19]

BJS Replication with Pre-Tax Special Items

BJS adjust special items for taxes in order to enhance comparability to the after tax income numbers that serve as the foundation of Equation (1). The tax rate used to adjust special items in

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[17] Our results with respect to positive and zero special item firms are quantitatively and qualitatively similar to those tabulated in BJS. These untabulated results are available from the authors on request.

[18] In all regression tests, the t-statistics we rely on are based on robust-standard errors using two-way clustering by firm and year to control for both time-series and cross-sectional dependence (Gow et al. 2010). We thank Ian Gow for providing us with the SAS code to perform the two-way clustering procedure.

[19] To arrive at the 63 percent expense transfer the b_{1k} coefficients are summed for $k = 1, 2, 3$, and 4 then 1 is added to this sum. Thus, we add $(-0.074) + (-0.102) + (-0.111) + (-1.342) + 1$ for a total of -0.629. Since *NSI* is a negative value in Equation (1), the summed negative coefficients imply an increase to earnings. The 56 percent for the BJS estimates is arrived at similarly.

TABLE 2

Relation between NSIs and Seasonally Adjusted Future Earnings (BJS Replication) Estimated b_{1k} and b_{2k} from Equation (1):

$$(E_{i,t+k} - E_{i,t+k-4}) = b_{0k} + b_{1k}NSI_{i,t} + b_{2k}(E_{i,t} - NSI_{i,t} - E_{t-4}) + e_{i,t}$$

	$k = 1$	$k = 2$	$k = 3$	$k = 4$	n
Estimates of b_{1k}					
Reported in BJS[a]	−0.087**	−0.116**	−0.084**	−1.277**++	4,885
Current Sample	−0.074**	−0.102**	−0.111**	−1.342**++	36,609
Estimates of b_{2k}					
Reported in BJS[a]	0.178**	0.108**	0.032*	−0.316**	4,885
Current Sample	0.211**	0.122**	0.053**	−0.213**	36,609

*, ** Significantly different from zero at the 0.05 and 0.01 levels, respectively, in a two-tailed test.
++ Significantly different from −1 at the 0.01 level in a two-tailed test (applies only for estimates of b_1 at $k = 4$).
[a] The significance of the BJS estimates are taken from BJS (2002, Table 2, p. 596).

Variable Definitions:
$E_{i,t+k}$ = income before extraordinary items (IBQ) for firm i in quarter $t+k$ (where $k = 1, 2, 3$, or 4) divided by the market value of equity (MKVALTQ) in quarter t–4; and
$NSI_{i,t}$ = negative special items (SPIQ) for firm i in quarter t multiplied by 1 minus the statutory tax rate (35 percent) divided by the market value of equity (MKTVALTQ) in quarter t–4.

their Table 2 (p. 596) analyses is the top federal statutory rate applicable for each year of their sample. However, Beaver et al. (2006) report evidence that suggests that average effective tax rates for NSI observations are substantially less than the top statutory rate.[20] Consistent with Beaver et al. (2006), in additional testing (not tabulated), we estimate that effective tax rates for NSIs range between 3 and 14 percent. Since firm- and quarter-specific values of the true special items tax rates are unobservable, we eliminate the assumed tax rate effect on parameter estimates by performing all subsequent analyses on pre-tax income and special items.

In this section, we evaluate how using pre-tax income and NSIs affects the BJS inferences about the association between NSIs and future income changes by estimating a pre-tax version of the BJS model. In particular, we modify Equation (1) by replacing earnings before extraordinary items (*E*) with pre-tax income (*PTI*) and tax-adjusted negative special items (*NSI*) with reported (unadjusted) negative special items (*RNSI*). Specifically, we estimate the following equation for $k = 1,\ldots,4$:

$$(PTI_{i,t+k} - PTI_{i,t+k-4}) = b_{0k} + b_{1k}RNSI_{i,t} + b_{2k}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}, \tag{2}$$

where:

$PTI_{i,t+k}$ = pre-tax income (PIQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,$or 4) divided by the market value of equity (MKVALTQ) in quarter t–4; and

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[20] The lower tax rate for NSI firms documented by Beaver et al. (2006) reflects the fact that these firms are frequently loss firms as well. Beaver et al. (2006) report that in their sample, loss observations have average ETRs of 7 percent, while profit observations have average ETRs of 33.2 percent. They further report that 37 (20) percent of loss (profit) firms report an NSI.

$RNSI_{i,t}$ = pre-tax negative special items (SPIQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter t–4.

We report the results of estimating Equation (2) in Table 3. For comparative purposes, we report the results from estimating Equation (1), the after-tax BJS model replication, in Table 3 as well. Based on the b_{1k} *RNSI* coefficient estimates reported in Table 3, the aggregate future period pre-tax income effect for the first four quarters following the recognition of the NSI is approximately 7.8 percent of the reported pre-tax negative special item (*RNSI*). This aggregate effect is markedly less than the 62.9 percent aggregate effect per our Table 2 estimates and the 56.4 percent effect obtained by aggregating the values reported in BJS (2002, 596, Table 2).

Particularly striking is the increase in the crucial $k = 4$ lag from -1.342 to -0.997. This increase is consistent with a true special items tax rate that is less than the top statutory tax rate used to adjust special items in Equation (1). Since the tax rate impact on coefficient estimates is of a scalar nature (Cready et al. 2011), the degree of coefficient shift increases directly with the magnitude of the underlying coefficient that is being scaled. Thus, it is not surprising that the coefficient shift is greatest for the fourth lag since the fourth quarter lag coefficient is greater in absolute value relative to the interim lags.[21] Overall, the evidence reported in Table 3 strongly supports the notion that the use of the top statutory tax rates to adjust special items introduces substantial bias into *NSI* coefficient estimates from Equation (1), consistent with the conclusions of Cready et al. (2011).[22]

Negative Special Items and Future Period Earnings Changes

Table 4 reports tests of our predictions about the relation between NSIs and future period earnings changes beyond quarter t+4. Specifically, we report the results of estimating the following regression:

$$(PTI_{i,t+k} - PTI_{i,t+k-4}) = b_{0k} + b_{1k}RNSI_{i,t} + b_{2k}(PTI_{i,t+k-l} - PTI_{i,t+k-l-4}) + e_{i,t}. \quad (3)$$

All variables are as previously defined. Table 4, Panel A reports *RNSI* coefficient estimates for those settings where the explanatory earnings change "straddles" the special item quarter. A straddle occurs when the later earnings number in the earnings change is for a post-special-item quarter, while the earlier earnings number is for a pre-special-items quarter. Earnings for quarter t+2 less earnings for quarter t–2, where quarter t is the special item event quarter, is an example of a straddle. We report these *RNSI* straddle coefficient estimates separately since, as reported in Figure 1, predictions for such straddle quarters differ somewhat from the predictions for post-special-item period explanatory earnings changes. We find negative and significant (two-tailed $p < 0.01$) *RNSI* coefficients for all ten of the straddle estimates we report in Panel A. The coefficients range in magnitude from -0.051 to -0.110. The negative coefficients for the fourth lag estimates are consistent with either increasing or constant expense transfer. On the other hand, the interim lag coefficient estimates are only consistent with increasing expense transfer.

Panel B of Table 4 reports *RNSI* coefficient estimates for those settings where the independent explanatory earnings changes are based exclusively on post-special items earnings (e.g., $PTI_{i,t+6}$ −

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[21] For a detailed evaluation of the empirical impact of misstated tax rates on the interim and fourth lag coefficient estimates, see Cready et al. (2011).

[22] In addition to using pre-tax special items to estimate our models, we also re-estimate each of our models using special items adjusted for the top statutory rate and the firm-specific marginal tax rate. We adjust at the firm-specific marginal rate by adjusting special items for firm-specific annual marginal tax rates provided by Graham (1996a, 1996b). The results of these untabulated additional tests are quantitatively and qualitatively similar to our tabulated results using pre-tax special items. We thank John Graham for making the firm-specific marginal tax rate data available to us.

TABLE 3

Relation between Pre-Tax NSIs and Seasonally Adjusted Pre-Tax Future Earnings Estimated b_{1k} and b_{2k} from Equation (2):

$$(PTI_{i,t+k} - PTI_{i,t+k-4}) = b_{0k} + b_{1k}RNSI_{i,t} + b_{2k}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

	$k = 1$	$k = 2$	$k = 3$	$k = 4$	n
Estimates of b_{1k}					
Reported in Table 2	−0.074**	−0.102**	−0.111**	−1.342**++	36,609
Current Estimation	0.013	−0.036**	−0.058**	−0.997**	36,609
Estimates of b_{2k}					
Reported in Table 2	0.211**	0.122**	0.053**	−0.213**	36,609
Current Estimation	0.247**	0.149**	0.062**	−0.188**	36,609

*, ** Significantly different from zero at the 0.05 and 0.01 levels, respectively, in a two-tailed test.
++ Significantly different from −1 at the 0.01 level in a two-tailed test (applies only for estimates of b_1 at $k = 4$).

Variable Definitions:
$PTI_{i,t+k}$ = pre-tax income (PIQ) for firm *i* in quarter *t*+*k* (where $k = 1,\ldots,$or 4) divided by the market value of equity (MKVALTQ) in quarter *t*–4; and
$RNSI_{i,t}$ = pre-tax negative special items (SPIQ) for firm *i* in quarter *t* divided by the market value of equity (MKVALTQ) in quarter *t*–4.

$PTI_{i,t+2}$). The expense transfer predictions on the *RNSI* coefficients are consistent for interim and fourth lag coefficients within each form of expense transfer. That is, the declining, constant, and increasing forms of expense transfer predict positive, zero, and negative *RNSI* coefficients for both interim and fourth lag estimates, respectively. Panel B reports 38 *RNSI* coefficient estimates for quarters *t*+6 through *t*+16. Of the 38 coefficients estimates, 36 are negative and 29 of these are significant (two-tailed $p < 0.05$). Our coefficient predictions summarized in Figure 1 suggest that negative coefficients in post-special-item period explanatory quarters are indicative of an NSI in quarter *t* being associated with an increasing pattern of expense transfer beyond quarter *t*+4.

Taken together, the evidence in Table 4 is consistent only with the increasing form of expense transfer. We find no evidence of zero or positive coefficients after quarter *t*+4 that would suggest a constant or declining form of expense transfer. However, it is conceivable that NSIs contain larger transfers of year +2 (i.e., quarters *t*+5 to *t*+8) expenses relative to year +1 (i.e., quarters *t*+1 to *t*+4) expenses. Such an increasing pattern of expense transfer could potentially explain the significantly negative *RNSI* coefficients in quarters *t*+5 through *t*+8. At the same time, if the increases to earnings truly reflect expense transfer, evidence of necessary reversals should be present by year +3 or +4 (i.e., quarters *t*+9 through *t*+12 and *t*+13 through *t*+16). Our results do not reflect such reversals in quarters *t*+9 through *t*+16, which would imply positive coefficient estimates. Of the 32 *RNSI* coefficient estimates we report in bold in Panel B of Table 4 for the quarters *t*+9 through *t*+16, all but one are negative and 23 of these are significantly less than zero (two-tailed $p < 0.05$).

The evidence in Table 4 is inconsistent with the declining or constant assumptions of the expense-transfer hypothesis. However, these results can be interpreted as providing support for the expense-transfer hypothesis under the increasing transfer assumption. This interpretation requires the seemingly unlikely assumption that the typical or average NSI consists of expense transfers that are monotonically declining from year +4 back through year +1. That is, expenses further in the future are transferred into current NSIs to a greater extent than near-term expenses. Once again, it

TABLE 4

Relationship between Negative Special Items and Seasonally Adjusted Extended Future Earnings for Quarters t+5 through t+16
Pre-Tax Estimate of b_{1k} estimated from Equation (3):

$$(PTI_{i,t+k} - PTI_{i,t+k-4}) = b_{0k} + b_{1k}RNSI_{i,t} + b_{2k}(PTI_{i,t+k-l} - PTI_{i,t+k-l-4}) + e_{i,t}$$

Panel A: Estimated Coefficients for Straddle Explanatory Quarters[a]

	Dependent Earnings Change Quarter			
Independent Earnings Change	**One Quarter Ahead**	**Two Quarters Ahead**	**Three Quarters Ahead**	**Four Quarters Ahead**
$PTI_{i,t+1} - PTI_{i,t-3}$	—	—	—	−0.106**
$PTI_{i,t+2} - PTI_{i,t-2}$	—	—	−0.105**	−0.088**
$PTI_{i,t+3} - PTI_{i,t-1}$	—	−0.095**	−0.051**	−0.110**
$PTI_{i,t+4} - PTI_{i,t}$	−0.109**	−0.058**	−0.052**	−0.053**

Panel B: Estimated Coefficients for Post-*RNSI* Explanatory Quarters[b]

	Dependent Earnings Change Quarter[c]			
Independent Earnings Change	**One Quarter Ahead**	**Two Quarters Ahead**	**Three Quarters Ahead**	**Four Quarters Ahead**
$PTI_{i,t+5} - PTI_{i,t+1}$	0.035**	−0.040**	−0.059**	**−0.079****
$PTI_{i,t+6} - PTI_{i,t+2}$	−0.043**	−0.060**	**−0.024**	**−0.045****
$PTI_{i,t+7} - PTI_{i,t+3}$	−0.053**	**−0.026**	**−0.008**	**−0.071****
$PTI_{i,t+8} - PTI_{i,t+4}$	**−0.015**	**−0.008**	**−0.046****	**−0.112****
$PTI_{i,t+9} - PTI_{i,t+5}$	**−0.002**	**−0.039****	**−0.088****	**−0.073****
$PTI_{i,t+10} - PTI_{i,t+6}$	**−0.041***	**−0.087****	**−0.051****	**−0.096****
$PTI_{i,t+11} - PTI_{i,t+7}$	**−0.075****	**−0.040***	**−0.082****	**−0.053***
$PTI_{i,t+12} - PTI_{i,t+8}$	**−0.036***	**−0.070****	**−0.011**	**−0.100****
$PTI_{i,t+13} - PTI_{i,t+9}$	**−0.083****	**−0.002**	**−0.059****	—
$PTI_{i,t+14} - PTI_{i,t+10}$	**0.009**	**−0.044****	—	—
$PTI_{i,t+15} - PTI_{i,t+11}$	**−0.058***	—	—	—

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*, ** Significantly different from zero at the 0.05 and 0.01 levels, respectively, in a two-tailed test.

[a] A "straddle" earnings change is defined as those instances where $t+k-l \geq t$ and $t+k-l-4 \leq t$. That is, the later earnings value is for a quarter after the NSI quarter and the earlier earnings value is from a quarter prior to the NSI quarter.

[b] A post-special-items change is defined as those instances where $t+k-l-4 > t$. That is, both earnings measures in the seasonally differenced earnings are values that occur after the NSI quarter.

[c] Numbers in bold refer to quarters t+9 through t+16.

Variable Definitions:

$PTI_{i,t+k}$ = pre-tax income (PIQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,$or 16) divided by the market value of equity (MKVALTQ) in quarter t–4; and

$RNSI_{i,t}$ = pre-tax negative special items (SPIQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter t–4.

appears more plausible that the earnings increases are the result of efficiency improvements after the NSI recognition, such as an effective restructuring, or a combination of efficiency gains and expense transfer. We further explore whether NSIs lead to expense transfer or real improvements in the following sections.

Expense Transfer or Real Improvements?

The evidence from Tables 3 and 4 does not preclude the possibility that NSI-induced expense transfer exists. To more fully explore the extent to which NSIs are associated with expense transfer versus efficiency gains, we estimate the following two regression equations:

$$\Sigma 4QPTI_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}, \tag{4}$$

and:

$$\Sigma 4QCFO_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}, \tag{5}$$

where:

$PTI_{i,t+k}$ = pre-tax income (PIQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,16$) divided by the market value of equity (MKVALTQ) in quarter $t-4$;

$CFO_{i,t+k}$ = cash flow from operating activities (OANCFQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,16$) divided by the market value of equity (MKVALTQ) in quarter $t-4$;

$\Sigma 4QPTI_{i,y} = (PTI_{i,t+k} - PTI_{i,t+k-4})$ summed for the four quarters where k equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$; and

$\Sigma 4QCFO_{i,y} = (CFO_{i,t+k} - CFO_{i,t+k-4})$ summed for the four quarters where k equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$.

All other variables are as previously defined. $\Sigma 4QPTI_{i,y}$ and $\Sigma 4QCFO_{i,y}$ are year-by-year, four-quarter summations of the seasonal differences in earnings and operating cash flows, respectively. We estimate Equations (4) and (5) for the four years subsequent to quarter t (i.e., quarters $t+1$ through $t+16$). Equation (4) allows us to measure the annually cumulated future earnings increases that result from the quarter t NSI. Equation (5) allows us to measure the extent to which the NSI in quarter t results in annually cumulated future increases in operating cash flows.

This portion of our analysis has similarities to, and distinctions from, prior work by Doyle et al. (2003) on the relation between future cash flows and items excluded from pro forma income numbers. They find that such excluded items, many of which are special items, have directionally consistent implications for future cash flows. That is, excluded expenses predict lower future cash flows, while excluded income items predict higher future cash flows. However, when Doyle et al. (2003) partition excluded items into special items and other excluded items, they report mixed evidence regarding the relation between special items and future cash flows. In particular, they report an insignificant relation between special items and operating cash flows, but a significantly positive relation with free cash flow (Doyle et al. 2003, 160–161, Tables 3 and 4).

Several important distinctions between Doyle et al. (2003) and this study should be noted. First, Doyle et al. (2003) do not partition special items into negative and positive amounts, whereas our entire focus is on the particular impact of NSIs documented in BJS. Second, their specification includes an accrual explanatory variable. This means that for accrued special items, the estimated coefficient reflects the marginal association of the item with future cash flows above and beyond its accrual effect, which has an opposite directional effect in their analysis. Third, their analysis is done using after-tax special item measures based on the top statutory tax rate. The earlier results in this study as well as the results in Beaver et al. (2006) suggest that such tax adjustments are problematic for NSIs.

Table 5 reports the coefficients from the estimation of Equations (4) and (5) as well as the annual change percentage implied by the coefficients and the cumulative change percentage from summed annual percentages. Estimation of Equation (5) requires additional data not incorporated in earlier tests. We follow the same screening procedures as discussed previously. Our data requirements and sample selection criteria result in a test sample of 36,609 (33,901) NSI observations with complete data to estimate Equation (4) (Equation (5)) for the first four years subsequent to quarter *t*.

The Equation (4) results are consistent with our prior tabulated evidence. In particular, we find that NSIs are associated with significant increases in earnings in each of the four years subsequent to the event quarter. The year +1 coefficient suggests that income increases in the first four quarters subsequent to the NSI by 8.6 percent of the quarter *t* NSI, while over the next four quarters (year +2) the estimated increase in income approximately doubles to generate a cumulative increase of 26.4 percent at the end year +2.[23] In years +3 and +4 we find income increases by another 40.6 and 56.2 percent of the NSI, respectively. Taken together, our evidence suggests that over the first 16 quarters subsequent to the charge, NSI-induced earnings increases equal more than 130 percent of the charge. As a practical matter, special-items-induced expense transfer is bounded by the amount of the NSI and must necessarily be less than 100 percent of the charge if the NSI contains *any* contemporaneous costs (classification shifting or true special item costs). Thus, our evidence suggests that realized efficiency gains over the subsequent 16 quarters equal *at least* 31.8 percent of the NSI.

We next examine the results from the estimation of Equation (5), which relates the NSI to future seasonally adjusted changes in operating cash flows. Because operating cash flows are less susceptible to manipulation by management, any NSI-induced increases in operating cash flows are more likely a result of efficiency gains than expense transfer. Our evidence suggests that NSIs are associated with substantial future increases in operating cash flows. Specifically, we find that over the first four years subsequent to the charge, NSI-induced increases in operating cash flows equal more than 40 percent of the charge. Our evidence suggests that a substantial portion of the future earnings increase related to the NSI is very likely the result of efficiency improvements rather than expense transfer.

Finally, the evidence from both Equations (4) and (5) suggests that the future earnings and cash flow effects of a current NSI are increasing over time. In particular, our evidence suggests that earnings increase by 8.6, 26.4, 40.6, and 56.2 percent of the quarter *t* NSI in years +1, +2, +3, and +4, respectively. Similarly, our evidence suggests that cash flows increase (decrease) by (0.6), 1.8, 12.8, and 26.2 percent of the quarter *t* NSI in years +1, +2, +3, and +4, respectively. This evidence is broadly consistent with NSIs being associated with real improvements since cash flow increases and expense transferred from four years in the future seems very unlikely to explain the earnings increases of the magnitude noted.

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[23] Since the dependent variables in Equations (4) and (5) are annually cumulated measures of seasonally differenced earnings or cash flows (e.g., $\Sigma 4QPTI_{i,y}$ for year +2 is the summed $(PTI_{i,t+k} - PTI_{i,t+k-4})$ for quarters t+6 to t+9), to determine the income and cash flow effect of the NSI in years beyond $y = 1$, the *RNSI* coefficients must be evaluated with respect to the coefficients for the prior year. For example, in year 1 the coefficient on *RNSI* in Equation (4) is −1.086 suggesting that pre-tax income (*PTI*) increases (NSIs enter as negative amounts) in year +1 relative to the year prior to quarter *t* by 8.6 percent of the NSI. In year +2 the coefficient on *RNSI* is −0.178 suggesting that special items-induced PTI increased in year +2 relative to year +1 by 17.8 percent of the NSI. Since earnings in year +1 are higher by 8.6 percent of the NSI, the total effect is that *PTI* increased in year +2 by 26.4 percent of the quarter *t* NSI relative to pre-special items earnings. The 26.4 percent is the amount we report in Panel A of Table 5 as the annual increase in year +2 *PTI*. This same procedure holds for calculating the cash flow increase induced by the NSI since this is also a seasonally adjusted measure.

TABLE 5

Relationship between Pre-Tax Negative Special Items and Seasonally Adjusted Pre-Tax Future Earnings and Operating Cash Flows for Years +1 through +4 Estimated b_{1y} from Equations (4) and (5):

$$\Sigma 4QPTI_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

$$\Sigma 4QCFO_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

	$y = 1$	$y = 2$	$y = 3$	$y = 4$
Equation (4) $\Sigma 4QPTI_{i,y}$				
n	36,609	26,997	20,331	15,106
b_{1y}	−1.086**++	−0.178**	−0.142**	−0.156**
Annual Change %[a]	8.6	26.4	40.6	56.2
Cumulative Change %[b]	8.6	35.0	75.6	131.8%
Equation (5) $\Sigma 4QCFO_{i,y}$				
n	33,901	26,094	19,987	14,913
b_{1y}	0.006	−0.024	−0.110**	−0.134**
Annual Change %[a]	(0.6)	1.8	12.8	26.2
Cumulative Change %[b]	(0.6)	1.2	14.0	40.2%

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*, ** Significantly different from zero at the 0.05 and 0.01 levels, respectively, in a two-tailed test.

+, ++ Significantly different from −1 at the 0.05 and 0.01 levels, respectively, in a two-tailed test (applies only for estimates of b_1 at $y = 1$).

[a] This is the percentage of the quarter t negative special item reflected in earnings and cash flows for years 1 through 4. For example, in Panel A for $\Sigma 4QPTI$ for $y = 2$ the 26.4 suggests the pre-tax income for quarters $t+5$ through $t+8$ (i.e., $y = 2$) increased by 26.4 percent of the *RNSI* relative to year 0.

[b] This represents the cumulative percentage impact of the quarter t negative special item on earnings and operating cash flows for quarters years +1 through +4.

Variable Definitions:

$PTI_{i,t+k}$ = pre-tax income (PIQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,$or 16) divided by the market value of equity (MKVALTQ) in quarter $t-4$;

$RNSI_{i,t}$ = pre-tax negative special items (SPIQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter $t-4$;

$CFO_{i,t+k}$ = cash flow from operating activities (OANCFQ) for firm i in quarter $t+k$ (where $k = 1,\ldots,$or 16) divided by the market value of equity (MKVALTQ) in quarter $t-4$;

$\Sigma 4QPTI_{i,y} = (PTI_{i,t+k} - PTI_{i,t+k-4})$ summed for the four quarters where k equals $1,\ldots,4$ for $y = 1$; $5,\ldots, 8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots, 16$ for $y = 4$; and

$\Sigma 4QCFO_{i,y} = (CFO_{i,t+k} - CFO_{i,t+k-4})$ summed for the four quarters where k equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots, 12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$.

Taken together, the results from estimating Equations (4) and (5) suggest that efficiency gains account for at least 30 percent of the earnings increases we document in Table 5. This estimate appears to be the lower bound on efficiency improvements because all increases in the accrual component of earnings are certainly not the result of expense transfer. Overall, our empirical evidence is not supportive of the expense-transfer hypothesis as the sole explanation for the results we observe. The pattern of *NSI* coefficients we observe is broadly consistent with NSIs representing a substantive repositioning or strategy shift by the entity where the primary benefit is received over a number of periods in the future. While we cannot fully disentangle the effects of these hypotheses for all special items, the differing implications of sub-types of special items may shed light on these inferences.

Restructuring Charges, Asset Write-Downs, and Goodwill Impairment

Our Table 5 results suggest that NSIs lead to substantial increases in earnings and operating cash flows that we attribute to both real improvements and expense transfer. In this section, we focus on the special item sub-types that we expect will have differing implications for future earnings and cash flows. In particular, we separately examine the future earnings and cash flow implications of restructuring charges, asset write-downs, and goodwill impairment charges. In so doing, we focus on NSI sub-types that are most likely to be associated with efficiency improvements, expense transfer, or those that should be purely transitory, respectively.

To the extent that NSIs are associated with future efficiency gains, we expect that restructuring charges will most likely lead to these gains (Brickley and Van Drunen 1990; Atiase et al. 2004; Clement et al. 2007). On the other hand, we expect asset write-downs to be most closely associated with the type of charge that represents an acceleration of future expense or expense transfer since future depreciation expense is brought forward into the quarter of recognition. Goodwill impairment charges do not promote efficiency gains nor are they an acceleration of future expenses since goodwill is not amortized in our sample period.

To more fully explore the extent to which these NSI sub-types are associated with future earnings and operating cash flows, we estimate the following two regression equations:

$$\begin{aligned}\Sigma 4QPTI_{i,y} = {} & b_{0y} + b_{1y}RC_{i,t} + b_{2y}WD_{i,t} + b_{3y}GW_{i,t} + b_{4y}OTHER\_NSI_{i,t} \\ & + b_{5y}(PTI_{i,t} - RNSI_{i,t} - PTI_{t-4}) + e_{i,t},\end{aligned} \tag{6}$$

and:

$$\begin{aligned}\Sigma 4QCFO_{i,y} = {} & b_{0y} + b_{1y}RC_{i,t} + b_{2y}WD_{i,t} + b_{3y}GW_{i,t} + b_{4y}OTHER\_NSI_{i,t} \\ & + b_{5y}(PTI_{i,t} - RNSI_{i,t} - PTI_{t-4}) + e_{i,t},\end{aligned} \tag{7}$$

where:

$RC_{i,t}$ = pre-tax restructuring charges (RCPQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter t–4;
$WD_{i,t}$ = pre-tax asset write-downs (WDPQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter t–4;
$GW_{i,t}$ = pre-tax goodwill write-offs (GDWLIPQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter t–4; and
$OTHER\_NSI_{i,t} = RNSI_{i,t} - RC_{i,t} - WD_{i,t} - GW_{i,t}$.

All other variables are as previously defined. We estimate Equations (6) and (7) for the four years subsequent to quarter t, quarters t+1 through t+16 (years +1 through +4).

Table 6 reports the coefficients from the estimation of Equations (6) and (7) as well as the cumulative change percentage implication of each special item sub-type for future earnings and cash flows. Consistent with our expectation, the Equation (6) results suggest that the future earnings implications of restructuring charges (*RC*s) are greater than those of asset write-downs (*WD*s) and goodwill impairment charges (*GW*s). Specifically, our evidence suggests that earnings increase over the subsequent four years by 573 percent of *RC*s, 152 percent of *WD*s, and 38 percent of *GW*s. The future cash flow results from the estimation of Equation (7) are consistent with the Equation (6) results. In particular, we find that operating cash flows increase over the subsequent four years by 156 percent of *RC*s, 66 percent of *WD*s, and *GW*s actually lead to a decrease in future cash flows

TABLE 6

Relation between Pre-Tax Restructuring Charges, Asset Write-downs, Goodwill Write-Offs, and Other Special Items and Seasonally Adjusted Future Earnings and Cash Flows Estimates of b_{1y}, b_{2y}, b_{3y}, and b_{4y} for years $y = 1$ through $y = 4$ from Equations (6) and (7):

$$\begin{aligned}\Sigma 4QPTI_{i,y} = {} & b_{0y} + b_{1y}RC_{i,t} + b_{2y}WD_{i,t} + b_{3y}GW_{i,t} + b_{4y}OTHER\_NSI_{i,t}\\ & + b_{5y}(PTI_{i,t} - RNSI_{i,t} - PTI_{t-4}) + e_{i,t}\end{aligned}$$

$$\begin{aligned}\Sigma 4QCFO_{i,y} = {} & b_{0y} + b_{1y}RC_{i,t} + b_{2y}WD_{i,t} + b_{3y}GW_{i,t} + b_{4y}OTHER\_NSI_{i,t}\\ & + b_{5y}(PTI_{i,t} - RNSI_{i,t} - PTI_{t-4}) + e_{i,t}\end{aligned}$$

	$y = 1$	$y = 2$	$y = 3$	$y = 4$
Equation (6) $\Sigma 4QPTI_{i,y}$				
n	36,609	26,997	20,331	15,106
RC	−2.109**++	−0.210**	−0.212**	−0.240**
Cumulative Change %[a]	110.9	242.8	395.9	573.0%
WD	−1.261**++	−0.226**	−0.075	0.354**
Cumulative Change %[a]	26.1	74.8	131.0	151.8%
GW	−0.871**++	−0.139**	−0.158**	−0.164**
Cumulative Change %[a]	(12.9)	(11.9)	4.9	38.1%
OTHER_NSI	−1.133**++	−0.176**	−0.151**	0.038
Cumulative Change %[a]	13.3	44.2	90.2	132.4%
Equation (7) $\Sigma 4QCFO_{i,y}$				
n	33,901	26,094	19,987	14,913
RC	−0.157**	−0.039	−0.267**	−0.278**
Cumulative Change %[a]	15.7	35.3	81.6	155.7%
WD	−0.007	−0.089	−0.070	−0.227**
Cumulative Change %[a]	0.7	10.3	26.9	66.2%
GW	0.050**	0.018	−0.021	−0.094
Cumulative Change %[a]	(5.0)	(11.8)	(16.5)	(11.8)
OTHER_NSI	−0.044	−0.019	−0.177**	−0.051
Cumulative Change %[a]	4.4	10.7	34.7	63.8%

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*, ** Significantly different from zero at the 0.05 and 0.01 levels, respectively, in a two-tailed test.

++ Significantly different from −1 at the 0.01 level in a two-tailed test (applies only for estimates of b_1 at $y = 1$).

[a] This represents the cumulative percentage impact of the quarter *t* negative special item on earnings and operating cash flows for quarters years +1 through +4.

Variable Definitions:

$PTI_{i,t+k}$ = pre-tax income (PIQ) for firm *i* in quarter *t*+*k* (where $k = 1,\ldots,$or 16) divided by the market value of equity (MKVALTQ) in quarter *t*–4;

$RNSI_{i,t}$ = pre-tax negative special items (SPIQ) for firm *i* in quarter *t* divided by the market value of equity (MKVALTQ) in quarter *t*–4;

$CFO_{i,t+k}$ = cash flow from operating activities (OANCFQ) for firm *i* in quarter *t*+*k* (where $k = 1,\ldots,$or 16) divided by the market value of equity (MKVALTQ) in quarter *t*–4;

$\Sigma 4QPTI_{i,y} = (PTI_{i,t+k} - PTI_{i,t+k-4})$ summed for the four quarters where *k* equals 1,. . .,4 for $y = 1$; 5,. . .,8 for $y = 2$; 9,. . .,12 for $y = 3$; and 13,. . .,16 for $y = 4$.

$\Sigma 4QCFO_{i,y} = (CFO_{i,t+k} - CFO_{i,t+k-4})$ summed for the four quarters where *k* equals 1,. . .,4 for $y = 1$; 5,. . .,8 for $y = 2$; 9,. . .,12 for $y = 3$; and 13,. . .,16 for $y = 4$.

(continued on next page)

TABLE 6 (continued)

$RC_{i,t}$ = pre-tax restructuring charges (RCPQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter t–4;
$WD_{i,t}$ = pre-tax asset write-downs (WDPQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter t–4;
$GW_{i,t}$ = pre-tax goodwill write-offs (GDWLIPQ) for firm i in quarter t divided by the market value of equity (MKVALTQ) in quarter t–4; and
$OTHER\_NSI_{i,t} = RNSI_{i,t} - RC_{i,t} - WD_{i,t} - GW_{i,t}$.

equal to 12 percent of the charge. Thus, we find increases in earnings and cash flows where we expect to find them with *RC*s and no impact where we expect there to be none with *GW*s.

Our results with respect to *WD*s are not entirely consistent with our expectation. We expected to see future earnings increases associated with the *WD* that fit the expense transfer scenario. That is, expected future earnings increases should not exceed 100 percent of the *WD* and evidence of declining earnings should exist in post-NSI change years (i.e., years +2 and beyond). However, the estimated cumulative increases in future earnings exceed 100 percent of the *WD* and the significantly positive coefficient on *WD* in year +4 provides only limited evidence of any sort of earnings decline. Overall, our Table 6 evidence with respect to *WD*s is inconsistent with these charges solely representing expense transfer. On the contrary, our evidence is more consistent with *WD*s having strategic implications. That is, strategy shifts lead to existing assets being used differently, triggering both the current *WD* and future improvements to earnings and operating cash flows. Consistent with this notion, we find that when a firm recognizes a *WD*, the firm also recognizes an *RC* 30 percent of the time.

To better address the impact of individual types of charges, we estimate Equations (6) and (7) for observations in which each of the individual special item sub-types are reported in isolation.[24] For example, for this set of tests we only include restructuring observations when all other NSI sub-types are zero. The results of these additional untabulated tests are consistent with our tabulated results in Table 6. For restructuring charges, we find *RC*-induced increases in earnings and cash flows over the four years subsequent to recognition of approximately 435 and 117 percent of the *RC*, respectively. Similarly, for *WD*s, we find *WD*-induced increases in earnings and cash flows over the four years subsequent to recognition of approximately 127 and 73 percent of the *WD*, respectively. With respect to *GW*s, we find earnings and cash flow increases of 10 and 17 percent, respectively. Thus, even when we require that these special item sub-types be reported in isolation, we still find increases in earnings and cash flows where we expect to find them with *RC*s and no impact where we expect there to be none with *GW*s.

Sensitivity Analyses

We believe our evidence supports the conclusion that, on average, real improvements are at least as important as expense transfer in determining the NSI-induced increases in earnings and cash flows for three reasons. First, we estimate NSI-induced earnings increases that exceed 130 percent of the NSI and 40 percent of this increase comes from increases to operating cash flows. Second, most of this increase is realized in years +3 and +4, which seems more likely to be the result of real performance improvements than a long-term expense transfer effect. Third, the increasing pattern of NSI-induced earnings we observe over the four years subsequent to recognition is broadly consistent with real improvements but not with expense transfer as the primary explanation.

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[24] We thank an anonymous referee for this suggestion.

That said, our results so far do not preclude a significant expense transfer effect in the data. To address this limitation, we offer additional tests, based on Riedl (2004), designed to shed some light on which of the two effects is more prevalent. Riedl (2004) concludes that asset impairments exhibit weaker association with the economic predictors of asset impairments in the post- versus pre-SFAS 121 period.[25] In addition, Riedl (2004) reports a higher association between asset impairments and "big bath" behavior after the enactment of SFAS 121. He concludes that big bath behavior reflects opportunistic reporting by managers rather than providing private information about the underlying performance of the firm, a notion consistent with big bath behavior being associated with expense transfer as opposed to real improvements.

If Riedl's (2004) findings extend to our setting, then we should find evidence more consistent with the declining expense transfer assumption for (1) post- versus pre-SFAS 121 and (2) big bath versus non-big bath behavior. For the post- versus pre-SFAS 121 tests (post-1996 versus pre-1995), we use all data to estimate Equation (4) for the years 1981 through 2009. The untabulated results of this test suggest no post- versus pre-SFAS 121 difference in the total NSI-induced earnings over the subsequent four years (127.6 and 119.6 percent of the NSI, respectively). In addition, the pattern of increases between the two subsamples is nearly identical.

Riedl (2004) identifies big bath observations as those firms with a negative change in pre-write-off earnings that is less than the median for all firms with a negative change in pre-write-off earnings.[26] Using the same sample from Table 5, untabulated results indicate that the total NSI-induced increases over the subsequent four years are greater for the non-big bath sample than the big bath sample (117 to 71 percent). In addition, we find that most of the increases for the big bath sample are in the first two years (increases of 8, 31, 24, and 8 percent for years 1, 2, 3, and 4, respectively), while the increases for the non-big bath sample are more concentrated in the last two years (increases of 5, 21, 34, and 58 percent for the same years, respectively). Thus, for the big bath sample, our results suggest evidence of reversal in years +3 and +4 (i.e., declining increases), while for the non-big bath sample our evidence suggests further increases in those same years.[27] Taken together, our evidence is consistent with expense transfer for those firms identified as most likely to be engaged in big bath behavior. However, the overwhelming majority of the sample firms do not engage in big bath behavior and the evidence with respect to these firms is more consistent with real improvements.

Prior Performance

Prior research provides compelling evidence that NSI firms underperform in the periods leading up to the recognition of the NSI (e.g., Dechow and Ge 2006; Fairfield et al. 2009). Since our analysis involves future changes in performance, our evidence of improved earnings and cash flows may be related to underperformance prior to recognition of the NSI. That is, underperforming firms have more room for improvement than highly performing firms. Accordingly, we investigate this by incorporating a matched-firm design to specifically control for pre-special item performance.[28] In particular, we estimate the following three regression equations:

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[25] Statement of Financial Accounting Standards (SFAS) No. 121, *Accounting for the Impairment of Long-Lived Assets*, was enacted in 1995.

[26] Riedl (2004, 833) addresses the measure of big bath (*BATH*) in his manuscript. Where he uses annual data and yearly changes, we substitute seasonal changes in pre-NSI earnings.

[27] We also perform this test on a sample of restructuring observations because these events are equally susceptible to both expense transfer and real improvement. Untabulated tests show that the total RC-induced earnings increases are greater for the non-big bath sample than the big bath sample (433 to 62 percent). In addition, the big bath sample actually shows a decrease in year +4 (i.e., evidence of reversal), while the non-big bath sample continues to show increases over all four years subsequent to recognition.

[28] Firms reporting an NSI in quarter t are matched by four-digit SIC to all non-NSI counterpart firms in the same industry, year, and quarter leaving zero or positive special item firms as a potential matched control firm. The NSI firm is then matched to the non-NSI firm with the smallest difference in four-quarter, pre-SI earnings growth over the four quarters prior to quarter t, defined as $[(E_{i,t} - SI_{i,t})/(E_{i,t-4} - SI_{i,t-4})] - 1$.

$$M\_\Delta PTI_{i,t+k} = b_{0k} + b_{1k}RNSI_{i,t} + b_{2k}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}, \tag{8}$$

$$M\_\Sigma 4QPTI_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}, \tag{9}$$

$$M\_\Sigma 4QCFO_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}, \tag{10}$$

where:

$M\_PTI_{i,t+k}$ = pre-tax income (PIQ) for non-NSI control firm *i* in quarter $t+k$ (where $k = 1,\ldots,4$) divided by the market value of equity (MKVALTQ) in quarter $t-4$;
$M\_CFO_{i,t+k}$ = cash flow from operating activities (OANCFQ) for non-NSI control firm *i* in quarter $t+k$ (where $k = 1,\ldots,16$) divided by the market value of equity (MKVALTQ) in quarter $t-4$;
$M\_\Delta PTI_{i,t+k} = [(PTI_{i,t+k} - PTI_{i,t+k-4}) - (M\_PTI_{i,t+k} - M\_PTI_{i,t+k-4})]$;
$M\_\Delta CFO_{i,t+k} = [(CFO_{i,t+k} - CFO_{i,t+k-4}) - (M\_CFO_{i,t+k} - M\_CFO_{i,t+k-4})]$;
$M\_\Sigma 4QPTI_{i,y} = M\_\Delta PTI_{i,t+k}$ summed for the four quarters where *k* equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$; and
$M\_\Sigma 4QCFO_{i,y} = M\_\Delta CFO_{i,t+k}$ summed for the four quarters where *k* equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$.

All other variables are as previously defined. This design subtracts the values of the independent variables for the control firm from the NSI firm to assess the effect, if any, of performance prior to the recognition of an NSI. We estimate Equation (8) for the four quarters subsequent to quarter *t* and Equations (9) and (10) for the four years subsequent to quarter *t*. Equation (8) allows us to assess the impact of controlling for prior performance on our Equation (2) results reported in Table 3. Equations (9) and (10) allow us to assess the impact of controlling for prior performance on our Equation (4) and (5) results reported in Table 5. We report the results of Equations (8), (9), and (10) in Table 7, Panels A, B, and C, respectively. In addition and for direct comparison, we re-estimate the corresponding Equations (2), (4), and (5) on the reduced subsample used for these tests.

In Panel A of Table 7, the matched control firm adjusted future earnings ($M\_\Delta PTI_{i,t+k}$) for quarters $t+1$ through $t+4$ suggests special item-induced earnings increases over the subsequent four quarters of approximately 6.7 percent of the NSI. This is almost exactly the same amount estimated in Equation (2), also reported in Panel A, where we find the increase is approximately 5.9 percent. We find similar evidence with respect to earnings and cash flow changes over the subsequent four years. In Panel B, our evidence suggests the matched control firm adjusted future earnings increase 106.9 percent of the NSI. And in Panel C, we find that the matched control firm adjusted future cash flows increase 65.9 percent of the NSI. Taken together, the evidence in Table 7 suggests that reversal of prior performance does not explain our results.[29]

Special Item Reporting Frequency

Cready et al. (2010) report that the market prices NSIs differently depending on prior reporting frequency. In particular, they report that as the prior reporting frequency increases, the pricing multiple on NSIs increases, consistent with the market treating recurring NSIs more like operating expenses than transitory items. In addition, Cready et al. (2010) and Johnson et al. (2011) report evidence that past reporting frequency predicts future reporting of NSIs. That is, the future

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[29] We also match NSI firms to non-NSI firms using cumulative four quarter earnings (scaled) for quarters $t-4$ to $t-1$ as a proxy for prior performance. Our results are robust to this alternative method.

TABLE 7

Relationship between Pre-Tax Negative Special Items and Matched Control-Firm Adjusted Seasonally Differenced Pre-Tax Future Earnings and Operating Cash Flows

Panel A: Estimated b_{1k} and b_{2k} from Equations (2) and (8) for Quarters t+1 to t+4

$$(PTI_{i,t+k} - PTI_{i,t+k-4}) = b_{0k} + b_{1k}RNSI_{i,t} + b_{2k}(PTI_{i,t} - RNSI_{i,t} - PTI_{t-4}) + e_{i,t}$$

$$M\_\Delta PTI_{i,t+k} = b_{0k} + b_{1k}RNSI_{i,t} + b_{2k}(PTI_{i,t} - RNSI_{i,t} - PTI_{t-4}) + e_{i,t}$$

	$k = 1$	$k = 2$	$k = 3$	$k = 4$	n
Equation (2) $(PTI_{i,t+k} - PTI_{i,t+k-4})$					
b_{1k}	0.007	−0.024**	−0.048**	−0.994**	27,688
b_{2k}	0.251**	0.137**	0.056**	−0.172**	27,688
Quarterly Change %	(0.7)	2.4	4.8	(0.6)	
Cumulative Change %^{b}	(0.7)	1.7	6.5	5.9	
Equation (8) $M\_\Delta PTI_{i,t+k}$					
b_{1k}	0.004	−0.037**	−0.051**	−0.983**	27,688
b_{2k}	0.180**	0.085**	0.037**	−0.164**	27,688
Quarterly Change %	(0.4)	3.7	5.1	(1.7)	
Cumulative Change %^{b}	(0.4)	3.3	8.4	6.7	

Panel B: Estimated b_{1y} from Equations (4) and (9) for Years $y = 1$ through $y = 4$

$$\Sigma 4QPTI_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

$$M\_\Sigma 4QPTI_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

	$y = 1$	$y = 2$	$y = 3$	$y = 4$
Equation (4) $\Sigma 4QPTI_{i,y}$				
n	25,827	19,772	14,818	10,946
b_{1y}	−1.063**++	−0.188**	−0.210**	−0.076*
Annual Change %	6.3	25.1	46.1	53.7
Cumulative Change %^{b}	6.3	31.4	77.5	131.2%
Equation (9) $M\_\Sigma 4QPTI_{i,y}$				
n	25,827	19,772	14,818	10,946
b_{1y}	−1.067**++	−0.154**	−0.233**	0.127*
Annual Change %	6.7	22.1	45.4	32.7
Cumulative Change %^{b}	6.7	28.8	74.2	106.9%

(continued on next page)

TABLE 7 (continued)

Panel C: Estimated b_{1y} from Equations (5) and (10) for Years $y = 1$ through $y = 4$

$$\Sigma 4QCFO_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

$$M\_\Sigma 4QCFO_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

	$y = 1$	$y = 2$	$y = 3$	$y = 4$
Equation (5) $\Sigma 4QCFO_{i,y}$				
n	24,878	19,203	14,646	10,871
b_{1y}	0.003	−0.008	−0.125**	−0.114**
Annual Change %	(0.3)	0.8	13.0	24.9
Cumulative Change %^{b}	(0.3)	0.5	13.5	38.4%
Equation (10) $M\_\Sigma 4QCFO_{i,y}$				
n	24,878	19,203	14,646	10,871
b_{1y}	−0.015	−0.038*	−0.102**	−0.183**
Annual Change %	1.5	5.3	17.0	42.1
Cumulative Change %^{b}	1.5	6.8	23.8	65.9%

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*, ** Significantly different from zero at the 0.05 and 0.01 levels, respectively, in a two-tailed test.

++ Significantly different from −1 at the 0.01 level in a two-tailed test (applies only for estimates of b_1 at $k = 4$ and $y = 1$).

Variable Definitions:

$PTI_{i,t+k}$ = pre-tax income (*PIQ*) for firm *i* in quarter $t+k$ (where $k = 1,\ldots,$or 4) divided by the market value of equity (*MKVALTQ*) in quarter $t-4$;

$RNSI_{i,t}$ = pre-tax negative pre-tax special items (*SPIQ*) for firm *i* in quarter *t* divided by the market value of equity (*MKVALTQ*) in quarter $t-4$;

$CFO_{i,t+k}$ = cash flow from operating activities (*OANCFQ*) for firm *i* in quarter $t+k$ (where $k = 1,\ldots,$or 16) divided by the market value of equity (*MKVALTQ*) in quarter $t-4$;

$\Sigma 4QPTI_{i,y} = (PTI_{i,t+k} - PTI_{i,t+k-4})$ summed for the four quarters where *k* equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$;

$\Sigma 4QCFO_{i,y} = (CFO_{i,t+k} - CFO_{i,t+k-4})$ summed for the four quarters where *k* equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$;

$M\_PTI_{i,t+k}$ = pre-tax income (*PIQ*) for non-*NSI* control firm *i* in quarter $t+k$ (where $k = 1,\ldots,$or 16) divided by the market value of equity (*MKVALTQ*) in quarter $t-4$;

$M\_CFO_{i,t+k}$ = cash flow from operating activities (*OANCFQ*) for non-NSI control firm *i* in quarter $t+k$ (where $k = 1,\ldots,16$) divided by the market value of equity (*MKVALTQ*) in quarter $t-4$;

$M\_\Delta PTI_{i,t+k} = [(PTI_{i,t+k} - PTI_{i,t+k-4}) - (M\_PTI_{i,t+k} - M\_PTI_{i,t+k-4})]$;

$M\_\Sigma 4QPTI_{i,y} = [(PTI_{i,t+k} - PTI_{i,t+k-4}) - (M\_PTI_{i,t+k} - M\_PTI_{i,t+k-4})]$ summed for the four quarters where *k* equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$; and

$M\_\Sigma 4QCFO_{i,y} = [(CFO_{i,t+k} - CFO_{i,t+k-4}) - (M\_CFO_{i,t+k} - M\_CFO_{i,t+k-4})]$ summed for the four quarters where *k* equals $1,\ldots,4$ for $y = 1$; $5,\ldots,8$ for $y = 2$; $9,\ldots,12$ for $y = 3$; and $13,\ldots,16$ for $y = 4$.

persistence of the NSI is linked to past reporting. Thus, the evidence we report with respect to future earnings increases may be influenced by prior reporting frequency.

To address this issue, we re-estimate Equations (2), (4), and (5) conditioned on NSI reporting frequency over the prior four years. The cumulative change percentage we find over the subsequent four quarters is (0.6), 11.5, 20.3, 13.2, and 12.3 percent of the NSI for observations with zero, one, two, three, and four prior NSIs, respectively. With respect to the re-estimation of Equation (4), we find subsequent NSI-induced cumulative earnings increases over the subsequent four years of 135 (128) percent for NSI observations preceded by one (four) prior NSI(s). Similarly with respect to the re-estimation of Equation (5), we find cumulative NSI-induced increases to cash flows over the subsequent four years of 70 (61) percent for NSI observations preceded by one (four) prior NSI(s).

Thus, the untabulated evidence from these tests is generally consistent with the conclusion that reporting frequency is largely unrelated to future earnings changes over the subsequent four quarters and four years, respectively.

Survivorship Bias

Our conclusion that NSIs lead to performance improvements is potentially explained by "survivors" that naturally perform better than firms that fail to survive subsequent to recognition of an NSI. To more fully examine this notion, we create two samples ("survivors" and "non-survivors") to ascertain the effect of survivorship bias on our results. We use these two samples to re-estimate Equations (4) and (5). A "survivor" is defined as an observation that has complete data to estimate Equation (4) or (5) for each of the four years subsequent to NSI recognition. On the other hand, a "non-survivor" is defined as an observation that has complete data to estimate Equation (4) or (5) for year +1 but does not have complete data for the years +2, +3 and +4. The results from the estimation of Equations (4) and (5) for the survivor and non-survivor samples are reported in Table 8 Panels A and B, respectively. We limit this examination to the three years subsequent to the NSI recognition because our data restricts us in creating a non-survivor sample to these particular years.

The Equation (4) results for the "survivor" sample are consistent with our prior tabulated evidence in Table 5. In particular, in Table 8, Panel A we find that NSIs are associated with significant increases in earnings in each of the three years subsequent to the event quarter. Taken together, our evidence suggests that over the first 12 quarters subsequent to the charge, NSI-induced earnings increases equal more than 123 percent of the charge. We next examine the results from the estimation of Equation (5) that relates the NSI to future seasonally adjusted changes in operating cash flows. Our evidence suggests that NSIs are associated with substantial future increases in operating cash flows. Specifically, we find that over the first 12 quarters subsequent to the charge, NSI-induced increases in operating cash flows equal more than 19 percent of the charge. Thus, our evidence suggests substantial NSI-induced increases in earnings and cash flows for the "survivor" sample that we would expect to perform the best subsequent to the NSI recognition.

However, our evidence in Table 8, Panel B suggests that survivorship bias is not driving our primary conclusions. Consistent with this notion, we find NSI-induced earnings increases for our non-survivor sample over the subsequent three years of 93.2 percent of the NSI. While not as great as the increases found for our sample of survivors in Panel A (123.1 percent), it is greater than the increases documented for the full sample in Table 5 (75.6 percent). Our evidence with respect to operating cash flows (estimation of Equation (5)) is similar to the earnings results. We find NSI-induced operating cash flow increases over the subsequent three years of 38.7, which is greater than the survivor sample in Panel A and the full sample results we report in Table 5 of 19.2 and 14.0 percent, respectively. While we cannot fully rule out a survivor effect, our evidence does not support the conclusion that survivorship bias drives our overall results.

Subsequent Special Item Reporting

Finally, we test the impact of subsequent NSI reporting on our results. If a firm engages in a pattern of consistently reporting NSIs, expense transfer may appear to be real improvements. Fairfield et al. (2009) discuss this notion in their study and suggest this as a potential explanation for their result that "some firms maintain high core profitability by becoming serial chargers." To address this possibility as a potential explanation for our results, we re-estimate Equation (4) conditioned on the number of years a firm recognizes an NSI over the subsequent four years. Our evidence suggests no relation between NSI-induced earnings increases and subsequent reporting frequency. In particular, untabulated results reveal NSI-induced earnings increases of 213, 175,

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TABLE 8

Relationship between Pre-Tax Negative Special Items and Seasonally Adjusted Pre-Tax Future Earnings and Operating Cash Flows for Years +1 through +3 for "Survivor" and "Non-Survivor" Subsamples[c]

Estimated b_{1y} from Equations (4) and (5):

$$\Sigma 4QPTI_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

$$\Sigma 4QCFO_{i,y} = b_{0y} + b_{1y}RNSI_{i,t} + b_{2y}(PTI_{i,t} - RNSI_{i,t} - PTI_{i,t-4}) + e_{i,t}$$

Panel A: Survivor Sample

	$y = 1$	$y = 2$	$y = 3$
Equation (4) $\Sigma 4QPTI_{i,y}$			
n	14,974	14,974	14,974
b_{1y}	−1.236**++	−0.211**	−0.101**
Annual Change %[a]	23.6	44.7	54.8
Cumulative Change %[b]	23.6	68.3	123.1
Equation (5) $\Sigma 4QCFO_{i,y}$			
n	14,219	14,219	14,219
b_{1y}	0.009	−0.045**	−0.129**
Annual Change %[a]	(0.9)	3.6	16.5
Cumulative Change %[b]	(0.9)	2.7	19.2

Panel B: Non-Survivor Sample

	$y = 1$	$y = 2$	$y = 3$
Equation (4) $\Sigma 4QPTI_{i,y}$			
n	8,376	6,630	5,357
b_{1y}	−1.042**+	−0.273**	−0.260**
Annual Change %[a]	4.2	31.5	57.5
Cumulative Change %[b]	4.2	35.7	93.2%
Equation (5) $\Sigma 4QCFO_{i,y}$			
n	8,036	6,437	5,271
b_{1y}	−0.014	−0.083*	−0.179**
Annual Change %[a]	1.4	9.7	27.6
Cumulative Change %[b]	1.4	11.1	38.7%

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*, ** Significantly different from zero at the 0.05 and 0.01 levels, respectively, in a two-tailed test.

+, ++ Significantly different from −1 at the 0.05 and 0.01 levels, respectively, in a two-tailed test (applies only for estimates of b_1 at $y = 1$).

[a] This is the percentage of the quarter *t* negative special item reflected in earnings and cash flows for years 1 through 4. For example, for *Σ4QPTI* for $y = 2$ the 31.5 suggests the pre-tax income for quarters *t*+5 through *t*+8 (i.e., $y = 2$) increased by 35.7 percent of the *RNSI* relative to year 0.

[b] This represents the cumulative percentage impact of the quarter *t* negative special item on earnings and operating cash flows for years +1 through +4.

(continued on next page)

TABLE 8 (continued)

[c] The "Survivor Sample" requires that each observation have complete data to estimate Equations (4) or (5) for all four years subsequent to the quarter in which the *RNSI* is reported. The "Non-Survivor Sample" requires that the observation have complete data to estimate Equation (4) or (5) in a particular year, but not in any of the subsequent years. For example, the 8,376 observations for the estimation of Equation (4) in $y = 1$ have complete data for the year after the *RNSI* is reported (i.e., year +1), but not for the years subsequent to $y = 1$ (i.e., years +2, +3 and +4).

Variable Definitions:
$PTI_{i,t+k}$ = pre-tax income (*PIQ*) for firm *i* in quarter *t+k* (where $k = 1,\ldots,$or 16) divided by the market value of equity (*MKVALTQ*) in quarter *t*–4;
$RNSI_{i,t}$ = pre-tax negative special items (*SPIQ*) for firm *i* in quarter *t* divided by the market value of equity (*MKVALTQ*) in quarter *t*–4;
$CFO_{i,t+k}$ = cash flow from operating activities (*OANCFQ*) for firm *i* in quarter *t+k* (where $k = 1,\ldots,$or 16) divided by the market value of equity (*MKVALTQ*) in quarter *t*–4;
$\Sigma 4QPTI_{i,y} = (PTI_{i,t+k} - PTI_{i,t+k-4})$ summed for the four quarters where *k* equals 1,. . .,4 for $y = 1$; 5,. . .,8 for $y = 2$; 9,. . .,12 for $y = 3$; and 13,. . .,16 for $y = 4$; and
$\Sigma 4QCFO_{i,y} = (CFO_{i,t+k} - CFO_{i,t+k-4})$ summed for the four quarters where *k* equals 1,. . .,4 for $y = 1$; 5,. . .,8 for $y = 2$; 9,. . .,12 for $y = 3$; and 13,. . .,16 for $y = 4$.

202, 64, and 219 percent of the NSI for firms with zero, one, two, three, and four subsequent annual NSIs, respectively.

V. CONCLUSION

In a path-breaking study of the time-series earnings implications of special items, Burgstahler et al. (2002; hereafter, BJS) provide evidence that suggests that the market fails to fully impound the predictable future earnings implications of special items. They also report that NSIs signal sizable increases in earnings over the subsequent four quarters. In fact, their evidence suggests that firms realize over 56 percent of the NSI in earnings over the subsequent four quarters. They conjecture that inter-period transfer of expense is the likely explanation for this observed relation between NSIs and future earnings. That is, firms accelerate the recognition of future period expenses in the form of a current NSI and thereby artificially boost future income.

The BJS conclusion that NSIs represent the current recognition of future period expenses is of particular interest for several reasons. First, these items are relatively transparent because unusual or infrequent items are separately identified on the income statement, disclosed in the footnotes, or both.[30] Second, the timing, classification, and magnitude of NSI recognition in certain circumstances represents a voluntary choice by management. Hence, the use of NSIs to opportunistically shift expenses from one period to another or from a normal operating category to an unusual or infrequent category supports the notion that voluntary disclosure environments are associated with adverse information quality. Third, identification and recognition of expenses in appropriate time periods is a bedrock principle of financial accounting (FASB Statement of Financial Accounting Concepts No. 5). If special items serve, as BJS suggest, primarily as a device for circumventing the recognition of expenses in appropriate reporting periods, then rules governing the recognition of special items may need improvement. Finally, the market's positive valuation of subsequent earnings increases documented in BJS raises the possibility that the market is fooled not only by the initial recognition of NSIs, which are over-discounted, but also by the subsequent predictable increases to earnings, which are over-valued.[31] That is, the inter-period transfer hypothesis suggests that future earnings induced by the recognition of NSIs may result in an unusually potent market misvaluation effect.[32]

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[30] NSI charges are often described on the income statement as "special," "unusual," "nonrecurring," "restructuring," or other similar descriptive labels.

Our analysis tests the BJS expense transfer conjecture with respect to its implications for earnings behavior in quarters beyond those immediately following the special items quarter. Under the expense-transfer hypothesis, the earnings increases observed in the first four quarters following NSI recognition must reverse in subsequent quarters. That is, if the observed earnings increases are the result of accelerating future expenses via an NSI, then these earnings increases cannot continue in perpetuity since the cumulative transfer effect is bounded by the amount of the initial charge. We find no evidence of such reversals over the four-year time period following the special item event quarter. Indeed, our evidence suggests that second, third, and fourth year income continues to build on the increases realized in the first year after the special item.

Our evidence can, in part, be explained as reflecting special items consisting of larger amounts of distant-period expenses relative to nearer expenses. However, a more straightforward explanation is that the increasing earnings pattern reflects real improvement in post-NSI firm performance that is either signaled by, or a consequence of, the NSI event. Consistent with this perspective, we find that NSIs are associated with substantial future increases in cash flows from operations. In particular, we find that over 40 percent of the NSI-induced earnings increases we document are realized through increases in operating cash flow. Since operating cash flows are less susceptible to temporal manipulation, our evidence suggests that the earnings increases we document are more likely to be the result of real improvements.

We interpret our evidence as demonstrating that NSIs lead to nontransitory future performance improvements that result from underlying shifts in firm strategies or operations that are costly in the short-run but improve profits in the long-run. Moreover, these long-run effects are an important factor in explaining the pattern of persistent earnings increases that we and BJS observe in post-special-items quarters, especially those beyond the fourth subsequent quarter. At the same time, it is also likely that some firms are engaging in accelerating the recognition of future ongoing expenses via the special item charge. We interpret our evidence as suggesting that such expense transfer effects do not account for the bulk of the predictable earnings behavior occurring in post-special items periods.

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[31] Elliott and Hanna (1996) and Cready et al. (2010) report that negative special items (write-offs) are typically valued at a much lower rate than other earnings components, suggesting that they are discounted substantially by the market.

[32] BJS evaluate the potential market mispricing of special items and find that the market does not properly impound the predictable increases in future earnings from NSIs. We have also run these tests and our untabulated results indicate that this mispricing phenomenon is still occurring.

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Multinational Taxation and R&D Investments

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ABSTRACT: This study examines the effects of taxation on the incentives of multinational firms to develop and use intellectual property. We model optimal investment and production decisions by firms that engage in a patent race by making R&D investments. We investigate how taxes affect the level and efficiency of R&D investments, and how these effects depend on whether the winner of the patent race uses it by either producing in the country in which the patent was developed (the domestic country) or in a foreign country. A higher domestic tax rate decreases investment in R&D if production occurs in the domestic country, but increases investment in R&D if production occurs in the foreign country. The present value of domestic tax revenues is strictly positive if production occurs in the domestic country, but is weakly negative if production occurs in the foreign country.

Keywords: *multinational taxation; R&D investment; transfer pricing; patent races.*

I. INTRODUCTION

We examine the effects of taxation on the incentive of a multinational firm to develop and use intellectual property. Intellectual property is an important and growing source of firm value. Lev (2001) attributes much of the tripling in the price-to-book ratios of the S&P 500 over the past 30 years to the value of intellectual property, which is reflected in a firm's market value but not its book value. Because intellectual property lacks a physical manifestation and does not appear as an asset on a firm's balance sheet, it is difficult to ensure that the income that intellectual property generates is taxed by the country in which the property was created. Therefore, the increasing importance of intellectual property poses a challenge to the current system of taxing multinational income, which evolved in an era in which tangible and financial assets were the

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dominant sources of firm value. Lev (2001) and Blair and Wallman (2001) provide an overview of the economic, accounting, and taxation issues associated with intangible assets.

We investigate the effects of taxation of intellectual property of multinational firms using a two-stage model. The first stage is the patent race in which many firms compete for a patent. The winner of the race obtains a valuable intangible asset. The second stage is the production stage in which the winner of the patent race uses its intellectual property. We model the full economic environment in which the quasi-rents that the winning firm receives in the second stage are competed away at the industry level in the first stage. Production in the second stage could occur either domestically or in a foreign country. In case of foreign production, the difficulty in valuing intangible assets creates important transfer pricing issues.

The traditional analysis of the taxation of investments in intellectual property (Gravelle 1994, 209) features a single firm that makes a positive net present value investment. The investment is expensed for tax purposes when it is made, and future cash flows from the investment are taxed as they occur. The government collects zero net tax revenue in present value terms on the normal return to capital, and a fraction equal to the statutory tax rate of the pretax net present value of the project. The tax treatment of investments in intangible assets differs from the taxation of investments in tangible assets in that tangible assets are capitalized instead of expensed. As a result, the normal return to capital from tangible investments is taxed.

This traditional analysis of the taxation of investments in intellectual property is incomplete in two respects. First, the analysis begins with a single firm that makes a positive net present value investment. This approach disregards the firms that made unsuccessful investments. In our study, we focus on all of the firms in an industry that try to develop intellectual property, rather than focusing only on the firm that succeeds. All firms that enter the patent race get to deduct their investments; the winning firm's pretax accounting profit from its use of the patent is subject to tax.

Second, the traditional analysis assumes that all of the economic returns to the investment are taxed by the country in which the investment takes place. In our study, we explicitly model the tangible investment needed to exploit the intellectual property once it is created. This tangible investment can be made either in the country in which the intellectual property is created or in a foreign country. When a patent is developed in one country and used in a different country, taxable income is divided between the two countries. The division of taxable income depends on the transfer price paid by the foreign subsidiary to the domestic firm as compensation for the use of the intellectual property. Governments have coordinated on the arm's-length standard for determining transfer prices, in which the results of the transaction are consistent with the results that would have occurred had the transaction taken place between two unrelated parties (Treas. Reg. §1.482-1(b)(1)).[1] We consider the case in which the market for inputs needed to manufacture the product in the foreign country is competitive. In that case, the income allocated to the foreign country under the arm's-length standard is the amount that is needed to provide the foreign subsidiary with an after-tax rate of return equal to the cost of capital; the remainder (the residual profit) should be allocated to the domestic parent because the domestic firm provides the intellectual property from winning the patent race.

Enforcing the arm's-length standard, however, can pose problems because it may be difficult for the tax authority to value the intellectual property. This difficulty implies that, in practice, firms may be able to shift some of the taxable income that the intellectual property generates to a low-tax country by deviating from the arm's-length standard. Incentives to shift income can be strong because corporate statutory tax rates differ significantly across countries, ranging from 35 percent in the U.S. and 40 percent in Japan to 12.5 percent in Ireland and 17 percent in Singapore. Bartelsman and Beetsma (2003), Huizinga and Laeven (2008), and Clausing (2003) find evidence of income shifting away from high-tax countries to low-tax countries. In our model, we allow for the possibility that the multinational firm is able to shift some of the pretax residual profit to the foreign country by using an allocation of taxable income that is not consistent with the arm's-length standard. When the foreign tax rate is strictly less than the domestic tax rate, the firm's ability to deviate from the arm's-length standard increases the attractiveness of foreign production. When the firm complies with the arm's-length standard, it will optimally produce in the foreign country if either lower costs and/or a lower tax rate cause the residual profit from foreign production to be higher than the residual profit from domestic production. When the ability of the firm to deviate from the arm's-length standard is high, however, it may be optimal for the firm to produce in the foreign (low-tax) country even when the residual profit is greater when producing domestically.

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[1] International coordination on the arm's-length standard has evolved over many years. The first Organization for Economic Co-operation and Development (OECD) model tax convention was enacted in 1958, but efforts to coordinate tax rules go back to efforts by the League of Nations in the 1920s.

We use our model to investigate the effect of taxation of intangible investments on the domestic government's tax collections. The traditional analysis of the taxation of intangible investments overstates the domestic government's tax collections on intangible investments because it ignores the tax deductions of the intangible investments of unsuccessful firms, and because it ignores the possibility that the tangible investment can be made in a foreign country. With free entry to the patent race, the project has zero after-tax net present value to the industry because firms optimally choose their investment levels such that, in present value terms, the industry-wide after-tax intangible investment cost equals the after-tax revenues generated by the successful investment. Because all investments in R&D are tax-deductible in the domestic country, this implies that the net present value of the domestic government's tax revenues associated with the *intangible investment* is zero if it gets to tax all the returns from the intangible investment, but strictly negative otherwise. In addition, the domestic government taxes the returns to the *tangible investment* only if this investment is made domestically. Therefore, the domestic government's tax revenues from the tangible and the intangible investment are strictly positive in case of domestic production, zero in case of foreign production with compliance with the arm's-length standard, and strictly negative in case of foreign production with deviations from the arm's-length standard.

We also investigate the effects of the level of the domestic tax rate on investments in R&D and domestic tax revenues. When production occurs domestically, a higher domestic tax rate increases the taxes collected on the successful firm's revenues in the production stage, but decreases the industry-wide level of investment in R&D, which delays the start of the production stage. The effect of a higher domestic tax rate on the present value of domestic tax revenues is therefore ambiguous, and depends on the effectiveness of the R&D process. In contrast, when production occurs in the foreign country, a higher domestic tax rate leads to more investment in R&D. This occurs because the entire investment in R&D is deducted at the domestic rate, but only part of the taxable income from the investment is taxed at the domestic rate. Hence, a higher domestic tax rate makes investment in R&D more attractive when production occurs in the foreign country. Because R&D investments generate weakly negative net present value tax revenues to the domestic government, a higher domestic tax rate unambiguously decreases the net present value of domestic tax revenues when production occurs in the foreign country.

We also use our model to investigate two ways in which taxation of intangible investments affects social welfare. Social welfare consists of the sum of firm value, the present value of taxes collected, and the present value of consumer surplus. Because taxes merely represent a redistribution of wealth, they do not directly affect social welfare. However, taxes indirectly affect social welfare through their effect on production and investment decisions. First, the taxation of intangible investments can affect the firm's choice of production location, which in turn affects social welfare. In our model, the firm's privately optimal production location is determined jointly by cost factors (fixed and variable production costs), the tax rates of the domestic and the foreign country, and the firm's ability to deviate from the arm's-length standard. Devereux and Griffith (1998) find that tax rate differentials have a significant effect on foreign investment decisions of U.S. multinationals; Hebous et al. (2011) find similar results with respect to foreign investment decisions by German multinationals. The socially optimal production location, however, is only driven by cost factors because taxes represent a wealth transfer, which does not affect the level of social welfare. Without tax rate differentials, the firm's privately optimal production location may not be socially optimal; therefore, a tax-induced choice of production location can increase social welfare.

Second, taxes affect social welfare indirectly because taxation affects the level of R&D investments in the first stage. We find that the privately optimal level of R&D investment is higher than the socially optimal level if the R&D process is relatively effective, and lower than the socially optimal level if the R&D process is relatively ineffective. We define effectiveness of R&D investments in terms of how quickly the winner of the race obtains the patent for a given level of investment. Because a higher domestic tax rate decreases the level of investment in R&D in case of domestic production, but increases the level of investment in R&D in case of foreign production, we find that whether a higher domestic tax rate moves the privately optimal investment level closer to or further away from the socially optimal level depends jointly on the production location and on the effectiveness of the R&D investments.

Overall, we find that the effects of taxation are ambiguous with respect to social welfare. The reason is that the equilibrium without taxes need not be efficient, for two reasons. First, Dasgupta and Stiglitz (1980) show that in a patent race, the privately optimal level of R&D investments could be inefficiently high or low. Second, we show that the privately optimal location of production could be socially inefficient, because the firm wants to maximize its own value, whereas social welfare also includes tax revenues and consumer surplus. Because the equilibrium without taxes need not be efficient, taxes could either increase or decrease efficiency, depending on the parameters of the model. This outcome is consistent with the theory of the second-best (Lipsey and Lancaster 1956).

In Section II we discuss prior literature. Section III presents the model. Section IV derives the privately optimal and socially optimal production location for the firm that wins the patent race. Section V derives the privately optimal and socially optimal investments in R&D, computes the net present value of the R&D investment for both the winner of the patent race and the industry as a whole, and investigates how taxes affect the level and efficiency of R&D investments. Section VI characterizes expected domestic tax collections. Section VII concludes.

II. PRIOR LITERATURE

Investments in Intangible Assets

Several papers in economics and accounting have focused on the tax-favored nature of investments in internally developed intangible assets. Fullerton and Lyon (1988) argue that effective tax rate measures that exclude the taxation of intangible capital are misleading. In an analysis restricted to investments in R&D and advertising, they estimate the effect of expensing these investments for the 1983 tax year. Dunbar and Sansing (2002) and Robinson and Sansing (2008) indirectly estimate investments in intangible assets based on the difference between a firm's book and market value of assets. All three papers find that the ability to expense investments in internally developed intangible assets significantly reduces the tax burden that corporations face by allowing the investment to be expensed when made, rather than being capitalized and amortized over the useful life of the asset. This paper extends this literature by considering the effects of developing the assets in one country and using them in another country.

Dasgupta and Stiglitz (1980) model a patent race in which there is free entry into the race, but only the first firm to make a discovery generates profits from the discovery. Free entry ensures that economic profits at the industry level are competed down to zero. They use their model to determine the socially optimal patent life and whether the privately optimal level of research is higher or lower than the socially optimal level. We use the Dasgupta and Stiglitz (1980) model to characterize the equilibrium level of investment in intellectual property. We extend their model by incorporating taxes into the analysis. We show that the effect of the domestic tax rate on the efficiency of R&D investments depends jointly on the production location and on the effectiveness of R&D investments.

Transfer Pricing Rules

Our study features a patent race in which the winner obtains valuable intellectual property. The tangible investment needed to use the intellectual property can be made either in the country in which the intellectual property is created or in a foreign country. The latter case raises the issue of transfer pricing because the owner of the intellectual property should be taxed on the value it adds. In the setting that we examine, 100 percent of the residual profit should be allocated to the domestic parent under the arm's-length standard, because the domestic firm provides the intellectual property from winning the patent race. The Comparable Profit Method (CPM), one of several transfer pricing methods prescribed by the United States Treasury Regulations interpreting Internal Revenue Code §482, achieves this outcome. Halperin and Srinidhi (1996) and Sansing (1999) both examine CPM in their studies of multinational taxation.

Our model allows for the possibility that firms may be able to shift some of the taxable income that the intellectual property generates to a low-tax country by deviating from the arm's-length standard. There is a large literature on the use of transfer pricing associated with intellectual property as a way of shifting taxable income from high-tax to low-tax countries. Intellectual property poses particularly difficult problems to tax authorities because it is difficult to value. Pearson (2008) reviews the challenges tax authorities face in this area. Grubert (2003) examines the relation between intangible assets generated by R&D investments and multinational taxation. He finds that about half of the income shifted from high-tax to low-tax countries is associated with R&D investments made by the parent corporation of a multinational enterprise. Similarly, Bartelsman and Beetsma (2003) find evidence of income shifting for OECD countries, particularly in industries such as pharmaceuticals with high levels of intellectual property. Huizinga and Laeven (2008) find evidence of income shifting by European multinationals away from high-tax Germany and toward European countries with lower tax rates. Clausing (2003) finds that when a U.S. multinational trades with a foreign affiliate in a country with a lower tax rate, intrafirm transfer prices are lower for exports by the parent and higher for imports by the parent, relative to transactions with unrelated parties. This is consistent with using transfer prices to shift income to the lower tax rate country.

Smith (2002) shows that the ability to deviate from the arm's-length standard, or *ex post* discretion, can enhance efficiency by acting as a substitute for inefficiently high investment in the low-tax country (*ex ante* discretion.) An important distinction is that Smith (2002) takes both upstream and downstream production location as exogenous, whereas we endogenize the location of production. Grubert and Slemrod (1998) find evidence of firms with high levels of intangible property, such as pharmaceuticals, choosing to manufacture in Puerto Rico to facilitate the shifting of income to a low-tax political jurisdiction. Grubert (2003) finds that R&D intensive multinational firms are likely to invest in countries with either very high or very low statutory tax rates, as the ability to shift income between political jurisdictions has the greatest benefit when the difference in the jurisdictions' statutory tax rates is high. Consistent with this evidence, in our model we find that the ability to shift income may induce the firm to produce in a low-tax foreign country. This can increase social welfare because social welfare includes tax revenues and consumer surplus, which the firm does not take into account when choosing where to produce. If the sum of consumer surplus and tax revenues is higher when production occurs in the low-tax country, then the tax-induced decision to produce there can increase social welfare.

III. MODEL

In this section, we develop a two-stage model in which firms engage in a patent race in stage one, and the winner of the race uses the patent to produce output in stage two. Tax considerations are important in each stage. Each firm gets to deduct its investment in R&D in the first stage, even though only the winner of the race will generate taxable income from the patent in the second stage. In the second stage, the winner of the race can produce in either its own country, or in a foreign country with a lower tax rate. The ability to shift taxable income to the low-tax country provides an incentive to produce in the foreign country.

The Patent Race

N identical firms compete for a patent by making an investment in R&D on date zero. Each firm can reduce the time it takes to obtain the patent by investing more in R&D. Specifically, in the absence of competitors, the time that it takes for firm i to obtain the patent is an exponentially distributed random variable $T_i \sim Exp(\eta_i \lambda)$, where η_i denotes firm i's investment in R&D and λ is the parameter reflecting the effectiveness of the search process. For any given investment level η_i, a higher value of λ implies that the expected time to obtain the patent decreases. Given that N firms compete, the patent will be obtained at time $T^* = \min\{T_i\}$. The firm that wins the patent will subsequently operate as a monopolist on a worldwide market, with an inverse demand function given by:

$$p(q) = \tilde{d} - \alpha q, \tag{1}$$

where $p(q)$ denotes the unit price, q denotes the quantity produced and sold, $\tilde{d}$ denotes a demand parameter, and α represents the sensitivity of price with respect to supply. To reflect variation in demand, we let $\tilde{d}$ be a random variable with mean μ and variance σ^2.

Net Present Value from Domestic and Foreign Production

The firm that wins the patent race then decides whether to produce domestically or in a foreign country. If the firm produces domestically, then it invests $K_D > 0$, which allows the firm to produce in perpetuity at a variable production cost of c_D per unit produced.[2] The investment could be a non-depreciable investment such as land; alternatively, it could be an investment in a physical plant that wears out over time, for which the variable production cost includes the amount needed to maintain its productive capacity.[3] Let:

$$\tilde{Y}_D = q_D[p(q_D) - c_D] = q_D[\tilde{d} - \alpha q_D - c_D] \tag{2}$$

denote the firm's annual taxable income from producing q_D units in the domestic country and selling them on the world market. Income from domestic production is taxed by the domestic government at a rate of τ_D. Future cash flows are discounted using the cost of capital r. Therefore, producing q_D units in perpetuity generates an expected after-tax net present value for the firm of:

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[2] The perpetuity assumption simplifies the analysis; qualitatively similar results would arise if the firm's patent had a finite life.

[3] Under this interpretation, tax depreciation is equal to economic depreciation. Our results continue to hold in a qualitative sense as long as the investment K_D is not expensed for tax purposes when it is incurred.

$$V_D = (1-\tau_D)\int_0^{\infty} Y_D e^{-rt}dt - K_D = \frac{(1-\tau_D)\Pi_D}{r} \tag{3}$$

where $Y_D = E[\tilde{Y}_D]$, and where:

$$\Pi_D = Y_D - \frac{rK_D}{1-\tau_D} \tag{4}$$

denotes the expected annual residual profit from producing in the domestic country.

Alternatively, the firm can decide to produce in the foreign country F using a wholly owned subsidiary. If the firm produces in the foreign country, then it incurs a fixed cost of $K_F > 0$ and a variable production cost of c_F per unit produced. Let:

$$\tilde{Y}_F = q_F[p(q_F) - c_F] = q_F[\tilde{d} - \alpha q_F - c_F] \tag{5}$$

denote the firm's annual taxable income from producing q_F units in the foreign country and selling them on the world market. We let τ_F denote the foreign country's corporate tax rate. Because tax rate differentials are an economically important reason for firms to produce in a foreign country (see, e.g., Grubert 2003), we focus on the case in which the foreign country has a weakly lower tax rate, i.e., $\tau_F \leq \tau_D$. We let:

$$\Pi_F = Y_F - \frac{rK_F}{1-\tau_F} \tag{6}$$

denote the annual expected residual profit from producing in country F, where $Y_F = E[\tilde{Y}_F]$. Because production takes place in country F but the patent was developed in country D, both governments have the right to levy a tax on their share of the income. We assume that the transfer pricing method that the taxpayer uses allocates to the foreign subsidiary an amount of income that compensates it for its investment, yielding it an after-tax return on its investment equal to the cost of capital. The transfer pricing method allocates the remainder, the pretax residual profit, to the domestic and foreign countries in the proportions π and $1-\pi$, $0 \leq \pi \leq 1$. We discuss how π is determined in the next section. For any given value of π, the expected annual taxable income allocated to the foreign subsidiary equals:

$$\frac{rK_F}{1-\tau_F} + (1-\pi)\Pi_F. \tag{7}$$

The term $\frac{rK_F}{1-\tau_F}$ represents the taxable income allocated to the foreign subsidiary to compensate it for its investment of K_F; an allocation of pretax income of $\frac{rK_F}{1-\tau_F}$ ensures that the after-tax return is rK_F, which ensures that the subsidiary earns the market after-tax rate of return r on its investment of K_F. The income allocated to the domestic country equals π times the annual residual income. Because the fraction of income that is allocated to the domestic country for tax purposes is taxed at the domestic tax rate τ_D, and the fraction that is allocated to the foreign country is taxed at the rate τ_F, the expected after-tax net present value from producing q_F units in perpetuity in the foreign country becomes:

$$\begin{aligned} V_F &= (1-\tau_D)\int_0^{\infty}(\pi\Pi_F)e^{-rt}dt + (1-\tau_F)\int_0^{\infty}\left(\frac{rK_F}{1-\tau_F} + (1-\pi)\Pi_F\right)e^{-rt}dt - K_F \\ &= \frac{[1-(1-\pi)\tau_F - \pi\tau_D]\Pi_F}{r} = \frac{(1-\theta)\Pi_F}{r}, \end{aligned} \tag{8}$$

where:

$$\theta = (1 - \pi)\tau_F + \pi\tau_D \tag{9}$$

denotes the combined domestic and foreign tax rate on foreign income.

The expected net present value to the firm from producing in the foreign or in the domestic country, as given by (3) and (8), respectively, depends on the corresponding production quantities q_F and q_D. The firm first observes the realized demand parameter d, then determines the production quantity that maximizes the expected net present value of production. Solving the first-order conditions for (3) in case of domestic production, and for (8) in case of foreign production shows that the optimal production quantities in case of production in the domestic and in the foreign country, respectively, are given by:

$$q_D = \frac{\tilde{d} - c_D}{2\alpha} \text{ and } q_F = \frac{\tilde{d} - c_F}{2\alpha}. \tag{10}$$

Allocation of Taxable Income

The expected net present value to the firm from production in the foreign country depends on the proportion π of the residual profit that the transfer pricing method allocates to the domestic country for tax purposes. In this section, we discuss how π is determined. We assume that the governments agree upon transfer pricing regulations that are consistent with the arm's-length standard. The profit-split π used by the firm reflects both the arm's-length standard and the extent to which the firm manages to deviate from the arm's-length standard.

Although several transfer price methods are described in sections 1.482-2 through 1.482-6 of the U.S. Treasury Regulations, the "best method rule" determines which method is consistent with the arm's-length standard (Treas. Reg. §1.482-1(c)). We assume that there is a competitive market in the foreign country for the inputs needed to manufacture the product. In that case, the income allocated to the foreign country under the arm's-length standard is the amount that is needed to provide the foreign subsidiary an after-tax rate of return equal to the cost of capital; the remainder (the residual profit) should be allocated to the domestic parent because it provides the intellectual property from winning the patent race (Treas. Reg. §1.482-1(d)(4)(ii)(D)). This is true even if production can occur more cheaply in the foreign country. Therefore, the value of π that satisfies the arm's-length standard, π_{AL}, is 1. This allocation is consistent with the CPM, in which the foreign manufacturer is the "tested party" under Treas. Reg. §1.482–5(a)(2)(i). It is also consistent with residual profit-split method in the case in which the foreign manufacturer only provides routine contributions to the business activity, and the domestic parent provides the non-routine contributions of valuable intangible property (Treas. Reg. §1.482-6(c)(3)). In each case, the foreign subsidiary is allocated just enough taxable income to compensate it for its capital investment; the remainder is allocated to the domestic parent, because it provides the intangible asset that makes the residual profit possible. We emphasize that even when the foreign subsidiary receives no part of the residual profit (i.e., when $\pi = \pi_{AL} = 1$), it is compensated for its tangible investment, receiving payments with a pretax net present value of $\frac{K_F}{1-\tau_F}$. Specifically, when the firm complies with the arm's-length standard, the after-tax expected net present value for the foreign subsidiary from engaging in the transaction with the domestic parent is given by:

$$[\frac{K_F}{1 - \tau_F} + (1 - \pi_{AL})\left(\frac{\Pi_F}{r}\right)](1 - \tau_F) - K_F = 0.$$

Because the market in the foreign country is competitive, full compliance with the arm's-length standard implies that all of the residual profits are taxed by the domestic country, and the foreign subsidiary's investment of K_F is a zero net present value investment. When $\tau_F < \tau_D$, however, the firm has an incentive to deviate from the arm's-length standard; shifting part of the residual profit to the foreign country (i.e., $\pi < 1$) reduces its tax burden. We therefore allow for the case in which the profit-split π is strictly less than 1. The further that π is away from $\pi_{AL} = 1$, the greater is the ability of the multinational firm to deviate from the arm's-length standard.

IV. PRODUCTION LOCATION

In this section, we investigate the conditions that determine whether the firm will produce domestically or in the foreign country. We also investigate whether the privately optimal location decision is economically efficient. We focus on the case in which the patent has strictly positive value regardless of whether production takes place domestically or abroad, i.e., $\Pi_D > 0$ and $\Pi_F > 0$.

Privately Optimal Location Decision

The domestic parent will produce in the foreign country if that yields a higher expected net present value for the firm than producing in the domestic country. Combining (3) and (8) shows that the domestic parent will produce in the foreign country if and only if:

$$\frac{(1-\theta)\Pi_F}{r} = \frac{[1-(1-\pi)\tau_F - \pi\tau_D]\Pi_F}{r} > \frac{(1-\tau_D)\Pi_D}{r}. \tag{11}$$

We distinguish three cases. First, when $\Pi_F \geq \Pi_D$, it follows immediately from (11) that foreign production is optimal for all π because the residual profit from domestic production is lower, and it is taxed at a higher tax rate (i.e., $\tau_D \geq \theta$).

Second, when $\Pi_F \leq \left(\frac{1-\tau_D}{1-\tau_F}\right)\Pi_D$, domestic production is preferred for all π. To see why, first consider the case in which the residual profit from foreign production would be fully taxed at the lower foreign tax rate, i.e., $\pi = 0$ and so $\theta = \tau_F$. Then, $(1-\tau_F)\Pi_F < (1-\tau_D)\Pi_D$ and (11) imply that domestic production is preferred. Thus, domestic production is preferred even when the firm manages to have all of the residual profit from foreign production taxed at the lower foreign tax rate. An increase in π would make foreign production even less attractive because it increases the fraction of income that is taxed at the higher domestic tax rate (i.e., θ increases because π increases). Therefore, domestic production is preferred for all π.

Finally, if $\left(\frac{1-\tau_D}{1-\tau_F}\right)\Pi_D < \Pi_F < \Pi_D$, the firm's optimal production location depends on π. Equation (11) shows that foreign production is preferred to domestic production if and only if:

$$\pi < \pi^* = \frac{(1-\tau_F)\Pi_F - (1-\tau_D)\Pi_D}{(\tau_D - \tau_F)\Pi_F} \in (0,1). \tag{12}$$

Foreign production is optimal if the firm's ability to deviate from the arm's-length standard is sufficiently high (i.e., if π is sufficiently low). If the allocation of taxable income is consistent with the arm's-length standard (i.e., $\pi = 1$), then domestic production is preferred. This occurs because the residual profit from foreign production is lower ($\Pi_F \leq \Pi_D$), and the arm's-length standard implies that it is taxed at the domestic tax rate (i.e., $\pi = 1$ implies $\theta = \tau_D$). In contrast, when the firm can shift all of the residual profit to the low-tax country (i.e., $\pi = 0$ and so $\theta = \tau_F$), $\Pi_F(1-\tau_F) > \Pi_D(1-\tau_D)$ implies that producing in the foreign country is optimal. Therefore, even though the residual profit from foreign production is lower than the residual profit from domestic production, foreign production is optimal if the firm's ability to deviate from the arm's-length standard is sufficiently high. We summarize these results in Table 1.

Table 1 shows that the choice to produce in the foreign country can be driven by cost differentials, tax rate differentials, or the firm's ability to deviate from the arm's-length standard.

TABLE 1

Privately Optimal Production Location

Case	Production Location
I. $\Pi_F \le \left(\frac{1-\tau_D}{1-\tau_F}\right)\Pi_D$	Domestic for all π
II. $\left(\frac{1-\tau_D}{1-\tau_F}\right)\Pi_D < \Pi_F \le \Pi_D$	$\begin{cases}\text{Domestic if } \pi \ge \pi^* \\ \text{Foreign if } \pi < \pi^*\end{cases}$
III. $\Pi_F > \Pi_D$	Foreign for all π

When the firm complies with the arm's-length standard (i.e., $\pi = 1$), it will produce in the foreign country if either lower costs and/or a lower tax rate imply that the residual profit from foreign production is higher than the residual profit from domestic production, i.e., when $\Pi_F > \Pi_D$. However, if the firm is able to deviate from the arm's-length standard, then it may be optimal to produce in the foreign country even when that yields a lower residual profit, i.e., when $\Pi_F \le \Pi_D$. This is the case when the ability to deviate is sufficiently high (i.e., when $\pi < \pi^*$), and the residual profit from foreign production is not too low compared to the residual profit from domestic production (i.e., when $\Pi_F > \left(\frac{1-\tau_D}{1-\tau_F}\right)\Pi_D$).

Social Efficiency of Production Location

In this subsection, we examine whether the production location that maximizes the firm's after-tax profits is also optimal from a social welfare perspective as of date T^*, where T^* denotes the date on which the firm wins the patent race, and chooses whether to invest domestically or in the foreign country.

We first derive the socially optimal production location. Social welfare on date T^* from producing in country $i \in \{D, F\}$ consists of the sum of after-tax firm value, the present value of taxes collected, and the present value of consumer surplus. Equivalently, social welfare equals the difference between the value received by consumers and the social cost of production, all discounted back to date T^*. The value received by consumers (SV_i) is the area under the demand curve. It can be expressed as the present value of the sum of the amount paid by consumers, $p(q_i) \cdot q_i$, and consumer surplus, $\frac{\left(\tilde{d}-p(q_i)\right)\cdot q_i}{2}$, which equals:

$$SV_i = \frac{\left(\tilde{d} - p(q_i)\right) \cdot q_i}{2r} + \frac{p(q_i) \cdot q_i}{r}.$$

The social cost (SC_i) of production in country $i \in \{D, F\}$ is the sum of the fixed production cost when production commences, K_i, and the present value of future variable production costs, $\frac{c_i \cdot q_i}{r}$,

$$SC_i = \frac{c_i \cdot q_i}{r} + K_i.$$

Taking the difference between the gross social value and social cost of production during the production stage, and using the facts that $\left(p(q_i) - c_i\right) \cdot q_i = \tilde{Y}_i$, and $\tilde{d} - p(q_i) = \alpha q_i$, yields that the social welfare (S_i) of producing in country i is given by:

$$S_i = E\left[\frac{p(q_i) \cdot q_i}{r} + \frac{\left(\tilde{d} - p(q_i)\right) \cdot q_i}{2r} - \frac{c_i \cdot q_i}{r} - K_i\right] = E\left[\frac{\tilde{Y}_i}{r} + \frac{\alpha q_i^2}{2r} - K_i\right].$$

It follows from (2), (5), and (10) that $E[\alpha q_i^2] = E[\tilde{Y}_i]$. This yields the following expressions for the social value of producing domestically (S_D) and in the foreign country (S_F):

$$S_D = \frac{3Y_D}{2r} - K_D; \tag{13}$$

$$S_F = \frac{3Y_F}{2r} - K_F. \tag{14}$$

Therefore, domestic production is socially optimal if and only if:

$$\frac{3Y_D}{2r} - \frac{3Y_F}{2r} > K_D - K_F. \tag{15}$$

We now investigate whether the firm's preferred location also maximizes the social value of production. It follows from (11) that the firm produces in the domestic country if and only if:

$$\frac{(1-\tau_D)Y_D}{r} - \frac{(1-\theta)Y_F}{r} > K_D - \left(\frac{1-\theta}{1-\tau_F}\right)K_F. \tag{16}$$

The choice to produce in the foreign country can be driven by cost considerations (lower fixed and/or variable production costs), or by tax considerations (a lower θ due to a lower value of τ_F or the ability to deviate from the arm's-length standard). As can be seen from (15), the socially optimal production location is driven entirely by cost considerations because taxes are zero sum transfers.

We first consider the case in which there are no tax rate differentials, i.e., $\tau_F = \tau_D$, so that the firm's location choice is driven by cost considerations only. When $\tau_F = \tau_D$, it follows immediately from (16) that the firm produces in the foreign country if and only if:

$$\frac{(1-\tau_D)(Y_D - Y_F)}{r} > K_D - K_F. \tag{17}$$

Comparing (15) and (17) shows that the firm's privately optimal choice need not be socially optimal. For example, if:

$$\frac{(1-\tau_D)(Y_F - Y_D)}{r} < K_F - K_D < \frac{3(Y_F - Y_D)}{2r},$$

the firm will not produce in the foreign country even though it would be socially optimal to do so.

This implies that a tax-induced choice to produce in the foreign country can be welfare-improving. To illustrate this, we consider an example in which foreign production has lower variable costs but requires a higher initial investment. Let $Y_D = 175$, $Y_F = 232$, $K_D = 600$, and $K_F = 1200$. Moreover, let $r = 10$ percent. It follows from (13) and (14) that $S_D = 2025$ and $S_F = 2280$. Social welfare is maximized when production occurs in the foreign country because $S_F > S_D$.

Whether foreign production is optimal for the firm depends on the tax rates τ_D and τ_F, and on the firm's ability to deviate from the arm's-length standard, as measured by π. We let $\tau_D = 40$ percent. This implies that the residual profit from domestic production, Π_D, equals 75 (see (4)). We consider three levels of the foreign tax rate τ_F. In each case we determine the residual profit from foreign production Π_F from (6), and then use the results from Table 1 to determine the firm's privately optimal production location for all values of π. We display the results in Table 2. We first consider the case in which the foreign tax rate equals the domestic tax rate, i.e., $\tau_F = \tau_D = 40$ percent. The residual profit from foreign production, Π_F, equals 32, which is strictly lower than the residual profit from domestic production. Because the domestic and foreign tax rates are the same, Case I from Table 1 applies, and so the firm will choose to produce in the domestic country for all values of π. The firm's optimal choice does not depend on its ability to deviate

TABLE 2

Illustrations of Privately and Socially Optimal Production Locations

τ_F	Π_F	θ	Case from Table 1	Privately Optimal Location	Socially Optimal Location
40%	32	40%	I	Domestic for all π	Foreign
25%	72	$(1-\pi)25\% + \pi 40\%$	II	Domestic if $\pi \geq \frac{5}{6}$ Foreign if $\pi < \frac{5}{6}$	Foreign
20%	82	$(1-\pi)20\% + \pi 40\%$	III	Foreign for all π	Foreign

from the arm's-length standard in this case because $\tau_F = \tau_D = 40$ percent implies that the combined tax rate θ incurred in case of foreign production is 40 percent for any value of π. Thus, with equal tax rates, the firm chooses to produce in the domestic country, which is suboptimal from a social welfare perspective because social welfare is maximized when production occurs in the foreign country.

In contrast, when $\tau_F = 20$ percent (the bottom row in Table 2), $\Pi_F = 82$, which is higher than the residual profit from domestic production. Case III from Table 1 applies, and so the firm produces in the foreign country for all π. The firm's choice again does not depend on its ability to deviate from the arm's-length standard. Although the combined tax rate θ now does depend on π, the low value of the foreign tax rate induces the firm to make the socially optimal location choice (i.e., foreign production) for all π. Finally, suppose $\tau_F = 25$ percent, which implies that $\Pi_F = 72$, which is lower than the residual profit from domestic production. Case II from Table 1 applies. In this case, the difference between Π_D and Π_F is sufficiently small that the firm will produce in the foreign country if π is sufficiently low. It will produce in the foreign country if $\pi < \pi^* = 5/6$ (see (12)), and produce domestically if $\pi \geq 5/6$. Therefore, the firm's location choice will be socially optimal if and only if the ability to deviate from the arm's-length standard is sufficiently high.

To summarize, the results from Table 2 show that a tax-induced choice to produce in the foreign country, due to either a sufficiently low foreign tax rate (when $\tau_F = 20$ percent), or the ability to deviate from the arm's-length standard (when $\tau_F = 25$ percent and $\pi < 5/6$), can improve welfare because the more favorable tax treatment mitigates the fact that, absent the tax benefit, the firm's location choice would be socially suboptimal.

V. THE PATENT RACE

In this section, we first derive the firms' privately optimal levels of R&D investments, as well as the level of R&D investments that maximizes social welfare. We also analyze how taxes affect the level and social efficiency of R&D investments, and how these effects depend on the production location.

Privately Optimal Investment in R&D

The firm that wins the patent race at time T^* earns an expected net present value of V^*, where V^* depends on whether the firm produces in the domestic or in the foreign country. Specifically:

$$
\begin{aligned}
V^* &= \frac{(1-\tau_D)\Pi_D}{r}, \text{ in case of domestic production,} \\
&= \frac{(1-\theta)\Pi_F}{r}, \text{ in case of foreign production,}
\end{aligned}
\tag{18}
$$

where the conditions for domestic and foreign production are given in Table 1, and θ is given by (9).

We let W_i be a random variable that equals 1 if firm i wins the patent race, i.e., if $T_i = T^*$, and 0 otherwise. We assume that the search times, $T_1, \ldots, T_N$, are independent of the demand parameter $\tilde{d}$, so that T^* is independent of $\tilde{d}$. Then, the net present value of investing η_i in R&D for firm i is given by:

$$
\begin{aligned}
I_i &= E[e^{-rT^*}W_i]V^* - (1-\tau_D)\eta_i = V^*\left(\int_0^\infty e^{-rt}\eta_i\lambda e^{-\lambda t\sum_{j=1}^{N}\eta_j}dt\right) - (1-\tau_D)\eta_i \\
&= \left(\frac{\lambda\eta_i}{r+\lambda\sum_{j=1}^{N}\eta_j}\right)V^* - (1-\tau_D)\eta_i,
\end{aligned}
\tag{19}
$$

where V^* is given by (18).

The N firms engage in a patent race. Specifically, firm i invests η_i in search costs, where η_i maximizes the firm's net present value from investment in R&D, given the search costs $\eta_j, j \neq i$, of the other $N-1$ firms. As in Dasgupta and Stiglitz (1980), we assume that N is sufficiently large for the effect of a marginal change in η_i on aggregate search costs $\sum_{j=1}^{N}\eta_j$ to be negligible. The first-order condition for (19) is given by:

$$
V^*\lambda = (1-\tau_D)\left(r+\lambda\sum_{j=1}^{N}\eta_j\right). \tag{20}
$$

Because all firms are identical, this implies that the optimal amount of search cost for each firm equals:

$$
\eta_i = \eta^* = \frac{1}{N}\max\left\{\frac{V^*}{1-\tau_D} - \frac{r}{\lambda}, 0\right\}, \text{ for } i = 1, \ldots, N. \tag{21}
$$

The firms will engage in a patent race if λ, the effectiveness of the search process, is sufficiently high relative to the net present value of production for the firm that wins the patent, V^*. To ensure that the after-tax value of the patent is large enough so that firms will enter the patent race, we focus on cases in which tax rates are sufficiently low and λ sufficiently high so that $\eta^* > 0$. Because $\theta \leq \tau_D$, it follows from (18) and (21) that a sufficient condition for $\eta^* > 0$ is:

$$
\lambda > \max\left\{\frac{r^2}{\Pi_D}, \frac{r^2}{\Pi_F}\right\}. \tag{22}
$$

We conclude this subsection by characterizing the net present value of R&D investments for both the winner of the patent race and for the industry as a whole. We emphasize that we focus on all of the firms in an industry that try to develop intellectual property, rather than focusing only on the firm that succeeds. The following proposition shows that this has important implications for the net present value of investment in R&D:

Proposition 1:

(i) The net present value from investment in R&D to the winner is strictly positive, i.e.,

$$E[e^{-rT^*}]V^* - (1 - \tau_D)\eta^* > 0.$$

(ii) The net present value from investment in R&D at industry level is zero, i.e.,

$$E[e^{-rT^*}]V^* - (1 - \tau_D)\eta^* N = 0.$$

The above proposition shows that although the firm that wins the patent earns a strictly positive net present value, the net present value of investment in R&D is zero at the industry level. At the industry level, the quasi-rents associated with winning the patent race are competed away at the beginning of the race, at which point there are no barriers to entry. We will show in Section VI that this result has important implications for the present value of taxes collected by the domestic government.

How Taxes Affect R&D Investments

In this subsection, we show how the domestic tax rate affects the level of investment in R&D in the cases of domestic and foreign production.

Proposition 2:

(a) If production occurs in the domestic country, then η^* is strictly decreasing in τ_D;
(b) if production occurs in the foreign country, then η^* is strictly increasing in τ_D if $\pi < 1$, and is unaffected by τ_D if $\pi = 1$.

A higher domestic tax rate makes investment in R&D less attractive in the case of domestic production, but more attractive in the case of foreign production. The reason is as follows. A higher domestic tax rate decreases the firm's expected after-tax revenues in the production stage, and its after-tax costs in the patent race stage. In case of domestic production, the after-tax benefits decrease by more than the after-tax costs because the fixed production cost is not tax deductible. Therefore, a higher domestic tax rate makes investment in R&D less attractive to the firm in the case of domestic production. In contrast, in the case of foreign production, the fixed production costs are incurred by the foreign subsidiary. Because part of the revenues are taxed by the foreign country, but the investment in R&D is fully deductible at the domestic tax rate, a higher domestic tax rate makes investment in R&D more attractive in the case of foreign production.

Efficiency of R&D Investment

In this subsection, we investigate how the effect of the domestic tax rate on the efficiency of R&D investments depends on the production location. We first derive the socially optimal investment level. When η is invested in R&D, the discovery will occur on date $T(\eta) \sim Exp(\eta\lambda)$. When production occurs in country $i \in \{D, F\}$, the socially optimal level of investment maximizes the present value of the social value of producing in country i, minus the social investment costs, η, i.e., it solves:

$$\max_{\eta}\left\{S_i \cdot E[e^{-rT(\eta)}] - \eta\right\} = \max_{\eta}\left\{\left(\frac{\eta\lambda}{r + \eta\lambda}\right)S_i - \eta\right\},$$

for $i \in \{D, F\}$, where S_D and S_F are given in (13) and (14), respectively. Differentiating with respect to η yields a unique maximum at:

$$\eta_S = \sqrt{\frac{rS_i}{\lambda}} - \frac{r}{\lambda}, i \in \{D, F\}. \tag{23}$$

Consistent with Dasgupta and Stiglitz (1980), we find that the socially optimal investment level η_S could be higher or lower than the privately optimal industry-wide investment level $N\eta^*$. Investment could be inefficiently low (i.e., $N\eta^* < \eta_S$) because profit-maximizing firms do not consider the consumer surplus when choosing their investments in R&D. On the other hand, investment could be inefficiently high. Suppose, for example, that λ is very high. Then, the primary effect of a firm's investment in R&D is to increase that firm's chances of winning the patent race, without having a significant effect on the time until the discovery occurs. This results in socially inefficient overinvestment in R&D. We show when R&D investment is inefficiently high or low in Proposition 3.

Proposition 3: Investment in R&D is inefficiently high (low) if $\lambda > \lambda^*$ ($\lambda < \lambda^*$), where:

$$\lambda^* = \lambda_D^* = \frac{r^2(3Y_D - 2rK_D)}{2\Pi_D^2} \text{ if production occurs in the domestic country;}$$

$$\lambda^* = \lambda_F^* = \frac{r^2(3Y_F - 2rK_F)}{2\Pi_F^2}\left(\frac{1-\tau_D}{1-\theta}\right)^2 \text{ if production occurs in the foreign country.}$$

The proof is in Appendix A.

The above proposition shows that regardless of the production location, the privately optimal level of R&D investment is inefficiently high (low) if the R&D process is relatively effective (ineffective), where λ represents effectiveness. Combined with Proposition 2, however, this implies that how taxes affect the efficiency of R&D investments depends on production location. Combining Propositions 2 and 3 yields the following proposition:

Proposition 4:

(a) In the case of domestic production, an increase in the domestic tax rate induces less overinvestment in R&D if $\lambda > \lambda_D^*$, and more underinvestment if $\lambda < \lambda_D^*$.
(b) In the case of foreign production, an increase in the domestic tax rate induces more overinvestment if $\lambda > \lambda_F^*$, and less underinvestment if $\lambda < \lambda_F^*$.

In the case of domestic production, a higher domestic tax rate reduces investment in R&D and, therefore, reduces overinvestment if λ is sufficiently high, and leads to more underinvestment if λ is sufficiently low. In contrast, when production occurs in the foreign country, a higher domestic tax rate increases the level of investment in R&D and, therefore, leads to more overinvestment if λ is sufficiently high, and less underinvestment if λ is sufficiently low. We summarize the effects of the domestic tax rate on the level and efficiency of R&D investments in Table 3.

VI. DOMESTIC TAX REVENUES

Our focus in Sections IV and V was on whether the privately optimal decisions were also socially optimal. However, aggregate social welfare may not be the only concern of policymakers. In this section, we examine the present value of domestic tax revenues associated with the R&D investments and subsequent production by the winner of the patent race.[4] We also investigate how the effect of the domestic tax rate on domestic tax revenues depends on the production location.

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[4] We limit our investigation to domestic corporate income taxes directly associated with the project. We do not speculate on whether other taxes would change, e.g., whether aggregate taxable labor income would increase due to the project, or instead be shifted from other activities.

TABLE 3

Effect of an Increase in τ_D on the Level and Efficiency of R&D Investment

Production Location	**Effect on the Level of R&D Investment**	**Effect on the Efficiency of R&D Investment**
Domestic production	Decreases	$\begin{cases}\text{Decreases if } \lambda < \lambda_D^* \\ \text{Increases if } \lambda > \lambda_D^*\end{cases}$
Foreign production with $\pi = 1$	No effect	No effect
Foreign production with $\pi < 1$	Increases	$\begin{cases}\text{Increases if } \lambda < \lambda_F^* \\ \text{Decreases if } \lambda > \lambda_F^*\end{cases}$

We first consider the present value of tax revenues received by the domestic government when production takes place domestically. The domestic government collects tax on the income earned from date T^* in perpetuity, and gives a tax deduction of η^* at time zero to each firm. Therefore, the net present value of the domestic government's tax revenues, X_D, equals:

$$X_D = \left[\frac{E[e^{-rT^*}]Y_D}{r} - \eta^* N\right]\tau_D. \tag{24}$$

We know from Proposition 1 that the net present value of investment in R&D is zero at the industry level. In case of domestic production, it follows from (18) that this implies that:

$$\eta^* N = \frac{E[e^{-rT^*}]\Pi_D}{r}. \tag{25}$$

Combining (24) and (25) shows that when production occurs in the domestic country, the net present value of taxes collected by the domestic government is given by:

$$X_D = \frac{E[e^{-rT^*}](Y_D - \Pi_D)\tau_D}{r} = \frac{E[e^{-rT^*}]K_D\tau_D}{1-\tau_D} > 0. \tag{26}$$

Even though the domestic government gives a deduction for the investment cost of all the firms that compete and collects tax only on the revenues of the winner, the net present value of taxes is strictly positive in case of domestic production. To see why, note that the domestic government collects taxes on the annual income Y_D as of date T^*, gives a tax deduction on date zero for the aggregate investment in R&D, $\eta^* N$, but does not give a tax deduction for the investment K_D on date T^*. Because the net present value of investment in R&D is zero at the industry level, the industry-wide deductible investment cost $\eta^* N$ equals the present value of the annual residual profit from production $\Pi_D = Y_D - \frac{rK_D}{1-\tau_D}$, which is strictly lower than annual taxable income Y_D. Therefore, the net present value of domestic tax collections is strictly positive in the case of domestic production. The domestic tax revenues are attributable to the normal return to capital from the tangible investment.

Next, we consider the tax revenues that the domestic government collects when production occurs in the foreign country. The domestic government collects tax on the fraction π of the residual profit Π_F from time T^* into perpetuity, and gives a tax deduction of η^* at time zero to each firm. Therefore, the present value of domestic government tax revenues is given by:

$$X_D = \left[\frac{E[e^{-rT^*}]\pi\Pi_F}{r} - \eta^* N\right]\tau_D. \tag{27}$$

Because the net present value of investment in R&D is zero at industry level (Proposition 1), it follows from (18) that:

$$\eta^* N = \frac{E[e^{-rT^*}]\left(\frac{1-\theta}{1-\tau_D}\right)\Pi_F}{r}. \tag{28}$$

Combining (27) and (28) shows that when production occurs in the foreign country, the net present value of taxes collected by the domestic government equals:

$$X_D = \frac{E[e^{-rT^*}]\left(\pi - \frac{1-\theta}{1-\tau_D}\right)\Pi_F \tau_D}{r} = \frac{E[e^{-rT^*}](\pi - 1)(1 - \tau_F)\Pi_F \tau_D}{r(1 - \tau_D)} \leq 0. \tag{29}$$

Comparing (26) and (29) shows that, in contrast to the case of domestic production, the net present value of domestic tax revenues in the case of foreign production is weakly negative. It is strictly negative when $\pi < 1$, and zero when $\pi = 1$. To see why, first consider the case in which the firm complies with the arm's-length standard, i.e., $\pi = \pi_{AL} = 1$. The foreign government taxes the normal return to capital on the tangible investment. The domestic country taxes 100 percent of the residual profit. Because industry-wide profits are zero, the deductible investment cost $\eta^* N$ equals the present value of the annual residual profit Π_F received as of date T^* (see Condition (28) with $\pi = 1$ so that $\theta = \tau_D$). Thus, if the firm complies with the arm's-length standard, then the present value of taxes collected on the revenues from production equals $\tau_D \eta^* N$, and so the net present value of domestic tax collections is zero. When the firm is able to deviate from the arm's-length standard, i.e., when $\pi < \pi_{AL} = 1$, the net present value of domestic tax revenues is strictly negative because the domestic country gives a tax deduction for the full investment cost, but collects taxes on only part of the residual profits.

We summarize the above results in the following proposition:

Proposition 5:

(a) If production occurs in the domestic country, then the net present value of domestic tax revenues is strictly positive; and
(b) if production occurs in the foreign country, then the net present value of domestic tax revenues is weakly negative; it is zero under the arm's-length standard, and strictly negative when the firm is able to deviate from the arm's-length standard.

The above proposition shows that when production takes place in the low-tax country, deviating from the arm's-length standard causes the present value of taxes collected by the domestic government to be strictly negative. As we illustrated in Table 2, it is possible that deviating from the arm's-length standard enhances social welfare. Therefore, deviating from the arm's-length standard has countervailing effects on domestic tax revenues and social welfare in this case.

Next, we investigate how the effect of the domestic tax rate on the present value of domestic tax revenues depends on the production location. First, consider the case of domestic production. A higher domestic tax rate has a direct effect on tax collections in the two stages. In addition, a higher domestic tax rate also affects domestic tax revenues indirectly through its effect on the amount invested in R&D. We know from Proposition 2 that a higher domestic tax rate reduces the amount invested in R&D. This affects domestic tax revenues in two ways. First, it decreases the level of the deductible investment costs. Second, it decreases the present value of tax revenues in the production stage by delaying the expected date T^* at which the patent race ends and production begins.

Now consider the case in which production takes place in the foreign country. We know from Proposition 2 that a higher level of τ_D in this case implies that more is invested in R&D. Therefore, a higher domestic tax rate not only increases the taxes collected on the income allocated to the domestic country, but also increases their present value by decreasing the expected date at which production starts. In addition, it increases both the level of the deductible investment cost and the tax rate that applies to the deduction.

In the following proposition we show that in the case of foreign production, the increase in the present value of tax revenues in the production stage is less than the decrease in tax revenues in the patent race stage, and so a higher domestic tax rate unambiguously decreases the net present value of domestic tax revenues. In the case of domestic production, the effect of a higher domestic tax rate is ambiguous, and depends jointly on the level of the domestic tax rate and the effectiveness of the R&D process, as measured by the parameter λ.

Proposition 6:

(a) If production occurs in the domestic country, then there exists a critical value $\bar{\lambda} > 0$ such that:

   (i) if $\lambda < \bar{\lambda}$, then there exists a critical value τ_D^* such that an increase in τ_D increases the net present value of domestic tax revenues if $\tau_D < \tau_D^*$, and decreases the net present value of domestic tax revenues if $\tau_D > \tau_D^*$;

   (ii) if $\lambda > \bar{\lambda}$, then an increase in τ_D increases the net present value of domestic tax revenues.

(b) If production occurs in the foreign country, an increase in τ_D decreases the net present value of domestic tax revenues if $\pi < 1$, and has no effect on the net present value of domestic tax revenues if $\pi = 1$.

Combined with Proposition 2, the above proposition implies that in the case of foreign production, a tax-induced increase in the level of R&D investments due to a higher domestic tax rate unambiguously decreases the net present value of taxes collected by the domestic government. In contrast, in the case of domestic production, the effect of a tax-induced increase in the level of R&D investments due to a lower domestic tax rate depends on the effectiveness of the R&D process.

VII. CONCLUSIONS

We examine the effects of taxes on investments in intellectual property in a setting in which the property can be developed in one country and used in another country. We develop a two-stage model in which a large number of firms engage in a patent race in stage one; in stage two, the winner of the patent race produces in either the domestic country, in which the property was developed, or in a foreign country, which has a lower tax rate.

The location decision of the firm that wins the patent race depends on the production costs, the tax rates, and the allocation of taxable income. Deviating from the arm's-length standard can cause a multinational to produce in a low-tax foreign country in a setting in which the firm would produce domestically if the allocation of taxable income were consistent with the arm's-length standard. This tax-induced shift in the location of production can be welfare improving. Weaker enforcement of the arm's-length standard may therefore improve aggregate social welfare, even though it reduces domestic tax revenues.

We also show how the effects of taxation on R&D investments depend on whether production takes place domestically or in a foreign country. First, a higher domestic tax rate decreases R&D investments in the case of domestic production, but increases R&D investments in the case of foreign production. Second, the privately optimal level of R&D investment could be higher or lower than the socially efficient level. Whether a higher domestic tax rate moves the privately optimal investment level closer to or further away from the socially optimal level depends jointly on the production location and on the effectiveness of the R&D investments, i.e., how quickly the patent is obtained for a given level of investment.

Finally, we show the effect of the production location on the domestic government's tax collections. We find that the present value of taxes collected by the domestic government is weakly negative if production occurs in the foreign country and strictly positive if production occurs domestically. In addition, a higher domestic tax rate unambiguously decreases the net present value of domestic tax revenues when production occurs in the foreign country, but has ambiguous effects on the present value of domestic tax revenues in the case of domestic production.

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APPENDIX A

Proof of Proposition 1

At the industry level, the net present value of investment in R&D is given by:

$$I_{industry} = \sum_{i=1}^{N} I_i = E[e^{-rT^*}]V^* - (1 - \tau_D)\eta^* N. \tag{30}$$

Conditional on winning the patent race, the winning firm earns an *NPV* of:

$$I_{winner} = E[e^{-rT^*}]V^* - (1 - \tau_D)\eta^*.$$

It follows from (21) that the expected discount factor is given by:

$$E[e^{-rT^*}] = \int_0^\infty e^{-rt} N\lambda\eta^* e^{-N\lambda\eta^* t} dt = \frac{N\lambda\eta^*}{r + N\lambda\eta^*} = 1 - \frac{(1 - \tau_D)r}{\lambda\ V^*}. \tag{31}$$

This implies that:

$$I_{industry} = 0, \text{ and that } I_{winner} = I_{industry} + (1 - \tau_D)\eta^*(N - 1) > 0.$$

This concludes the proof.

Proof of Proposition 2

(a) If domestic production is optimal, then:

$$V^* = \frac{(1 - \tau_D)\Pi_D}{r}, \eta^* = \frac{1}{N}\left[\frac{\Pi_D}{r} - \frac{r}{\lambda}\right], \text{ and } \frac{\partial\eta*}{\partial\tau_D} = -\frac{K_D}{N(1 - \tau_D)^2} < 0.$$

(b) If foreign production is optimal, then:

$$\eta^* = \frac{1}{N}\left[\frac{(1 - \theta)\Pi_F}{r(1 - \tau_D)} - \frac{r}{\lambda}\right] = \frac{1}{N}\left[\frac{\left(1 - \pi\tau_D - (1 - \pi)\tau_F\right)\Pi_F}{r(1 - \tau_D)} - \frac{r}{\lambda}\right].$$

Differentiation shows that $\frac{\partial\eta*}{\partial\tau_D} \geq 0$, with a strict inequality when $\pi < 1$.

Proof of Proposition 3

Follows immediately from comparing $N\eta*$ and η_S, where $N\eta*$ follows from Equations (18) and (21), and η_S follows from (13), (14), and (23).

Proof of Proposition 6

(a) We first determine the range of values of τ_D for which domestic production is optimal. If domestic production is optimal for some value of τ_D, then it is optimal for all smaller values of τ_D, i.e., there exists a τ_D^{ind} such that domestic production is optimal if $\tau_D \leq \tau_D^{\text{ind}}$.

Differentiating X_D from Equation (24) with respect to τ_D and simplifying yields:

$$sign\left[\frac{\partial X_D}{\partial\tau_D}\right] = sign\left[\Pi_D^2 - \frac{r^2}{\lambda}(Y_D - rK_D)\right].$$

The right-hand side is decreasing in τ_D. It follows from (22) that $\lambda > \frac{r^2}{\Pi_D} \geq \frac{r^2}{(Y_D - rK_D)}$. For any $\lambda > \frac{r^2}{(Y_D - rK_D)}$, it holds that:

$$\Pi_D^2 = (Y_D - rK_D)^2 > \frac{r^2}{\lambda}(Y_D - rK_D), \text{ for } \tau_D = 0,$$

$$\Pi_D^2 = 0 < \frac{r^2}{\lambda}(Y_D - rK_D), \text{ for } \tau_D = \tau_D^{\max},$$

where $\tau_D^{\max}$ is such that $\Pi_D = 0$ for $\tau_D = \tau_D^{\max}$. Therefore, $\frac{\partial X_D}{\partial \tau_D}$ has a unique root $\tau_D^*(\lambda) \in [0, \tau_D^{\max}]$, and $\frac{\partial X_D}{\partial \tau_D} > 0$ for $\tau_D < \tau_D^*(\lambda)$, and $\frac{\partial X_D}{\partial \tau_D} < 0$ for $\tau_D > \tau_D^*(\lambda)$. The root $\tau_D^*(\lambda)$ is strictly increasing in λ. Moreover, it holds that:

$$\lim_{\lambda \to \infty} \tau_D^*(\lambda) = \tau_D^{\max},$$
$$\lim_{\lambda \to [r^2/(Y_D - rK_D)]} \tau_D^*(\lambda) = 0.$$

Because $\Pi_D = 0$ for $\tau_D = \tau_D^{\max}$, and $\Pi_F > 0$, foreign production is optimal when $\tau_D = \tau_D^{\max}$. This implies that $\tau_D^{ind} \in [0, \tau_D^{\max})$. Therefore, there exists a $\bar{\lambda} \in [\frac{r^2}{(Y_D - rK_D)}, \infty)$ such that $\tau_D^*(\bar{\lambda}) = \tau_D^{ind}$. We distinguish two cases:

- if $\lambda > \bar{\lambda}$, then it holds that $\tau_D^*(\lambda) > \tau_D^{ind}$. Therefore, when domestic production is optimal, i.e., when $\tau_D \leq \tau_D^{ind}$, it holds that $\tau_D < \tau_D^*(\lambda)$ so that $\frac{\partial X_D}{\partial \tau_D} > 0$.
- If $\lambda < \bar{\lambda}$, then it holds that $0 \leq \tau_D^*(\lambda) \leq \tau_D^{ind}$, and an increase in the domestic tax rate increases the present value of domestic tax revenues if $\tau_D \leq \tau_D^*(\lambda)$, but decreases the present value of domestic tax revenues if $\tau_D \geq \tau_D^*(\lambda)$.

(b) Next, we consider the case of foreign production. An increase in the domestic tax rate increases η^*, which implies that $E[e^{-rT^*}]$ increases. Because $\frac{\tau_D}{1-\tau_D}$ is increasing in τ_D, it holds that:

$$E[e^{-rT^*}]\frac{(1-\pi)(1-\tau_F)\tau_D}{(1-\tau_D)},$$

is a product of nonnegative increasing terms, and is therefore increasing in τ_D. It therefore follows immediately from (29) that X_D is weakly decreasing in τ_D, and is strictly decreasing in τ_D if $\pi < 1$.

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Do Managerial Incentives Drive Cost Behavior? Evidence about the Role of the Zero Earnings Benchmark for Labor Cost Behavior in Private Belgian Firms

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ABSTRACT: This study investigates the influence of managerial incentives to meet or beat the zero earnings benchmark on labor cost behavior of private Belgian firms. We posit that relative to managers of firms reporting healthy profits, managers meeting or beating the zero earnings benchmark will increase labor costs to a smaller extent when activity increases and decrease labor costs to a larger extent when activity decreases. This should take the form of more symmetric labor cost behavior for firms that report a small profit. Our findings are consistent with this prediction. Using detailed employee data, we show that managers of firms reporting a small profit focus on firing employees who are relatively low cost to fire. To protect their reputation in the labor market, managers of other firms, particularly those reporting healthy profits, limit the numbers of dismissals and react to activity changes by changing the number of hours that employees work.

Keywords: *cost asymmetry; real activities; labor costs; employee data.*

Data Availability: *Data are available from the sources referred to in the text.*

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I. INTRODUCTION

Traditional cost models assume that variable costs change symmetrically and proportionally with changes in activity levels. Recent empirical research, however, documents asymmetry in cost behavior in which costs increase more rapidly with an activity increase than they decrease with an activity decrease (Anderson et al. 2003; Balakrishnan et al. 2004). Anderson et al. (2003) and Noreen and Soderstrom (1997) suggest that asymmetric cost behavior arises because managers make rational decisions as they trade off the costs of resource adjustments against the costs of keeping slack resources. It can also arise because managers perceive the costs of downward adjustments to be higher than those of upward adjustments.

Although managerial behavior can be an important driver of cost asymmetry, empirical evidence about this conjecture is only beginning to emerge. The few studies that exist generally use aggregate data for publicly traded U.S. firms, making it difficult to address how specific costs are managed. In particular, because these studies often rely on aggregate proxies for discretionary costs, such as selling, general, and administrative costs, it is difficult to infer which managerial decisions lead to variations in cost behavior. In addition, because managers of publicly traded firms often face a more complex array of incentives than those faced by managers of private firms, it is more difficult to infer whether specific managerial incentives affect how particular costs are managed.

In this study, we examine whether and how managerial incentives to meet or beat the zero earnings benchmark affect cost behavior in private Belgian firms, resulting in predictable differences in the degree of labor cost asymmetry for firms that do and do not face the benchmark. The unique institutional setting of this study provides us with several advantages relative to studies that examine cost behavior in publicly traded U.S. firms. First, private Belgian firms must disclose disaggregated cost information, including total labor costs, number of employees, and total number of hours worked. These data enable us to study cost behavior for labor cost, which is commonly the largest expense on the income statement. Furthermore, having specific information regarding the components of labor cost, including number of employees, number of hours worked, and the composition of the labor force, i.e., number of blue-collar workers and number of white-collar workers, permits us to examine which specific actions managers take when managing labor costs. Second, because managers of private Belgian firms have unusually strong incentives to avoid losses for reasons we explain more fully below, we have a powerful setting in which to study how the incentive to meet or beat the specific zero earnings benchmark affects discretionary decisions regarding labor costs. Third, studying cost behavior in private Belgian firms mitigates certain issues of potential goal incongruence between managers and shareholders that arise in publicly traded firms. This enables us to make predictions of how the managerial incentive to meet or beat the zero earnings benchmark affects discretionary labor costs with limited contamination from the effects of shareholder-manager agency costs.

Two characteristics of the Belgian institutional environment motivate the importance of the zero earnings benchmark. First, meeting the zero earnings benchmark determines whether private Belgian firms are allowed to pay dividends to the owners and cash bonuses to the managers. Second, auditors are required to issue a going-concern opinion when Belgian firms report a loss during two consecutive years, and this opinion often results in bank intervention. Because debt financing of private firms in Belgium is heavily bank-oriented, firms have an interest in avoiding a going-concern opinion and have strong incentives to report a profit in the year following a reported loss. Given the importance of the zero earnings benchmark for private Belgian firms, we posit that the incentive to meet or beat the zero earnings benchmark increases the costs of upward resource adjustments and decreases the costs of downward resource adjustments. Hence, managers who want to avoid a loss will be more willing to cut labor costs when sales decrease and to limit the increase in labor costs when sales increase. As a result, we expect the incentives to meet or beat the zero earnings benchmark to offset the forces that lead to asymmetric cost behavior. This leads us to predict that, relative to other firms, firms that just meet the zero earnings benchmark will exhibit a more symmetric labor cost pattern.

Because private Belgian firms are required to disclose detailed information about the composition of their workforce and their employee decisions in their publicly available financial statements, studying labor cost behavior in private Belgian firms enables us to gain insights regarding managerial decisions in response to activity changes in two different ways. First, the detailed data allow us to measure empirically the richness in managerial responses to activity changes and whether managers react differently when facing the zero earnings benchmark. Adapting labor costs to activity changes can affect the number of hours each employee works or the number of individuals the firm employs. Given the high costs associated with firing employees, including severance costs and loss of reputation in the labor market, we expect that only managers facing a strong need for immediate cost reduction, particularly managers who require cost adjustments to meet or beat the zero earnings target, will fire employees in response to activity decreases.

Second, we examine whether managers take into account variations in severance costs of employees when adjusting their labor costs. Belgium is an ideal institutional setting to investigate this issue because Belgian labor law draws a strict distinction with respect to termination provisions between blue-collar and white-collar workers. Blue-collar workers receive shorter notice of termination compared to white-collar workers, and redundancy payments are lower when dismissing blue-collar workers compared to white-collar workers.[1] Therefore, firing blue-collar workers is a relatively low-cost means to realize immediate cost reductions and should be utilized more frequently by firms facing incentives to meet or beat the zero earnings benchmark.[2]

Using a sample of 37,880 firm-year observations over the period 1995–2006, we first document significant labor cost asymmetry for a broad cross-section of private Belgian firms. A 1 percent increase in revenues leads to a 0.60 percent increase in labor costs, and a 1 percent decrease in revenues is associated with a 0.34 percent decrease in labor costs. We next provide evidence supporting our key hypothesis that firms that just meet or beat the zero earnings benchmark exhibit less cost asymmetry relative to other firms. That is, they spend less on resources in response to sales increases and hoard fewer resources in response to sales decreases. To further examine the role of earnings targets on cost behavior, we partition our sample into different subsamples. Subsample analysis indicates that firms that just meet or beat the zero earnings benchmark actually exhibit cost symmetry, i.e., the change in labor costs in response to an activity change does not differ between an activity increase and activity decrease. In contrast, firms that report a small loss or a large profit experience cost asymmetry. We also test our hypothesis by restricting our sample to observations relating to those firms that meet or beat the zero earnings benchmark at least once during our sample period. We find that these firms exhibit a more symmetric cost pattern only in the years that they report a small profit. Our results are robust to the inclusion of a control for the amount of accrual-based earnings management and other control variables. Collectively, these results provide support for the hypothesis that firms that meet or beat the zero earnings benchmark limit the increase in labor costs when activity increases and are more willing to cut labor costs when activities decrease.

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[1] The redundancy payment typically consists of the salary that the employee would have earned during the period of notice. Although the period of notice depends upon the seniority of the employee, the period of notice of a blue-collar worker is at most four weeks while the period of notice of a white-collar worker is at least three months.

[2] Because blue-collar workers are easier and cheaper to fire, firms have tried to exploit this characteristic of blue-collar workers to manage labor costs during the recent economic crisis. For example, the Belgian division of the U.S. firm Brink's, a firm that provides global security services such as cash handling, proposed changing the contracts of the employees responsible for cash transportation from a white-collar contract to a blue-collar contract to increase the firm's flexibility with respect to labor costs. Brink's management contends that this change is necessary to save the firm from bankruptcy because firing a large number of white-collar workers would saddle the firm with so much additional cost that the firm would be forced into bankruptcy (*De Standaard*, January 12, 2011).

Next, we examine the underlying managerial decisions for the observed cost patterns and find that managerial decisions are consistent with economic incentives. Specifically, when faced with decreasing sales, firms that report a healthy profit reduce the number of hours that employees work rather than the number of employees. In contrast, firms that report a small profit are more likely to terminate blue-collar workers who are the least costly to fire. These findings are consistent with small-profit firms facing a more immediate need to cut labor costs and being less concerned about loss of reputation in the labor market.

Our study extends the asymmetric cost literature in two ways. First, we present evidence that variations in cost behavior are driven by managerial incentives. Although Chen et al. (2012) and Kama and Weiss (2010) consider the impact of managerial incentives on cost behavior, these studies do so in a setting in which there is a clear separation between ownership and decision-making. In contrast, we examine the influence of managerial incentives on cost behavior in private firms. Although public and private firms share common objectives, such as avoiding the violation of debt covenants, private firms are not exposed to the disciplining force of the capital markets, thereby making it easier to infer whether specific managerial incentives affect how particular costs are managed. Second, our analysis of detailed employee data illuminates how managers behave when adjusting labor costs and shows that variations in cost asymmetry are driven by different managerial decisions. This analysis emphasizes the link between the literature on the use of real activities to meet earnings benchmarks and the literature on asymmetric cost behavior.

This remainder of this paper is organized as follows. Section II discusses the relevant literature and develops the hypotheses. Section III describes the research method. Section IV discusses the findings and provides a number of sensitivity analyses.

II. LITERATURE OVERVIEW AND HYPOTHESES

Cost Asymmetry

Traditional cost accounting models assume that variable costs change proportionally and symmetrically with changes in the activity driver, regardless of the direction of the activity change (e.g., Noreen 1991). In contrast, models of asymmetric cost behavior postulate that managers influence resource adjustments, resulting in asymmetric cost behavior. To the extent that managers perceive that adjustment costs are greater for a downward adjustment than for an upward adjustment, costs will fall less with activity decreases than they will rise with activity increases.

Recent large-sample empirical evidence documents asymmetric cost behavior. Anderson et al. (2003) find that selling, general, and administrative (SG&A) costs increase on average by 0.55 percent for a 1 percent increase in sales but decrease only by 0.35 percent for a 1 percent decrease in sales. Anderson et al. (2003) also show that the degree of cost asymmetry depends on the expected level of adjustment costs, which varies systematically across firms and over time.

Anderson et al. (2003) have stimulated research in the area of cost asymmetry along three dimensions. A first area examines the link between variables that influence adjustment costs and the degree of cost asymmetry (e.g., Balakrishnan et al. 2004; Calleja et al. 2006). A second area addresses the moderating influence of structural factors on the degree of cost asymmetry (e.g., Banker and Chen 2006; Balakrishnan and Gruca 2008). A third, less explored area involves the impact of managerial incentives on cost asymmetry. Chen et al. (2012) investigate managerial empire-building incentives as a determinant of cost asymmetry. The study finds that empire-building incentives are positively related to the degree of cost asymmetry and that good corporate governance mitigates this relationship. Kama and Weiss (2010) examine the influence of incentives to meet earnings targets and technological constraints on cost asymmetry in listed U.S. firms. Our study extends this third line of research by examining the influence of incentives to meet or beat the zero earnings benchmark on cost asymmetry of labor costs in private Belgian firms. By focusing on discretionary labor costs in this unique institutional setting, we are able to shed light on the underlying managerial decisions to change costs in response to activity changes.

Earnings Management

A large number of studies examine the reasons why firms manage their earnings (e.g., Burgstahler and Dichev 1997; Degeorge et al. 1999). The evidence shows that firms manage earnings to avoid small losses and earnings decreases, to meet analysts' earnings forecasts, to avoid breaching debt covenants, or to show a smooth earnings pattern. Most of the evidence in these studies relates to publicly traded firms that face the disciplining force of capital markets, although some of the objectives are common to private firms, such as avoiding violation of debt covenants.

The literature identifies two main strategies to manage reported earnings: (1) accrual-based earnings management that does not affect cash flows and (2) real actions that affect cash flows. Recent research has focused on how firms manage earnings by manipulating real activities (e.g., Roychowdhury 2006; Gunny 2010). For example, Roychowdhury (2006) shows that firms use real activities to meet financial reporting targets, such as providing price discounts to temporarily boost sales, reducing discretionary expenditures to improve reported margins, and overproducing to lower the cost of goods sold. Other studies document that managers cut strategic investments, such as R&D investments, to meet earnings targets (Baber et al. 1991; Garcia Osma and Young 2009).

Cost Asymmetry and Earnings Management in Private Belgian Firms

One important real earnings management tool is managing labor costs. Indeed, labor costs are subject to managerial discretion and represent a significant cost to the firm, especially for private firms (Anderson and Lanen 2007).[3] Furthermore, relative to investments in fixed or other tangible assets, investments in employees are generally more divisible and reversible, although they may have other costs, including hiring and firing costs, loss of reputation in the labor market, and reduced morale for the remaining employees (Pindyck 1988). In other words, firms will adjust labor costs when the benefits of doing so outweigh the associated costs. We posit that the zero earnings benchmark provides managers with particularly strong incentives to manage labor costs.

The zero earnings benchmark is an important benchmark for private Belgian firms for several reasons. First, meeting or beating the zero earnings target helps to avoid violating debt covenants and to prevent banks from intervening in the management of the firm (Kamin and Ronen 1978; Danos et al. 1989; Graham et al. 2005). Second, reporting a positive profit not only conveys an important signal to other stakeholders, such as employees and suppliers (Bowen et al. 1995; Burgstahler and Dichev 1997), but also is a necessary legal condition for Belgian firms to be able to pay dividends to owners and cash bonuses to managers. Finally, when Belgian firms report a loss in two consecutive years, the external audit firm must issue a going-concern opinion, which can damage the firm's creditworthiness. Taken together, private Belgian firms have numerous reasons to avoid reporting a loss, which is consistent with previous research documenting that private Belgian firms manipulate accruals to avoid reporting a loss (Coppens and Peek 2005).

We hypothesize that private Belgian firms will manage labor costs to avoid reporting a loss. In particular, Belgian firms will be more willing to cut labor costs when sales decrease and less willing to increase labor costs when sales increase if these actions enable firms to avoid reporting a loss. If so, then the results should include a lower degree of cost asymmetry, i.e., a more symmetric cost pattern, in firms that meet or beat the zero earnings benchmark (hereafter, small-profit firms) compared to other firms.

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[3] For many businesses, especially those in the service industry, such as restaurants, or manufacturing firms that are labor intensive, labor costs are the largest line item on the income statement. In our sample, on average 27 percent of total revenues is spent on labor costs, ranging from 8 percent to almost 87 percent.

> **H1:** Small-profit firms exhibit a lower degree of labor cost asymmetry, i.e., *ceteris paribus*, they exhibit a reduced response to sales increases and an increased response to sales decreases.

Mechanisms for Adjusting Labor Costs

Firing employees imposes various costs on Belgian firms, including redundancy payments, loss of morale of the remaining employees, and loss of reputation in the labor market. As a result, managers of firms that report a large profit (hereafter, large-profit firms) are less likely to dismiss employees when faced with an activity decrease. These termination costs lead to our prediction that large-profit firms will change the number of employees asymmetrically in response to changes in activity levels. They will increase the number of employees more when activity levels increase than they will decrease the number of employees when activity levels decrease. Rather than firing employees, managers of large-profit firms are more likely to reduce the number of hours that employees work when faced with an activity decrease. Thus, we predict the number of hours per employee to decrease more when activity levels decrease than they increase when activity levels increase. In activity-increasing periods, large-profit firms will meet labor needs by hiring workers rather than by increasing the number of hours per employee.

In contrast, managers of firms seeking to avoid reporting a loss are less concerned about costs such as loss of morale of the remaining employees and loss of reputation in the labor market and, therefore, are more likely to revert to measures that result in significant cost reductions, such as employee dismissals. As a result, we predict that managers of small-profit firms will adjust both the number of employees and the number of hours per employee symmetrically in response to changes in activity levels.

Because large-profit firms are less likely to fire employees in response to activity declines, we predict that the number of blue-collar and white-collar workers will exhibit similar asymmetric changes in response to activity changes. In contrast, we predict that small-profit firms will dismiss employees, particularly blue-collar workers, to manage their labor costs. Thus, for small-profit firms, we predict that the number of blue-collar workers will exhibit symmetric changes in response to activity changes, but the number of white-collar workers will exhibit asymmetric changes.[4]

III. METHOD

Sample Selection

We obtain our sample from the Bel-First database of Bureau Van Dijk, which consists of all Belgian firms that are unlisted and have at least 50 employees.[5] Our sample covers the years 1993 through 2006 and consists of 51,826 firm-year observations. Table 1, Panel A, describes how we construct our final sample. As in Anderson et al. (2003) and Chen et al. (2012), we require labor costs and sales revenues to be available in the current and previous year and labor costs to be less

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[4] Our prediction depends on whether the labor cost savings from firing one higher paid white-collar worker relative to one blue-collar worker is sufficiently smaller than the relatively smaller redundancy costs for blue-collar workers. However, because the period of notice and redundancy payments for the white-collar workers are substantially larger than those for blue-collar workers, white-collar workers should be fired earlier in the fiscal year to be beneficial for managing earnings upward. Because managers prefer to have good information regarding the firm's expected financial performance before taking any action and such information becomes clearer later in the fiscal year, we predict that blue-collar workers are more susceptible to be fired to meet or beat the zero earnings benchmark.

[5] By requiring firms to have at least 50 employees, we eliminate small firms from the sample. These firms are not required to have their financial statements audited.

TABLE 1

Sample Selection

Panel A: Full Sample

	Number of Firm-Years
Potential Sample[a]	51,826
Less: Observations with missing data on either sales revenue or labor costs for the current or preceding year	(9,051)
Less: Observations for which labor costs exceed sales revenue for the current year	(324)
Less: Observations from listed firms	(535)
Less: Observations with missing data on other variables	(3,190)
Less: Trim top and bottom of change in sales and change in labor costs at 0.5 percent level	(846)
Final Sample	37,880

Panel B: Distribution of Firm-Year Observations over Sample-Years

Sample-Year	Large-Loss Sample	Small-Loss Sample	Small-Profit Sample[b]	Large-Profit Sample	Full Sample
1995	486	162	515	1,821	2,984
1996	469	172	551	1,802	2,994
1997	420	148	497	1,959	3,024
1998	436	138	476	1,959	3,009
1999	515	141	451	1,953	3,060
2000	524	158	455	1,918	3,055
2001	645	168	454	1,801	3,068
2002	675	188	481	1,769	3,113
2003	450	112	252	1,694	2,508
2004	600	146	304	2,568	3,618
2005	619	116	297	2,665	3,697
2006	539	135	334	2,742	3,750
Total	6,378	1,784	5,067	24,651	37,880

[a] Firms need to satisfy two criteria in order to be included in the sample: they should be unlisted and have at least 50 employees. We obtain this sample from the Bel-first database of Bureau Van Dijck over the period 1993–2006.

[b] We label "small-profit" firm-years as those relating to firm-years for which net income scaled by total assets is greater than or equal to 0 but less than 1 percent (Roychowdhury 2006). "Small-loss" firm-years are those firm-years for which net income scaled by total assets is smaller than 0 but greater than −1 percent. "Large-profit" firm-years are all profitable firm-years that are not "small-profit" and "large-loss" firm-years are all loss-making firm-years that are not "small-loss."

than sales. Also, following Chen et al. (2012), we delete observations for which changes in sales and labor costs are in the top and bottom 0.5 percent. This results in a final sample of 37,880 firm-year observations between 1995 and 2006.[6]

Following prior research, we define firms suspected of managing their earnings to meet or beat the zero earnings benchmark as consisting of firm-year observations for which the net income as a percentage of beginning-of-year total assets is larger than or equal to zero but smaller than 1 percent (Roychowdhury 2006; Cohen et al. 2008). This is our so-called *small-profit* sample. Our *large-*

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[6] Because we require sales data to be available for the two prior years, we lose observations from 1993 and 1994.

profit sample includes all other firm-year observations that report a profit. We classify firm-years that fall in the area immediately to the left of zero as *small-loss* observations, i.e., firm-year observations for which the net income as a percentage of beginning-of-year total assets is less than zero but greater than −1 percent. We classify all other loss-making firm-year observations as *large-loss* observations. Table 1, Panel B, presents the distribution of the firm-year observations over the sample-years for the full sample and various subsamples.

Model Specification and Variable Definitions

Asymmetry of Labor Costs

To test for labor cost asymmetry, we use models similar to those in Anderson et al. (2003). The basic model is given by Equation (1):

$$\log\left(\frac{LaborC_{it}}{LaborC_{it-1}}\right) = \beta_0 + \beta_1 \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) + \beta_2 Decrease\_Dummy_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) + \varepsilon_{it}, \tag{1}$$

where $LaborC_{it}$ denotes labor costs of firm i in year t; $Sales_{it}$ are sales revenues; and $Decrease\_Dummy_{it}$ is an indicator variable set equal to 1 when sales in year t are smaller than sales in year t–1, and 0 otherwise.

The coefficient, β_1, is the percentage increase in labor costs for a 1 percent increase in sales; the sum of β_1 and β_2 represents the percentage decrease in labor costs for a 1 percent decrease in sales. Based on prior literature, we expect β_1 to be positive, β_2 to be negative, and the absolute value of β_1 to be greater than the absolute value of β_2 for the full sample of private Belgian firm-years. To control for error dependence of firm observations, we use Rogers (1993) standard errors clustered at the firm level in all regressions (Petersen 2009).

Compared to the basic model, the extended model given by Equation (2) includes controls for economic determinants of cost asymmetry identified in prior research (Anderson et al. 2003; Pinnuck and Lillis 2007) and the extent of accrual-based earnings management:

$$\begin{aligned}
\log\left(\frac{LaborC_{it}}{LaborC_{it-1}}\right) = {} & \beta_0 + \beta_1 \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) + \beta_2 Decrease\_Dummy_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) \\
& + \beta_3 Abnor\_Accruals_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) \\
& + \beta_4 Decrease\_Dummy_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) * Successive\_Decrease_{it} \\
& + \beta_5 Decrease\_Dummy_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) * Employee\_Intensity_{it} \\
& + \beta_6 Decrease\_Dummy_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) * Asset\_Intensity_{it} \\
& + \beta_7 Decrease\_Dummy_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) * Economic\_Growth_{t} \\
& + \beta_8 Decrease\_Dummy_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) * Loss\_Prior\_Year_{it} \\
& + \beta_9 Decrease\_Dummy_{it} * \log\left(\frac{Sales_{it}}{Sales_{it-1}}\right) * Abnor\_Accruals_{it} \\
& + \beta_{10} Successive\_Decrease_{it} + \beta_{11} Employee\_Intensity_{it} \\
& + \beta_{12} Asset\_Intensity_{it} + \beta_{13} Economic\_Growth_{t} + \beta_{14} Loss\_Prior\_Year_{it} \\
& + \beta_{15} Abnor\_Accruals_{it} + \varepsilon_{it}.
\end{aligned} \tag{2}$$

Successive_Decrease equals 1 if sales have decreased in two consecutive years (i.e., $Sales_{t-2} > Sales_{t-1} > Sales_t$), and 0 otherwise. *Employee_Intensity* is the ratio of total number of employees over sales, while *Asset_Intensity* is the ratio of total assets over sales. *Economic_Growth* is growth in real GNP in year *t*. *Loss_Prior_Year* indicates whether the firm reported, or did not report, a loss in the prior year. All other variables are as previously defined. We include the economic determinants as main terms and as three-way interaction variables to model their moderating behavior with regard to decreases in sales. Based on prior research, we do not expect these variables to have an impact on increases in sales and therefore we do not include them in a two-way interaction with the log of the sales ratio.[7]

In addition to the economic determinants used in prior literature, we include a control for the amount of accrual-based earnings management (*Abnor_Accruals*) because managers can also manipulate accruals to meet or beat the zero earnings benchmark. We use the DeFond and Park (2001) model as adapted by Francis and Wang (2008) and include signed abnormal accruals as a main term and add two- and three-way interaction terms. Prior research documents that real and accrual-based earnings management are negatively correlated (Zang 2012; Cohen and Zarowin 2010), which suggests that firms that manipulate their accruals more can manage their labor costs less. On the other hand, if firms want to meet or beat the zero earnings benchmark, then they can use both mechanisms to avoid reporting a loss. Therefore, we make no predictions with regard to the sign of the coefficients on variables relating to abnormal accruals.

Cost Asymmetry and Incentives to Report a Small Profit

We use three approaches to investigate whether small-profit observations exhibit a lower degree of labor cost asymmetry. First, we add to Equation (2) an indicator variable, *Small_Profit*, which equals 1 for firm-year observations that report a small profit and 0 otherwise, and include two-way ($Small\_Profit * \text{Log}\,(Sales_{it}/Sales_{it-1})$) and three-way ($Small\_Profit * Decrease\_Dummy * \text{Log}\,(Sales_{it}/Sales_{it-1})$) interaction terms. This permits Equation (2) to be estimated using the entire sample while permitting coefficients to differ for small-profit and other firms. Specifically, the two-way (three-way) interaction term represents the difference in labor cost increase (decrease) following an activity increase (decrease) of small-profit firms relative to other firms. We predict a significantly negative coefficient on the two-way interaction term, i.e., small-profit firms will have a smaller increase in labor costs for a 1 percent increase in sales. We also predict a significantly positive coefficient on the three-way interaction term, i.e., small-profit firms will have a larger decrease in labor costs for a 1 percent decrease in sales.

Second, we use split-sample regressions, i.e., we estimate the basic and extended model separately for the small-profit, large-profit, and small-loss samples. We do not estimate regressions for firms with large losses, because such firms are likely to restructure or to manage earnings using "big bath" accounting, reflecting one-time charges against income, which makes it difficult to compare their cost behavior with that of other observations. We predict small-profit firms will exhibit more cost symmetry than large-profit firms. In comparison to large-profit observations, we predict β_1 to be smaller and β_2 to be less negative or insignificantly different from zero for small-profit observations. That is, firms that seek to meet or beat the zero earnings benchmark and that face an increase (decrease) in sales, will increase (decrease) labor costs to a smaller (larger) extent than large-profit firms. Our expectations for the small-loss observations are less clear because small-loss observations could include observations relating to firms that attempted to report a small profit but did not succeed or observations relating to firms that do not have incentives to report a small profit. Although the former group should exhibit a cost pattern similar to that of small-profit observations, the latter group should exhibit an asymmetric cost pattern. Given the heterogeneity of the small-loss sample, we therefore expect a more symmetric cost pattern for the small-profit sample compared to the small-loss sample.

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[7] Nonetheless, we also estimated Equation (2) including two-way interaction terms in the regression models. However, because the inclusion of all two-way interaction terms simultaneously leads to multicollinearity and spurious regression results, we include the two-way interactions terms separately in sequential estimations of Equation (2). Untabulated findings result in inferences identical to those in the tabulated results.

In a third test, we restrict our sample to observations of firms that have reported a small profit at least once during the period 1995–2006. As with the extended model regressions, we predict a significantly negative coefficient on the two-way interaction term, i.e., small-profit firm-years will have a smaller increase in labor costs for a 1 percent increase in sales relative to non-small-profit firm-years, and a significantly positive coefficient on the three-way interaction term, i.e., small-profit firm-years will have a larger decrease in labor costs for a 1 percent decrease in sales relative to non-small-profit firm-years. An advantage of this third analysis is that each firm serves as its own control, thereby mitigating the effects on our inferences in unmodeled differences in firm characteristics between small-profit and large-profit firms.

IV. RESULTS

Descriptive Results

Table 2 provides descriptive statistics for the different subsamples. Because our analyses focus on comparisons of large-profit and small-profit firms, we describe only the summary statistics for these subsamples. On average, the large-profit observations have sales revenues of €54,792,680 and labor costs of €9,023,870. In comparison, the small-profit firm-years have on average lower sales revenues and labor costs, €46,247,380 and €8,662,690. The average ratio of labor costs to sales revenues is 26 (27) percent for the large-profit (small-profit) subsample, indicating that labor costs are an important cost category for both sets of firms. The average number of employees is almost 200 for both subsamples. Mean beginning-of-year total assets is lower for the large-profit observations, €53,308,310 versus €75,187,500, versus for small-profit observations. Table 2 also reveals that large-profit observations have a lower probability of a decrease in sales, 27 percent versus 36 percent for small-profit observations, a lower probability of a successive decrease in sales, 9 percent versus 12 percent, and a lower probability of having recorded a loss in the previous year, 11 percent versus 21 percent. Abnormal accruals do not differ significantly across the subsamples. Finally, untabulated statistics reveal no significant differences with regard to the distribution across industries between the different samples.[8]

Results of Hypothesis Tests

Asymmetry of Labor Costs

Columns I and II of Table 3 present the regression summary statistics for the basic and extended models, Equations (1) and (2), for the full sample of 37,880 firm-year observations. The results of the basic model show significant labor cost asymmetry in private Belgian firms with $\beta_1 = 0.64$ ($t = 19.44$) and $\beta_2 = -0.22$ ($t = -4.47$). After controlling for other determinants of cost asymmetry by means of the extended model, we still find significantly asymmetric labor costs ($\beta_1 = 0.60$, $t = 15.93$; $\beta_2 = -0.26$, $t = -3.66$). Hence, an increase in sales revenues of 1 percent leads to an

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[8] As a robustness check, we add industry indicators to all estimated regressions. Untabulated findings reveal that none of the inferences differ from those relating to tabulated results.

TABLE 2

Sample Descriptive Statistics

Variable		Full Sample	Large-Profit Sample	Small-Profit Sample	Small-Loss Sample
Sales (in €000)	Mean	52,658.43	54,792.68	46,247.38	52,446.15
	Median	20,110.50	21,377.00	18,064.00	17,328.50
	Std. Dev.	133,332.03	132,715.69	124,638.91	132,006.85
LaborC (in €000)	Mean	9,040.36	9,023.87	8,662.69	9,053.99
	Median	4,186.00	4,226.00	3,777.00	3,998.00
	Std. Dev.	23,377.27	21,545.72	22,729.14	33,961.92
Change_Sales (in €000)	Mean	2,846.51	3,420.72	2,195.96	1,968.38
	Median	813.00	1,087.00	586.00	345.00
	Std. Dev.	13,161.80	13,158.18	12,187.19	13,017.49
Change_LaborC (in €000)	Mean	402.60	437.01	322.83	351.68
	Median	167.00	189.00	132.00	139.00
	Std. Dev.	1,676.63	1,646.69	1,562.85	1,550.03
Relative_Change_Sales (in %)	Mean	0.21	0.26	0.09	0.05
	Median	0.07	0.09	0.04	0.02
	Std. Dev.	8.37	10.15	0.48	0.37
Relative_Change_LaborC (in %)	Mean	0.05	0.06	0.02	0.01
	Median	0.01	0.01	0.01	0.01
	Std. Dev.	2.42	2.90	0.10	0.11
LaborC/Sales (in %)	Mean	0.27	0.26	0.27	0.28
	Median	0.23	0.22	0.24	0.26
	Std. Dev.	0.18	0.17	0.18	0.18
Number_of_Employees	Mean	200.17	198.05	196.03	214.26
	Median	97	96	93	97
	Std. Dev.	580.33	526.24	500.16	1,037.49
Beginning_Total_Assets (in €000)	Mean	54,782.81	53,308.31	75,187.50	64,974.88
	Median	12,912.00	12,685.00	13,707.00	13,872.00
	Std. Dev.	256,740.14	268,367.40	318,478.70	217,478.63
Decline_Observations	Mean	0.32	0.27	0.36	0.41
	Median	0.00	0.00	0.00	0.00
	Std. Dev.	0.47	0.44	0.48	0.49
Successive_Decrease	Mean	0.12	0.09	0.12	0.15
	Median	0.00	0.00	0.00	0.00
	Std. Dev.	0.32	0.28	0.33	0.36
Employee_Intensity	Mean	0.01	0.01	0.01	0.01
	Median	0.01	0.01	0.01	0.01
	Std. Dev.	0.01	0.02	0.01	0.02
Asset_Intensity	Mean	1.85	1.84	2.93	1.66
	Median	0.63	0.60	0.68	0.72
	Std. Dev.	17.25	18.90	21.52	5.89
Economic_Growth	Mean	2.26	2.29	2.39	2.21
	Median	2.38	2.38	2.38	2.01
	Std. Dev.	0.97	0.96	1.01	1.00
Loss_Prior_Year	Mean	0.22	0.11	0.21	0.40
	Median	0.00	0.00	0.00	0.00
	Std. Dev.	0.41	0.31	0.41	0.49

(continued on next page)

TABLE 2 (continued)

Variable		**Full Sample**	**Large-Profit Sample**	**Small-Profit Sample**	**Small-Loss Sample**
Abnor_Accruals	Mean	0.01	0.00	0.00	0.01
	Median	0.00	0.00	0.00	0.00
	Std. Dev.	0.32	0.32	0.25	0.27
Observations		37,880	24,651	5,067	1,784

This table presents descriptive statistics for the full sample and subsamples over the period 1995–2006. See Table 1 for definitions of the subsamples.

Variable Definitions:
LaborC = labor costs;
Sales = sales revenue;
Change_LaborC = change in labor costs between year *t* and year *t*–1;
Change_Sales = change in sales between year *t* and year *t*–1;
Relative_Change_LaborC = ratio of change in labor costs between year *t* and year *t*–1 to beginning total assets of year *t*;
Relative_Change_Sales = ratio of change in sales between year *t* and year *t*–1 to beginning total assets of year *t*;
LaborC/Sales = ratio of labor costs to sales revenue;
Number_of_Employees = number of employees in year *t*;
Beginning_Total_Assets = total assets of year *t*–1;
Decline_Observations = percentage of observations with a decrease in sales revenue between year *t* and year *t*–1;
Successive_Decrease = 1 when $Sales_{t-2} > Sales_{t-1} > Sales_t$, and 0 otherwise;
Employee_Intensity = ratio of total number of employees to sales revenue;
Asset_Intensity = ratio of total assets to sales revenue;
Economic_Growth = growth in real gross national product (GNP);
Loss_Prior_Year = 1 when the firm reports a loss in the previous year, and 0 otherwise; and
Abnor_Accruals = signed abnormal accruals following Francis and Wang (2008).

increase in labor costs of 0.60 percent, while a decrease in sales revenues of 1 percent results in a decrease in labor costs of 0.34 percent.

With regard to the control variables, we find no significant effect of a successive decrease in sales ($\beta_6 = -0.00$, t = –0.01), asset intensity ($\beta_8 = 0.00$, t = 0.14), economic growth ($\beta_9 = -0.01$, t = –0.28) and loss prior year ($\beta_{10} = 0.05$, t = 1.16). Firms with a higher employee intensity exhibit a lower degree of cost asymmetry ($\beta_7 = 4.27$, t = 4.87). Abnormal accruals have no effect on cost asymmetry ($\beta_4 = 0.02$, t = 0.31; $\beta_{11} = 0.02$, t = 0.18).[9]

Cost Asymmetry and Incentives to Report a Small Profit

Our primary prediction is that firms reporting a small profit exhibit a lower degree of labor cost asymmetry. More specifically, we predict that small-profit observations will exhibit a smaller labor cost increase following an activity increase and a larger labor cost decrease following an activity decrease, both of which induce the overall pattern of labor cost symmetry.

Table 3, Column III, presents findings from the first test of this hypothesis, which is based on estimating Equation (2) including the small-profit indicator variable and associated interaction terms. Consistent with our predictions, we find a significantly negative coefficient on the two-way interaction term ($\beta_3 = -0.26$; t = –2.83) and a significantly positive coefficient on the three-way

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[9] The labor cost elasticities for sales increases and decreases are for a firm with zero values for the control variables, i.e., ignoring the interactive effect of the control variables on sales. For a firm with median values of the control variables, we still find clear evidence of cost asymmetry. In particular, for the median firm, an increase in sales revenues of 1 percent leads to an increase in labor costs of 0.60 percent, while a decrease in sales revenues of 1 percent results in a decrease in labor costs of 0.25 percent.

TABLE 3

Summary Statistics from Regressions with Log($LaborC_{it}/LaborC_{it-1}$) as the Dependent Variable

	Predicted Sign	I Basic Model[a] Coeff. (t-stat)	II Extended Model[b] Coeff. (t-stat)	III Extended Model with *Small_Profit*[c] Coeff. (t-stat)	IV Extended Model with *Small_Profit* for Restricted Sample[d] Coeff. (t-stat)
Variable					
β_0: Constant		0.01	0.01	0.01	0.01
		(2.85)***	(3.18)***	(2.67)***	(1.72)*
β_1: Log($Sales_{it}/Sales_{it-1}$)	+	0.64	0.60	0.63	0.53
		(19.44)***	(15.93)***	(15.54)***	(7.77)***
Two-Way Interaction Terms					
β_2: *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	–	−0.22	−0.26	−0.30	−0.28
		(−4.47)***	(−3.66)***	(−4.12)***	(−2.67)***
β_3: *Small_Profit* * Log($Sales_{it}/Sales_{it-1}$)	–	—	—	−0.26	−0.13
		—	—	(−2.83)***	(−1.87)**
β_4: *Abnor_Accruals* * Log($Sales_{it}/Sales_{it-1}$)	+	—	0.03	0.02	0.04
		—	(0.36)	(0.31)	(0.38)
Three-Way Interaction Terms					
β_5: Small_Profit * Decrease_Dummy	+	—	—	0.34	0.27
* Log($Sales_{it}/Sales_{it-1}$)		—	—	(2.77)***	(1.94)**
β_6: *Successive_Decrease* * *Decrease_Dummy*	+	—	−0.00	−0.01	0.05
* Log($Sales_{it}/Sales_{it-1}$)		—	(−0.01)	(−0.13)	(−0.67)
β_7: *Employee_Intensity* * *Decrease_Dummy*	–	—	4.27	4.02	3.86
* Log($Sales_{it}/Sales_{it-1}$)		—	(4.87)***	(4.68)***	(5.84)***
β_8: *Asset_Intensity* * *Decrease_Dummy*	–	—	0.00	0.00	−0.00
* Log($Sales_{it}/Sales_{it-1}$)		—	(0.14)	(0.34)	(−0.43)
β_9: *Economic_Growth* * *Decrease_Dummy*	–	—	−0.01	−0.01	0.00
* Log($Sales_{it}/Sales_{it-1}$)		—	(−0.28)	(−0.35)	(0.16)

(continued on next page)

TABLE 3 (continued)

	Predicted Sign	I Basic Model[a] Coeff. (t-stat)	II Extended Model[b] Coeff. (t-stat)	III Extended Model with *Small_Profit*[c] Coeff. (t-stat)	IV Extended Model with *Small_Profit* for Restricted Sample[d] Coeff. (t-stat)
β_{10}: *Loss_Prior_Year* ∗ *Decrease_Dummy*	+	—	0.05	0.06	0.03
∗ Log($Sales_{it}/Sales_{it-1}$)		—	(1.16)	(1.36)	(0.64)
β_{11}: *Abnor_Accruals* ∗ *Decrease_Dummy*	–	—	0.02	0.04	0.00
∗ Log($Sales_{it}/Sales_{it-1}$)		—	(0.18)	(0.34)	(0.01)
Main Terms					
β_{12}: *Small_Profit*	?	—	—	0.01	0.01
		—	—	(2.43)**	(4.45)***
β_{13}: *Successive_Decrease*	?	—	–0.00	–0.00	–0.01
		—	(–0.64)	(–0.87)	(–2.52)**
β_{14}: *Employee_Intensity*	?	—	0.31	0.29	0.25
		—	(4.66)***	(4.47)***	(4.74)***
β_{15}: *Asset_Intensity*	?	—	–0.00	0.00	–0.00
		—	(–0.10)	(0.50)	(–0.13)
β_{16}: *Economic_Growth*	?	—	–0.00	–0.00	–0.00
		—	(–0.05)	(–0.31)	(–1.83)*
β_{17}: *Loss_Prior_Year*	?	—	–0.01	–0.01	–0.01
		—	(–9.87)***	(–9.64)***	(–8.36)***
β_{18}: *Abnor_Accruals*	?	—	0.01	0.01	0.01
		—	(2.10)**	(2.23)**	(5.55)**
Number of Observations		37,880	37,880	37,880	20,516
Adjusted R^2		46.86%	42.11%	42.72%	33.46%

(continued on next page)

TABLE 3 (continued)

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a]
$$\text{Log}(\mathit{LaborC}_{it}/\mathit{LaborC}_{it-1}) = \beta_0 + \beta_1 * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2 * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \varepsilon_{it}.$$

[b]
$$\begin{aligned}\text{Log}(\mathit{LaborC}_{it}/\mathit{LaborC}_{it-1}) = {} & \beta_0 + \beta_1 * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2 * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_4 * \mathit{Abnor\_Accruals} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \beta_6 * \mathit{Successive\_Decrease} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_7 * \mathit{Employee\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \beta_8 * \mathit{Asset\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_9 * \mathit{Economic\_Growth} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \beta_{10} * \mathit{Loss\_Prior\_Year} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_{11} * \mathit{Abnor\_Accruals} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \text{Main Terms} + \varepsilon_{it}.\end{aligned}$$

[c]
$$\begin{aligned}\text{Log}(\mathit{LaborC}_{it}/\mathit{LaborC}_{it-1}) = {} & \beta_0 + \beta_1 * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2 * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_3 * \mathit{Small\_Profit} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \beta_4 * \mathit{Abnor\_Accruals} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_5 * \mathit{Small\_Profit} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \beta_6 * \mathit{Successive\_Decrease} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_7 * \mathit{Employee\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \beta_8 * \mathit{Asset\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_9 * \mathit{Economic\_Growth} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \beta_{10} * \mathit{Loss\_Prior\_Year} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_{11} * \mathit{Abnor\_Accruals} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) \\ & + \text{Main Terms} + \varepsilon_{it}.\end{aligned}$$

[d] The model used is similar to the model used in Column III. The restricted sample consists of firm-year observations relating to firms that have reported a small profit at least once during the period 1995–2006.

See Table 2 for variable definitions.

interaction term ($\beta_5 = 0.34$, $t = 2.77$). In comparison to firms that do not report a small profit, the labor cost increase of small-profit firms for a 1 percent increase in sales is 0.26 percent smaller and the labor cost decrease of small-profit firms for a 1 percent decrease in sales is 0.34 percent larger.[10]

Table 4 presents the result for our split sample tests, which are conducted by estimating the basic and extended models separately for the large-profit and small-profit observations. Findings for the large-profit sample are similar to those of the full sample. Specifically, we find strong evidence for asymmetric cost behavior of large-profit firm-year observations for the basic model ($\beta_1 = 0.64$, $t = 14.94$; $\beta_2 = -0.24$, $t = -3.77$) as well as for the extended model ($\beta_1 = 0.62$, $t = 12.18$; $\beta_2 = -0.16$, $t = -5.48$). Consistent with our predictions, small-profit observations exhibit symmetric labor cost behavior for the basic model ($\beta_1 = 0.41$, $t = 4.86$; $\beta_2 = 0.10$, $t = 0.89$) and for the extended model ($\beta_1 = 0.43$, $t = 6.36$; $\beta_2 = -0.12$, $t = -0.90$). Furthermore, the increase in labor costs following an activity increase is significantly smaller for small-profit observations compared to large-profit observations ($F = 350.83$; p-value < 0.01).[11] Taken together, results for the split-sample regressions provide additional support for the hypothesis that firms manage their labor costs to meet or beat the zero earnings benchmark, which results in symmetric labor cost behavior.

Table 3, Column IV, presents findings from the third test of our primary hypothesis in which we estimate Equation (2), limiting our sample to those observations of the firms that have reported a small profit for at least one year during the period 1995–2006. Although this test reduces our sample to 20,516 firm-year observations, we still find support for our hypothesis. In particular, the coefficient on the two-way interaction term is significantly negative ($\beta_3 = -0.13$; $t = -1.87$) and the coefficient on the three-way interaction is significantly positive ($\beta_5 = 0.27$; $t = 1.94$).[12]

Taken together, the findings in Tables 3 and 4 provide consistent support for our primary hypothesis by showing that small-profit observations exhibit symmetric labor cost behavior while large-profit observations exhibit asymmetric labor cost behavior.[13,14]

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[10] Estimating Equation (2) including the small-profit indicator variable and associated interaction terms for a sample of firms that includes only small-profit firms and large-profit firms leads to the same inferences. Specifically, we find a significantly negative coefficient on the two-way interaction ($\beta_3 = -0.25$, $t = -2.52$) and a significantly positive coefficient on the three-way interaction ($\beta_5 = 0.30$, $t = 2.26$). The coefficients relating to large-profit firms' activity increases and decreases, β_1 and β_2, are consistent with such firms exhibiting cost asymmetry ($\beta_1 = 0.62$, $t = 12.04$, and $\beta_2 = -0.22$, $t = -2.36$).

[11] The F-test is based on the combined sample of small-profit and large-profit firm-year observations.

[12] Excluding the firm-years in which a loss is reported leads to the same inferences. Specifically, we find a significantly negative coefficient on the two-way interaction term ($\beta_3 = -0.13$; $t = -1.92$) and a significantly positive coefficient on the three-way interaction term ($\beta_5 = 0.26$; $t = 1.97$).

[13] Untabulated findings from estimations of the different models that include a control for firm size (i.e., the natural logarithm of the beginning-of-year total assets) indicate that inferences are unchanged. To facilitate comparisons with prior literature, we tabulate the results from estimations of the regression models that are used in prior literature.

[14] To mitigate the effect of other differences between small-profit observations and large-profit observations on labor cost behavior, we also conducted all of our analyses using a matched-sample design comprising a set of small-profit firms (i.e., treatment firms) and large-profit firms (i.e., matched firms). Specifically, we matched small-profit and large-profit firm-year observations using propensity scores that are estimated separately for each sample year. The first stage of the propensity score matching analysis is a logistical regression that includes variables to predict whether a firm reports a small profit, specifically whether a firm experiences a successive yearly income decrease, beginning-of-year asset intensity, beginning-of-year employee intensity, loss prior year, auditor type as Big N-auditor or not, industry, the natural logarithm of the beginning-of-year total assets, and abnormal accruals. We then assign a matched firm to each treatment firm based on predicted propensity scores that insignificantly differ according to paired t-tests. Untabulated findings from estimations of the different regression models on a sample that includes the treatment and matched firm-years result in no change in inferences relative to tabulated findings.

TABLE 4

Summary Statistics from Split Sample Regressions with Log($LaborC_{it}/LaborC_{it-1}$) as the Dependent Variable

	Large-Profit Sample[a]		**Small-Profit Sample**[b]	
	Basic Model[c]	**Extended Model**[d]	**Basic Model**[c]	**Extended Model**[d]
Variable				
β_0: Constant	0.00	0.01	0.01	0.01
	(1.48)	(1.96)	(3.78)***	(2.95)***
β_1: Log($Sales_{it}/Sales_{it-1}$)	0.64	0.62	0.41	0.43
	(14.94)***	(12.18)***	(4.86)***	(6.36)***
Two-Way Interaction Terms				
β_2: *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	−0.24	−0.16	0.10	−0.12
	(−3.77)***	(−5.48)***	(0.89)	(−0.90)
β_3: *Abnor_Accruals* * Log($Sales_{it}/Sales_{it-1}$)	—	−0.03	—	0.28
	—	(−0.38)	—	(2.36)**
Three-Way Interaction Terms				
β_4: *Successive_Decrease* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	—	−0.08	—	0.13
	—	(−1.15)	—	(1.00)
β_5: *Employee_Intensity* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	—	3.74	—	2.90
	—	(2.36)**	—	(2.89)***
β_6: *Asset_Intensity* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	—	0.00	—	0.00
	—	(0.65)	—	(0.62)
β_7: *Economic_Growth* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	—	−0.04	—	0.03
	—	(−1.35)	—	(0.60)
β_8: *Loss_Prior_Year* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	—	0.08	—	−0.19
	—	(1.20)	—	(−1.39)
β_9: *Abnor_Accruals* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	—	0.02	—	−0.23
	—	(0.16)	—	(−0.95)
Main Terms				
β_{10}: *Successive_Decrease*	—	−0.00	—	0.00
	—	(−1.14)	—	(0.18)
β_{11}: *Employee_Intensity*	—	0.29	—	−0.01
	—	(2.69)***	—	(−0.05)
β_{12}: *Asset_Intensity*	—	0.00	—	−0.00
	—	(1.62)	—	(−0.23)
β_{13}: *Economic_Growth*	—	−0.00	—	0.00
	—	(−0.58)	—	(1.21)
β_{14}: *Loss_Prior_Year*	—	−0.02	—	−0.02
	—	(−9.21)***	—	(−4.15)***
β_{15}: *Abnor_Accruals*	—	0.02	—	0.00
	—	(2.47)**	—	(0.17)
Number of Observations	24,651	24,651	5.067	5.067
Adjusted R^2	46.36%	43.28%	34.64%	32.64%

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a] The small-profit sample consists of firm-years for which net income scaled by total assets is greater than or equal to 0 but less than 1 percent (Roychowdhury 2006).

[b] The large-profit sample consists of profitable firm-years that are not "small-profit."

(continued on next page)

TABLE 4 (continued)

c

$$\text{Log}(LaborC_{it}/LaborC_{it-1}) = \beta_0 + \beta_1 {*} \text{Log}(Sales_{it}/Sales_{it-1}) + \beta_2 {*} Decrease\_Dummy {*} \text{Log}(Sales_{it}/Sales_{it-1}) + \varepsilon_{it}.$$

d

$$\begin{aligned} \text{Log}(LaborC_{it}/LaborC_{it-1}) = {} & \beta_0 + \beta_1 {*} \text{Log}(Sales_{it}/Sales_{it-1}) + \beta_2 {*} Decrease\_Dummy {*} \text{Log}(Sales_{it}/Sales_{it-1}) \\ & + \beta_4 {*} Abnor\_Accruals {*} \text{Log}(Sales_{it}/Sales_{it-1}) \\ & + \beta_6 {*} Successive\_Decrease {*} Decrease\_Dummy {*} \text{Log}(Sales_{it}/Sales_{it-1}) \\ & + \beta_7 {*} Employee\_Intensity {*} Decrease\_Dummy {*} \text{Log}(Sales_{it}/Sales_{it-1}) \\ & + \beta_8 {*} Asset\_Intensity {*} Decrease\_Dummy {*} \text{Log}(Sales_{it}/Sales_{it-1}) \\ & + \beta_9 {*} Economic\_Growth {*} Decrease\_Dummy {*} \text{Log}(Sales_{it}/Sales_{it-1}) \\ & + \beta_{10} {*} Loss\_Prior\_Year {*} Decrease\_Dummy {*} \text{Log}(Sales_{it}/Sales_{it-1}) \\ & + \beta_{11} {*} Abnor\_Accruals {*} Decrease\_Dummy {*} \text{Log}(Sales_{it}/Sales_{it-1}) + \text{Main Terms} \\ & + \varepsilon_{it}. \end{aligned}$$

See Table 2 for variable definitions.

Mechanisms of Adjusting Labor Costs

In this section, we present results from tests that use detailed employee data to examine the mechanisms that managers of private Belgian firms use to manage their labor costs to meet or beat the zero earnings benchmark. Because Belgian firms have been required to disclose detailed employee data only since 1997, the sample of 21,552 firm-year observations, which we refer to as the "Employee-Detail" sample, is approximately one-third smaller than the sample available for prior tests. We also eliminate observations that have missing values for at least one of the variables required to conduct the tests presented in this section. Untabulated descriptive statistics for the Employee-Detail sample are comparable to those of the full sample. Inferences from the previous section do not change when we estimate the basic and extended model on the Employee-Detail sample.

Table 5 presents findings from estimations of the extended model with number of hours per employee and number of employees as the dependent variable. The models analyze how managers adjust the number of employees by firing and hiring employees, and/or adjust the number of hours per employee in response to activity changes. The results generally suggest that managers of small-profit firms prefer the former strategy of adjusting the number of employees, and managers of large-profit firms prefer the latter strategy of adjusting the number of hours worked by each employee. In particular, small-profit firm-years exhibit a symmetric pattern for the number of employees ($\beta_1 = 0.25$, t = 4.26; $\beta_2 = 0.06$, $t = 0.33$) and for the number of hours per employee ($\beta_1 = 0.09$, t = 2.53; $\beta_2 = 0.04$, t = 0.43). In contrast, large-profit firm-years exhibit asymmetry with respect to the number of employees ($\beta_1 = 0.41$, t = 11.34; $\beta_2 = -0.28$, t = −3.72) and with respect to number of hours per employee ($\beta_1 = 0.19$, t = 5.42; $\beta_2 = 0.09$, t = 1.92), although the positive β_2 implies that the number of hours per employee *decreases* more with an activity decrease than it increases with an activity increase. In addition, the number of hours per employee is more sensitive to activity changes for large-profit firm-years, i.e., an increase (decrease) of 0.19 percent (0.28 percent) following a 1 percent increase (decrease) in activity, than for small-profit observations, i.e., a change of 0.09 percent following a 1 percent change in activity.

Taken together, the findings in Table 5 provide support for our prediction that the asymmetric cost pattern of large-profit observations is driven by managerial decisions to avoid employee dismissals when activity decreases and to react to activity decreases by changing the number of

TABLE 5

Summary Statistics from Split Sample Regressions with Number of Hours per Employee and Number of Employees as the Dependent Variables

	Small-Profit[a]		Large-Profit[b]	
	Number of Employees[c]	Number of Hours/ Employee[d]	Number of Employees[c]	Number of Hours/ Employee[d]
Variable				
β_0: Constant	0.01	0.00	−0.00	−0.00
	(1.46)	(1.03)	(−1.66)*	(−0.98)
β_1: Log($Sales_{it}/Sales_{it-1}$)	0.25	0.09	0.41	0.19
	(4.26)***	(2.53)**	(11.34)***	(5.42)***
Two-Way Interaction Terms				
β_2: *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	0.06	0.04	−0.28	0.09
	(0.33)	(0.43)	(−3.72)***	(1.92)*
β_3: *Abnor_Accruals* * Log($Sales_{it}/Sales_{it-1}$)	0.24	−0.51	0.08	−0.02
	(2.49)**	(−2.42)**	(1.08)	(−0.31)
Three-Way Interaction Terms				
β_4: *Successive_Decrease* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	0.27	0.03	0.16	−0.28
	(1.91)**	(0.28)	(2.80)***	(−5.20)***
β_5: *Employee_Intensity* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	−3.19	5.72	5.27	2.85
	(−2.63)***	(8.08)***	(5.91)***	(−3.11)***
β_6: *Asset_Intensity* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	0.00	−0.01	−0.00	−0.00
	(0.41)	(−1.53)	(−1.69)*	(−0.53)
β_7: *Economic_Growth* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	−0.06	−0.00	−0.06	0.05
	(−1.23)	(−0.05)	(−3.19)***	(1.32)
β_8: *Loss_Prior_Year* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	0.01	−0.03	0.13	−0.11
	(0.07)	(−0.27)	(2.21)**	(−1.80)*
β_9: *Abnor_Accruals* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	−0.22	0.81	−0.07	−0.11
	(−0.90)	(2.65)***	(−0.70)	(−0.98)
Main Terms included in model but coefficients not reported				
Number of Observations	2,644	2,644	14,232	14,232
Adjusted R^2	16.04%	8.51%	16.97%	6.29%

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a] The small-profit sample consists of firm-years for which net income scaled by total assets is greater than or equal to 0 but less than 1 percent (Roychowdhury 2006).

[b] The large-profit sample consists of profitable firm-years that are not "small-profit."

[c]

$$\begin{aligned}
&\text{Log}(\textit{Number of Employees}_{it}/\textit{Number of Employees}_{it-1})\\
&\quad = \beta_0 + \beta_1 {*}\text{Log}(Sales_{it}/Sales_{it-1}) + \beta_2 {*} Decrease\_Dummy {*}\text{Log}(Sales_{it}/Sales_{it-1})\\
&\qquad + \beta_4 {*} Abnor\_Accruals {*}\text{Log}(Sales_{it}/Sales_{it-1})\\
&\qquad + \beta_6 {*} Successive\_Decrease {*} Decrease\_Dummy {*}\text{Log}(Sales_{it}/Sales_{it-1})\\
&\qquad + \beta_7 {*} Employee\_Intensity {*} Decrease\_Dummy {*}\text{Log}(Sales_{it}/Sales_{it-1})\\
&\qquad + \beta_8 {*} Asset\_Intensity {*} Decrease\_Dummy {*}\text{Log}(Sales_{it}/Sales_{it-1})\\
&\qquad + \beta_9 {*} Economic\_Growth {*} Decrease\_Dummy {*}\text{Log}(Sales_{it}/Sales_{it-1})\\
&\qquad + \beta_{10} {*} Loss\_Prior\_Year {*} Decrease\_Dummy {*}\text{Log}(Sales_{it}/Sales_{it-1})\\
&\qquad + \beta_{11} {*} Abnor\_Accruals {*} Decrease\_Dummy {*}\text{Log}(Sales_{it}/Sales_{it-1}) + \text{Main Terms} + \varepsilon_{it}.
\end{aligned}$$

(continued on next page)

TABLE 5 (continued)

[d]

$$
\begin{aligned}
&\mathrm{Log}(\textit{Number of Hours per Employee}_{it}/\textit{Number of Hours per Employee}_{it-1})\\
&\quad = \beta_0 + \beta_1{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) + \beta_2{*}\textit{Decrease\_Dummy}{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1})\\
&\qquad + \beta_4{*}\textit{Abnor\_Accruals}{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1})\\
&\qquad + \beta_6{*}\textit{Successive\_Decrease}{*}\textit{Decrease\_Dummy}{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1})\\
&\qquad + \beta_7{*}\textit{Employee\_Intensity}{*}\textit{Decrease\_Dummy}{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1})\\
&\qquad + \beta_8{*}\textit{Asset\_Intensity}{*}\textit{Decrease\_Dummy}{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1})\\
&\qquad + \beta_9{*}\textit{Economic\_Growth}{*}\textit{Decrease\_Dummy}{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1})\\
&\qquad + \beta_{10}{*}\textit{Loss\_Prior\_Year}{*}\textit{Decrease\_Dummy}{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1})\\
&\qquad + \beta_{11}{*}\textit{Abnor\_Accruals}{*}\textit{Decrease\_Dummy}{*}\mathrm{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) + \text{Main Terms} + \varepsilon_{it}.
\end{aligned}
$$

See Table 2 for variable definitions.

hours per employee. In contrast, managers of small-profit firms do so by making decisions that result in immediate cost reductions, i.e., by firing employees.

Table 6 reports findings from estimations of the extended model using the number of white-collar and blue-collar workers as the dependent variables. These estimations compare the decisions regarding white-collar and blue-collar workers for large-profit and small-profit firm-years. Consistent with our predictions and consistent with the findings for the number of employees reported in Table 5, the findings in Table 6 indicate asymmetric behavior for both number of white-collar workers ($\beta_1 = 0.39$, $t = 8.47$; $\beta_2 = -0.26$, $t = -2.73$) and for the number of blue-collar workers ($\beta_1 = 0.39$, $t = 7.59$; $\beta_2 = -0.20$, $t = -2.42$) for large-profit observations. In contrast, small-profit observations exhibit symmetric behavior for white-collar workers ($\beta_1 = 0.17$, $t = 2.65$; $\beta_2 = 0.02$, $t = 0.15$). For blue-collar workers, the number increases 0.21 percent following a 1 percent increase in activity, but decreases 0.68 percent following a 1 percent decrease in activity ($\beta_1 = 0.21$, $t = 2.83$; $\beta_2 = 0.47$, $t = 3.89$). These results indicate that whereas differences in severance costs do not play a role for large-profit firms, managers of small-profit firms focus on the type of employees that can be fired in the cheapest way, i.e., blue-collar workers.

Small-Loss Firms

As noted above, small-loss firms could potentially have faced similar incentives as small-profit firms, but were unable to meet the zero earnings benchmark. If this is the case, then we expect small-loss observations to exhibit similar cost behavior as small-profit firms. Table 7 presents findings for small-loss firms analogous to those presented for large-profit and small-profit firms in Tables 4 to 6. Findings for estimations based on total labor costs appear to contradict the expectation that small-loss observations face similar incentives as small-profit observations, as small-loss observations exhibit cost asymmetry for the basic model ($\beta_1 = 0.68$, $t = 8.04$; $\beta_2 = -0.30$, $t = -2.52$) and for the extended model ($\beta_1 = 0.72$, $t = 8.94$; $\beta_2 = -0.55$, $t = -3.51$). However, findings from estimations based on the number of employees, the number of hours per employee, and the number of blue-collar workers indicate that they respond in a symmetric way to activity changes ($\beta_1 = 0.38$, $t = 4.47$; $\beta_2 = -0.17$, $t = -1.11$ for the number of employees; $\beta_1 = 0.25$, $t = 3.14$; $\beta_2 = -0.41$, $t = -1.62$ for the number of hours per employee; $\beta_1 = 0.38$, $t = 3.79$; $\beta_2 = -0.16$, $t = -0.45$ for the number of blue-collar workers). Surprisingly, the number of white-collar workers increases following an activity decrease ($\beta_1 = 0.27$, $t = 2.11$; $\beta_2 = -0.46$, $t = -2.13$), and likely accounts for the difference in total labor cost behavior between small-loss observations and small-profit observations.

TABLE 6

Summary Statistics from Split Sample Regressions with Number of Blue-Collar Workers and Number of White-Collar Workers as the Dependent Variables

	Large-Profit[a]		Small-Profit[b]	
	Number of White-Collar Workers[c]	Number of Blue-Collar Workers[d]	Number of White-Collar Workers[c]	Number of Blue-Collar Workers[d]
Variable				
β_0: Constant	−0.00	−0.01	0.01	−0.00
	(−0.14)	(−2.12)**	(1.20)	(−0.31)
β_1: Log($Sales_{it}/Sales_{it-1}$)	0.39	0.39	0.17	0.21
	(8.47)***	(7.59)***	(2.65)***	(2.83)***
Two-Way Interaction Terms				
β_2: *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	−0.26	−0.20	0.02	0.47
	(−2.73)***	(−2.42)**	(0.15)	(3.89)***
β_3: *Abnor_Accruals* * Log($Sales_{it}/Sales_{it-1}$)	0.11	0.15	0.18	0.13
	(1.06)	(1.49)	(1.75)*	(1.00)
Three-Way Interaction Terms				
β_4: *Successive_Decrease* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	0.08	0.28	−0.18	−0.30
	(0.95)	(1.84)*	(−0.98)	(−1.40)
β_5: *Employee_Intensity* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	4.48	2.18	−0.14	−1.14
	(2.55)**	(1.02)	(−0.12)	(−0.56)
β_6: *Asset_Intensity* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	−0.00	−0.00	−0.00	0.00
	(−0.98)	(−0.54)	(−0.31)	(0.14)
β_7: *Economic_Growth* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	−0.06	−0.06	−0.06	−0.21
	(−1.96)**	(−1.50)	(−1.08)	(−1.48)
β_8: *Loss_Prior_Year* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	0.10	−0.06	0.34	0.13
	(1.17)	(−0.58)	(1.42)	(0.73)
β_9: *Abnor_Accruals* * *Decrease_Dummy* * Log($Sales_{it}/Sales_{it-1}$)	−0.09	−0.19	−0.15	0.48
	(−0.61)	(−1.27)	(−0.95)	(1.12)
Main Terms included in model but coefficients not reported				
Number of Observations	14,232	14,232	2,644	2,644
Adjusted R^2	6.15%	4.48%	2.58%	5.57%

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a] The large-profit sample consists of profitable firm-years that are not "small-profit."

[b] The small-profit sample consists of firm-years for which net income scaled by total assets is greater than or equal to 0 but less than 1 percent (Roychowdhury 2006).

[c]

$$\begin{aligned}
&\text{Log}(\mathit{Number\ of\ White\text{-}Collar\ Workers}_{it}/\mathit{Number\ of\ White\text{-}Collar\ Workers}_{it-1})\\
&\quad= \beta_0 + \beta_1{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2{*}\mathit{Decrease\_Dummy}{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_4{*}\mathit{Abnor\_Accruals}{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_6{*}\mathit{Successive\_Decrease}{*}\mathit{Decrease\_Dummy}{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_7{*}\mathit{Employee\_Intensity}{*}\mathit{Decrease\_Dummy}{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_8{*}\mathit{Asset\_Intensity}{*}\mathit{Decrease\_Dummy}{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_9{*}\mathit{Economic\_Growth}{*}\mathit{Decrease\_Dummy}{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_{10}{*}\mathit{Loss\_Prior\_Year}{*}\mathit{Decrease\_Dummy}{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_{11}{*}\mathit{Abnor\_Accruals}{*}\mathit{Decrease\_Dummy}{*}\text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \text{Main Terms} + \varepsilon_{it}.
\end{aligned}$$

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TABLE 6 (continued)

[d]

$$\begin{aligned}
&\text{Log}(\mathit{Number\ of\ Blue\text{-}Collar\ Workers}_{it}/\mathit{Number\ of\ Blue\text{-}Collar\ Workers}_{it-1})\\
&\quad = \beta_0 + \beta_1 * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2 * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_4 * \mathit{Abnor\_Accruals} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_6 * \mathit{Successive\_Decrease} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_7 * \mathit{Employee\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_8 * \mathit{Asset\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_9 * \mathit{Economic\_Growth} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_{10} * \mathit{Loss\_Prior\_Year} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\
&\qquad + \beta_{11} * \mathit{Abnor\_Accruals} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \text{Main Terms} + \varepsilon_{it}.
\end{aligned}$$

See Table 2 for variable definitions.

Additional Analyses and Sensitivity Testing

In this section we consider several additional tests to determine the sensitivity of our findings regarding symmetry of labor costs for small-profit firms to an alternative measure of accruals-based earnings management, the time difference between real and accrual-based earnings management, the flexibility to manage labor costs, operational difficulties, and future growth prospects.

First, we repeat our analyses measuring the amount of accrual-based earnings management using the performance-adjusted Modified Jones (1991) model. Although doing so reduces the sample to 16,122 firm-year observations, untabulated findings from estimations of the extended model and split sample regressions indicate that inferences are unchanged because when we use this alternative measure of abnormal accruals, small-profit (large-profit) firms exhibit cost symmetry (asymmetry). Next, to mitigate the potential effects arising from the time difference between real and accrual-based earnings management, we replace our *ex post* measurement of the amount of accrual-based earnings management with two *ex ante* measures. The first is the presence of a Big N auditor, measured by an indicator variable that equals 1 if the firm is audited by a Big N auditor, and 0 otherwise. The second is the extent of accounting flexibility (Zang 2012; Cohen and Zarowin 2010; Gunny 2010), which we measure as net operating assets as a fraction of total assets (Barton and Simko 2002). We expect the presence of a Big N auditor to increase the cost of using accrual-based earnings management. Accounting flexibility to manipulate accruals is constrained by the extent to which a firm has used accrual-based earnings management in the past because most accruals reverse in the current period (Barton and Simko 2002; Hirshleifer et al. 2004). Untabulated findings from estimations of the extended model and split sample regressions that include each of these *ex ante* variables indicate that inferences are unchanged.

Our findings can also be influenced by a firm's flexibility to manage labor costs. Because blue-collar workers can be more easily fired in Belgium, firms with a greater proportion of blue-collar workers can more easily manage their labor costs. Consequently, we expect labor costs to behave more symmetrically for firms with a greater proportion of blue-collar workers. Untabulated findings of the extended model and split sample regressions reveal that small-profit firms with more flexibility to manage labor costs indeed have more symmetric cost behavior, but the proportion of blue-collar workers has no effect on the cost behavior of large-profit firms.

Operational difficulties could also contribute to the difference in labor cost behavior between small-profit and large-profit firms. We expect firms with operational difficulties to increase labor costs less in response to an activity increase and decrease labor costs more in response to an activity decrease. We measure operational difficulties using two proxy variables, the ratio of cash and cash flow to total assets, and by including the quick ratio, the change in quick ratio, and a measure of the probability of bankruptcy based on Altman (1968). Untabulated findings relating to the estimation of the extended model and split sample regressions with these controls for operational difficulties do not change our inferences.

Finally, cost asymmetry can arise because managers react to private information regarding the extent to which an activity change is temporary or permanent. Prior studies typically use economic growth and successive activity decreases to capture the manager's estimation of persistence of the activity change (Anderson et al. 2003). However, because these variables may not sufficiently capture firm-level future prospects, the differences in cost asymmetry we observe between small-profit and large-profit observations can be driven by insufficient controls for future prospects. Specifically, weak future prospects can lead managers of small-profit firms to spend less than proportionally in the current period when activities are increasing, and to cut down costs more than proportionally in the current period, when activities are decreasing. This alternative explanation would also yield symmetric behavior for small-profit firms for reasons unrelated to incentives to beat the zero earnings benchmark in the current year. To address this concern, we estimate the extended model and split sample regressions including the average sales growth of the three subsequent years. Untabulated findings indicate that inferences are largely unchanged for both small-profit and large-profit observations. In particular, large-profit observations exhibit cost asymmetry for labor costs ($\beta_1 = 0.60$, $t = 9.76$; $\beta_2 = -0.20$, $t = -4.93$), number of employees ($\beta_1 = 0.42$, $t = 10.54$; $\beta_2 = -0.32$, $t = -4.52$), blue- and white-collar workers ($\beta_1 = 0.45$, $t = 6.91$; $\beta_2 = -0.28$, $t = -2.26$ for blue-collar workers; $\beta_1 = 0.40$, $t = 9.06$; $\beta_2 = -0.34$, $t = -4.13$ for white-collar workers), and symmetric behavior for the number of hours per employee ($\beta_1 = 0.21$, $t = 5.42$; $\beta_2 = 0.02$, $t = 0.24$). Small-profit observations exhibit asymmetric behavior in the opposite direction for labor costs ($\beta_1 = 0.26$, $t = 4.53$; $\beta_2 = 0.33$, $t = 2.18$), symmetric behavior for the number of employees ($\beta_1 = 0.21$, $t = 3.78$; $\beta_2 = 0.28$, $t = 1.50$), no reaction to activity changes for the number of hours per employee ($\beta_1 = 0.04$, $t = 1.42$; $\beta_2 = 0.16$, $t = 1.45$) and white-collar workers ($\beta_1 = 0.10$, $t = 1.22$; $\beta_2 = 0.20$, $t = 1.10$), and asymmetric behavior in the opposite direction for the blue-collar workers ($\beta_1 = 0.16$, $t = 1.50$; $\beta_2 = 1.02$, $t = 1.71$).

In contrast, the findings for small-loss firms differ markedly from those reported in Table 7. In particular, small-loss observations exhibit symmetric labor costs once future sales growth is included as a control ($\beta_1 = 0.45$, $t = 3.20$; $\beta_2 = -0.16$, $t = -0.65$). Moreover, the number of white-collar workers is no longer increasing following an activity decrease ($\beta_1 = 0.29$, $t = 2.31$; $\beta_2 = -0.31$, $t = -1.14$). Thus, it appears that failure to control for future growth is responsible for the mixed result for small-loss firms in Table 7.[15]

V. CONCLUSION

This study investigates whether and how incentives to meet or beat the zero earnings benchmark influences labor cost behavior in a sample of private Belgian firms. Our findings show that firms with incentives to meet or beat the zero earnings benchmark limit the increase in labor costs following an activity increase and are more willing to cut labor costs following an activity decrease, which results in symmetric labor cost behavior. Firms that do not face significant pressure from the earnings benchmark exhibit asymmetric labor cost behavior, which is consistent with prior research on SG&A expenditures of publicly traded U.S. firms.

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[15] Similar to small-profit observations, after controlling for future sales growth, small-loss observations exhibit symmetric behavior for the number of employees ($\beta_1 = 0.24$, $t = 2.59$; $\beta_2 = -0.03$, $t = -0.18$), the number of hours per employee ($\beta_1 = 0.23$, $t = 2.86$; $\beta_2 = -0.31$, $t = -0.89$), and the number of blue-collar workers ($\beta_1 = 0.43$, $t = 2.49$; $\beta_2 = -0.19$, $t = -0.60$).

TABLE 7

Summary Statistics from Regressions for Small-Loss Observations with Labor Costs, Number of Hours per Employees, Number of Employees, Number of Blue-Collar Workers, and Number of White-Collar Workers as the Dependent Variables

	Full Sample		Employee-Detail Sample[a]			
	Labor Costs[b]	Labor Costs[c]	Number of Employees[d]	Number of Hours per Employee[e]	Number of Blue-Collar Workers[f]	Number of White-Collar Workers[g]
Variable						
β_0: Constant	0.01	0.01	−0.00	0.01	−0.02	−0.02
	(2.90)***	(1.12)	(−0.46)	(0.66)	(−1.29)	(−1.23)
β_1: Log($Sales_{it}/Sales_{it-1}$)	0.68	0.72	0.38	0.25	0.38	0.27
	(8.04)***	(8.94)***	(4.47)***	(3.14)***	(3.79)***	(2.11)**
Two-Way Interaction Terms						
β_2: $Decrease\_Dummy$ * Log($Sales_{it}/Sales_{it-1}$)	−0.30	−0.55	−0.17	−0.41	−0.16	−0.46
	(−2.52)**	(−3.51)***	(−1.11)	(−1.62)	(−0.45)	(−2.13)**
β_3: $Abnor\_Accruals$ * Log($Sales_{it}/Sales_{it-1}$)		0.39	−0.11	0.57	0.08	−0.10
		(2.64)***	(−0.55)	(1.81)*	(0.41)	(−0.27)
Three-Way Interaction Terms						
β_4: $Successive\_Decrease$ * $Decrease\_Dummy$ * Log($Sales_{it}/Sales_{it-1}$)		−0.10	0.09	−0.28	−0.04	0.07
		(−0.59)	(0.86)	(−1.38)	(−0.13)	(0.47)
β_5: $Employee\_Intensity$ * $Decrease\_Dummy$ * Log($Sales_{it}/Sales_{it-1}$)		4.59	−12.65	16.95	44.77	5.46
		(1.42)	(−1.76)*	(1.75)	(1.26)	(0.65)
β_6: $Asset\_Intensity$ * $Decrease\_Dummy$ * Log($Sales_{it}/Sales_{it-1}$)		0.00	0.01	−0.01	−0.01	0.01
		(0.92)	(0.63)	(−0.35)	(−0.26)	(0.23)
β_7: $Economic\_Growth$ * $Decrease\_Dummy$ * Log($Sales_{it}/Sales_{it-1}$)		0.05	0.00	0.14	−0.04	0.05
		(0.96)	(0.10)	(1.73)*	(−0.40)	(0.74)
β_8: $Loss\_Prior\_Year$ * $Decrease\_Dummy$ * Log($Sales_{it}/Sales_{it-1}$)		0.17	−0.03	−0.02	−0.29	0.09
		(1.38)	(−0.26)	(−0.09)	(−1.03)	(0.56)
β_9: $Abnor\_Accruals$ * $Decrease\_Dummy$ * Log($Sales_{it}/Sales_{it-1}$)		−0.33	−0.61	−0.65	0.56	0.64
		(−1.34)	(2.30)**	(−1.94)**	(0.98)	(1.33)
Main Terms included in model but coefficients not reported						
Number of Observations	1,784	1,784	1,013	1,013	1,013	1,013
Adjusted R^2	32.86%	38.37%	43.71%	28.51%	12.84%	3.62%

(continued on next page)

TABLE 7 (continued)

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a] The employee-detail sample consists of all firm-year observations from 1997–2006 because Belgian firms have been required to disclose detailed employee data only since 1997.

[b]

$$\text{Log}(\mathit{LaborC}_{it}/\mathit{LaborC}_{it-1}) = \beta_0 + \beta_1 * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2 * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \varepsilon_{it}.$$

[c]

$$\begin{aligned}\text{Log}(\mathit{LaborC}_{it}/\mathit{LaborC}_{it-1}) = {} & \beta_0 + \beta_1 * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2 * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_4 * \mathit{Abnor\_Accruals} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_6 * \mathit{Successive\_Decrease} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_7 * \mathit{Employee\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_8 * \mathit{Asset\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_9 * \mathit{Economic\_Growth} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_{10} * \mathit{Loss\_Prior\_Year} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_{11} * \mathit{Abnor\_Accruals} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \text{Main Terms} + \varepsilon_{it}.\end{aligned}$$

[d]

$$\begin{aligned}\text{Log}(\mathit{Number\_of\_Employees}_{it}/\mathit{Number\_of\_Employees}_{it-1}) = {} & \beta_0 + \beta_1 * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2 * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_4 * \mathit{Abnor\_Accruals} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_6 * \mathit{Successive\_Decrease} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_7 * \mathit{Employee\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_8 * \mathit{Asset\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_9 * \mathit{Economic\_Growth} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_{10} * \mathit{Loss\_Prior\_Year} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_{11} * \mathit{Abnor\_Accruals} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \text{Main Terms} + \varepsilon_{it}.\end{aligned}$$

[e]

$$\begin{aligned}\text{Log}(\mathit{Number\ of\ Hours\ Per\ Employee}_{it}/\mathit{Number\ of\ Hours\ Per\ Employee}_{it-1}) = {} & \beta_0 + \beta_1 * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \beta_2 * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_4 * \mathit{Abnor\_Accruals} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_6 * \mathit{Successive\_Decrease} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_7 * \mathit{Employee\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_8 * \mathit{Asset\_Intensity} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_9 * \mathit{Economic\_Growth} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_{10} * \mathit{Loss\_Prior\_Year} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1})\\ & + \beta_{11} * \mathit{Abnor\_Accruals} * \mathit{Decrease\_Dummy} * \text{Log}(\mathit{Sales}_{it}/\mathit{Sales}_{it-1}) + \text{Main Terms} + \varepsilon_{it}.\end{aligned}$$

(continued on next page)

TABLE 7 (continued)

[f]

$$\begin{aligned}\text{Log}(\textit{Number of Blue-Collar Workers}_{it}/\textit{Number of Blue-Collar Workers}_{it-1}) = {} & \beta_0 + \beta_1 * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) + \beta_2 * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_4 * \textit{Abnor\_Accruals} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_6 * \textit{Successive\_Decrease} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_7 * \textit{Employee\_Intensity} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_8 * \textit{Asset\_Intensity} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_9 * \textit{Economic\_Growth} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_{10} * \textit{Loss\_Prior\_Year} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_{11} * \textit{Abnor\_Accruals} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) + \text{Main Terms} + \varepsilon_{it}.\end{aligned}$$

[g]

$$\begin{aligned}\text{Log}(\textit{Number of White-Collar Workers}_{it}/\textit{Number of White-Collar Workers}_{it-1}) = {} & \beta_0 + \beta_1 * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) + \beta_2 * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_4 * \textit{Abnor\_Accruals} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_6 * \textit{Successive\_Decrease} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_7 * \textit{Employee\_Intensity} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_8 * \textit{Asset\_Intensity} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_9 * \textit{Economic\_Growth} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_{10} * \textit{Loss\_Prior\_Year} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) \\ & + \beta_{11} * \textit{Abnor\_Accruals} * \textit{Decrease\_Dummy} * \text{Log}(\textit{Sales}_{it}/\textit{Sales}_{it-1}) + \text{Main Terms} + \varepsilon_{it}.\end{aligned}$$

See Table 2 for variable definitions.

Examination of the underlying managerial decisions shows that managers of firms that report a healthy profit react to activity changes by changing the number of hours that employees work because such an approach limits the damage to their reputation within the firm and in the labor market. Managers of firms that meet or beat the zero earnings benchmark, on the other hand, adjust to activity changes by firing employees, which results in immediate cost reductions. Finally, managers of small-profit firms are more likely to terminate employees who are least costly to fire. Whether managing labor costs in order to meet or beat the zero earnings benchmark harms future firm performance is beyond the scope of this paper, but this question can serve as an interesting topic for follow-up work.

This study extends contemporaneous accounting literature in two ways. First, we present evidence that variations in cost behavior are driven by managerial incentives. Examining the influence of managerial incentives on cost behavior in private firms permits us to focus on managerial incentives in a setting in which agency costs are likely to play a minimal role. Second, we extend recent work in accounting by documenting real managerial decisions with respect to employees. Our findings show that accounting benchmarks influence managerial decision-making with respect to employees and provide potential insights to labor market policy makers.

By design, our focus on labor cost behavior in private Belgian firms limits the generalizability of our results to other types of costs and other institutional settings. However, the primary aim of this paper was to test whether managerial incentives influence cost behavior and this requires a specific institutional setting. Future research may test whether our findings replicate in other institutional settings with other types of incentives and other costs. Furthermore, employee decisions that managers can make to influence their cost level are not limited to hiring and firing decisions and increasing or decreasing the number of hours per employee. We leave to future research the way in which managers use other tools, such as bonus determination and promotion decisions, to influence labor costs.

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Rules-Based Accounting Standards and Litigation

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ABSTRACT: Some claim that rules-based accounting standards shield firms from litigation, while others argue that violations of detailed rules give plaintiffs a "roadmap" to successful litigation. We inform this debate by investigating whether rules-based standards are associated with the incidence and outcome of securities class action litigation. Overall, our results suggest that rules-based standards are associated with a lower incidence of litigation but are not associated with litigation outcomes. These results are of interest in the debate regarding the switch from a more rules-based U.S. GAAP to a more principles-based IFRS.

Keywords: *securities litigation; safe harbor; rules-based standards; principles-based standards.*

JEL Classifications: *K22, K41, M41.*

I. INTRODUCTION

Many practitioners and members of the popular press decry the rules-based nature of U.S. GAAP and propose the adoption of standards that are more principles-based (see, e.g., DiPiazza et al. 2006). The Securities and Exchange Commission (SEC 2010) is considering just such a shift to the more principles-based International Financial Reporting Standards (IFRS). One concern with such proposals is that a move to principles-based standards

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will increase firms' exposure to litigation (Schipper 2003). The SEC (2003), however, predicts a decline in litigation if principles-based standards are implemented. Despite the important policy implications, current arguments regarding rules-based standards and litigation rest on rhetoric and there is no clear consensus on their relation to litigation. Schipper (2003, 68) summarizes the need for empirical evidence on this issue, stating that "there is no systematic evidence on the question of whether detailed accounting guidance has any effect on either the incidence or cost of litigation." This study provides such evidence.

In order to understand the role of rules-based standards in accounting-related litigation, it is critical to understand certain aspects of the U.S. securities litigation system. First, securities class actions almost never proceed to trial. As a result, cases that survive the defendant firm's motion to dismiss are almost always settled out of court. This makes the motion to dismiss the pivotal issue in a securities class action suit. To avoid dismissal, plaintiffs must persuasively argue the firm *intentionally* misstated its financial statements, leading to an inflated stock price. Second, plaintiffs are not allowed access to company records through discovery until after they withstand the motion to dismiss, so allegations must draw on publicly available information unless the plaintiff obtains inside information from a whistleblower. Third, because a restatement establishes the misstatement element, intent is usually the only contested question in cases involving restatements. Thus, restatements increase the probability of both a lawsuit filing and a settlement (see Johnson et al. 2007). On the other hand, if firms do not issue restatements, then plaintiffs must convincingly allege both a misstatement and intent.

Against this backdrop, the precise effects of rules-based standards on the threat of litigation are unclear. We distill arguments relating rules-based standards to litigation into two competing theories. The first theory, which we label the "protection" theory, incorporates all arguments that make firms less likely to be sued or less likely to lose conditional on being sued. This theory thus predicts that rules-based standards shield firms from litigation. The competing view is the "roadmap" theory, which includes all arguments that make plaintiffs more likely to file a suit or prevail in a suit conditional on its filing. This theory contends that the specificity of rules-based standards provides plaintiffs with a clear path to successful litigation.

The "protection" theory is based on two arguments. First, when firms do not publicly admit to a misstatement, they can assert that they followed the detailed guidance of rules-based standards. Thus, in cases not involving a restatement, rules provide a "safe harbor" that protects firms from litigation. Principles-based standards, by way of contrast, do not afford such protection, leaving firms open to *ex post* claims that they did not exercise reasonable judgment when preparing financial statements. Second, in cases involving an admitted misstatement, the complex nature of rules-based standards offers an "innocent misstatement" defense that deters litigation. This argument stems from the belief that characteristics of rules-based standards (including volumes of implementation guidance, exceptions, and high levels of detail) make standards complex. Managers violating rules-based standards can argue that the misstatement was an innocent mistake caused by the complexities of GAAP, rather than an intentional violation.

The competing "roadmap" theory only applies to instances in which an identifiable misstatement occurred, as evidenced by a restatement. This theory argues that the detail and objectivity of rules-based standards help to establish intent, as plaintiffs can argue that it is implausible that executives would unintentionally violate the clear guidance provided by a rules-based standard. In essence, this theory is the converse of the innocent misstatement defense discussed above and argues that rules-based standards increase the threat of litigation.

The first challenge in testing these theories involves measuring whether standards are rules-based or principles-based. We identify the key characteristics of rules-based standards espoused by academics, practitioners, and regulators. While some believe that "rules-based" simply refers to bright-line thresholds, such a definition is incomplete. Rules-based accounting standards are highly detailed, contain scope exceptions, and require a significant amount of implementation guidance (e.g., FASB 2002; Nelson 2003; Schipper 2003; SEC 2003; DiPiazza et al. 2006). Despite the perception that U.S. GAAP is rules-based, there is considerable variation in the extent to which U.S. accounting standards exhibit these characteristics (e.g., Cunningham 2007). We exploit these within-GAAP differences and use the Rules-Based Continuum (RBC) score from Mergenthaler (2010) to measure the extent to which individual accounting standards contain: (1) bright-line thresholds, (2) scope and legacy exceptions, (3) large volumes of implementation guidance, and (4) a high level of detail. Higher RBC scores represent standards with more of these rules-based characteristics.

We begin by validating the RBC measure. Across a variety of tests, we find evidence suggesting RBC is a valid proxy that measures whether standards are rules-based. For instance, consistent with common perceptions, our findings suggest that U.S. GAAP is more rules-based than IFRS. In addition, the RBC measure produces high scores for standards the SEC (2003) classified as rules-based in its 2003 report, thus confirming the RBC measure lines up well with the SEC's view of what constitutes a rules-based standard.

Next, we use lawsuits citing specific accounting standards from 1996–2005 to test the protection and roadmap theories. We examine lawsuits in which the defendant firm does not admit to a prior accounting misstatement ("non-restatement cases"), as well as lawsuits where the firm does admit to such a misstatement ("restatement cases"). As discussed above, the protection theory relates to both non-restatement and restatement cases, while the roadmap theory relates only to cases involving accounting restatements.

Non-restatement cases involve the announcement of bad news, which leads to a material stock price drop. Plaintiffs typically argue the financial statements violated GAAP and did not reflect the true economic state of the firm prior to this news revelation. If the protection theory is correct, then we expect plaintiffs to allege relatively principles-based violations in these cases, as plaintiffs will not have the objective evidence provided by a restatement to demonstrate that specific rules-based standards have been violated. Thus, this theory posits that rules-based standards provide a safe harbor from litigation. To test this conjecture, we use each firm as its own control and compare the RBC score of the alleged violations to that of the accounting standards important to the firm—those contained in its Summary of Significant Accounting Policies 10-K disclosure. Consistent with the safe harbor arguments of the protection theory, we find the RBC score of allegedly violated standards in filed lawsuits is significantly lower (i.e., more principles-based) than the RBC score of standards important to the firm. Thus, absent an admitted accounting violation, plaintiffs tend to target principles-based areas of GAAP.

We next examine restatement cases. The protection theory predicts that admitted violations of rules-based standards should be associated with a lower threat of litigation because rules-based standards provide defendants with an innocent misstatement defense. In contrast, the roadmap theory predicts that violations of clear, specific rules-based standards are more likely to be perceived as intentional, which will provide plaintiffs with stronger cases and thereby increase the threat of litigation. To test these theories, we compare our lawsuit sample against a control sample of admitted accounting violations that could have plausibly resulted in a lawsuit, but did not. Specifically, the control sample includes restatements that resulted in a significant decline in market value without associated litigation. We find that violations of rules-based standards are less likely to result in a lawsuit filing, which is consistent with the protection theory. We also examine whether rules-based standards affect case outcomes. Again, the protection and roadmap theories produce competing predictions. We find no evidence that rules-based standards are related to case outcomes. Overall, we find support for the protection theory with respect to litigation incidence but not case outcomes. We find no support for the roadmap theory with respect to litigation incidence or outcomes.

This study makes two contributions. Most importantly, we provide the first archival evidence on the relation between rules-based accounting standards and litigation. Despite important public policy implications, there is a dearth of research in this area. Our evidence is particularly relevant to policy debates because we utilize a comprehensive database of securities class action lawsuits. Our sample comprises the universe of GAAP-related lawsuits that cite accounting standards among economically significant firms from 1996–2005. In addition, we distill arguments relating accounting standards to litigation into a simple framework involving two competing theories. This framework can be used to guide future policy debates and foster additional academic research in this area.

Section II discusses rules-based standards, prior literature, and features of the U.S. litigation environment. Section III discusses the relevant theories in more detail and develops testable hypotheses. Section IV contains our research design, empirical results, and robustness checks. Section V discusses limitations of our study. Section VI concludes and discusses the policy implications of our findings.

II. LITERATURE REVIEW AND INSTITUTIONAL BACKGROUND

Characteristics of Rules-Based Accounting Standards

Despite significant recent focus on rules-based and principles-based accounting standards, "The exact meaning of this distinction is not well defined and is often unclear" (Hail et al. 2010, 376). There is agreement that rules-based standards often involve bright-line thresholds (e.g., Kadous and Mercer 2012). However, a review of commentary by academics and regulators makes it clear that the characteristics of rules-based standards involve more than bright-line thresholds. These characteristics include scope and legacy exceptions, a high level of detail, and extensive implementation guidance (FASB 2002; Nelson 2003; Schipper 2003; SEC 2003).[1] The SEC (2003, 23), for example, indicates that "the primary characteristics of rules-based standards are the existence of exceptions and bright-lines which lead to very detailed implementation guidance."

The SEC (2003) suggests these characteristics make standards more precise and lower the amount of professional judgment required in application. For example, scope and legacy exceptions provide a lengthy list of transactions to which a standard does not apply, making it clearer whether the standard applies to a particular transaction. Increased detail and implementation guidance similarly limit the amount of professional judgment required, while bright-line thresholds provide objective guidance regarding application of the standard. Nelson (2003) notes that the presence of these characteristics makes a standard both more specific and complex, and thus more rules-based.

Many standards begin with a simple principle, but ultimately begin to take on rules-based characteristics. As Schipper (2003, 67) states:

> Once a standard lays out a financial reporting principle, the only way to permit deviations from that principle is to include in the standard a rule or rules that provide for some combination of excluded items (scope exceptions) or items that receive special treatment (legacy exceptions). These rules add to the length and complexity of the standard, and lead to requests for explanations of the breadth of the exceptions.

Because the nature of rules-based standards is multifaceted, we use all of the characteristics described above (bright-line thresholds, scope and legacy exceptions, a high level of detail, and implementation guidance) in our empirical analysis when measuring the extent to which standards are rules-based.

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[1] Scope and legacy exceptions are exceptions to standards that permit alternative treatments for a defined set of transactions or for a specific industry (Schipper 2003).

Prior Research

Most studies related to rules-based versus principles-based accounting standards investigate whether rules-based standards affect financial reporting decisions.[2] There is significantly less work addressing legal or regulatory enforcement with respect to GAAP violations. Kadous and Mercer (2012) utilize an experiment to investigate the effect of bright-line thresholds on juror decisions in auditor negligence cases.[3] In another recent study, Mergenthaler (2010) finds that the SEC is less likely to penalize a rules-based GAAP violation.

However, no study examines the effect of rules-based standards in the securities litigation context. Regulatory enforcement efforts and auditor negligence cases are not a good proxy for the private enforcement arena of securities litigation due to differing incentives (see Coffee 2006) and institutional details. The SEC is a regulatory agency whose enforcement actions are undertaken to shape financial reporting policy and punish companies for not complying with securities laws (Cox and Thomas 2003). Class action plaintiffs are focused on the potential recoverable settlement amount. In addition, auditor negligence cases have a much lower intent threshold than securities class actions, where fraud must be alleged. Thus, direct evidence on the effect of rules-based standards on the threat of litigation is needed in the securities class action context. As we discuss below, the securities class action litigation environment in the U.S. is unique, and key procedural hurdles must be met for plaintiffs to bring a successful suit.

Securities Litigation Background

The securities class action litigation environment is now largely defined by the Private Securities Litigation Reform Act of 1995 (PSLRA), which was passed to limit frivolous securities litigation. Prior to the passage of the PSLRA, securities class action complaints were often filed whenever a significant stock price decline occurred, regardless of the presence of actual fraud. Complaints were often of low quality and consisted largely of boilerplate allegations (Casey 2008). The PSLRA limits such practice, requiring that complaints allege facts that yield a "strong inference that the defendant acted with the required [fraudulent] state of mind" (15 U.S.C. section 78u-4(b)(2)).[4] The PSLRA also prevents discovery until after a motion to dismiss is decided, making the motion to dismiss the key procedural hurdle.[5]

To survive a motion to dismiss, a complaint must allege facts sufficient to satisfy each element of Rule 10b-5, including "(1) a misstatement or omission of (2) a material fact (3) made with intent (4) that the plaintiff justifiably relied on (5) causing injury in connection with the purchase or sale of securities" (Skinner 1994).[6] Based on the elements of Rule 10b-5, accounting-related securities class action lawsuits follow a reasonably predictable pattern. First, to satisfy element (1), a firm either admits to a misstatement by restating its prior financial statements ("restatement cases"), or shareholders infer a misstatement or omission of prior financial statements ("non-restatement cases"). Second, the revelation or alleged discovery of the misstatement is accompanied by a large stock price decline, satisfying elements (2) (materiality) and (5) (causation). Element (4) is generally presumed under the fraud on the market theory (see Francis et al. 1994). Thus, the key element in most accounting-based securities litigation is element (3), fraudulent intent (see Pritchard and Sale 2005). To establish intent, the plaintiff must be able to allege facts that indicate that the defendant firm's managers acted with scienter. In the law, acting with scienter means that the defendant's attempt to deceive or defraud was intentional or so reckless that the act "reflects some degree of intentional or conscious misconduct" (*In re Silicon Graphics, Inc. Securities Litigation*, 183 F.3d 970, 977 (9th Cir. 1999)). If the plaintiffs' complaint alleges scienter with sufficient factual detail and is not dismissed, then securities class action lawsuits almost always settle rather than go to trial.

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[2] Nelson et al. (2002) find that executives manage earnings by structuring transactions when standards are "precise," and that executives manage earnings in an "unstructured" manner when standards are "imprecise." Cuccia et al. (1995) provide evidence that executives use judgment to defend aggressive tax behavior when standards are vague, and that they evaluate the evidence in a manner consistent with the desired outcome when standards are strict. Mergenthaler (2010) finds that the magnitude of earnings management is larger when a rules-based standard is violated. Finally, Jamal and Tan (2010) find that while managers' decisions may differ based on the type of accounting standard, their reporting choices are affected by auditor type (principles-oriented, rules-oriented, or client-oriented).

[3] Kadous and Mercer (2012) find that bright-lines shield auditors from litigation when the client reporting decision is both conservative and deviates from the industry norm. However, they find that merely complying with the industry norm renders the bright-line threshold unimportant in such cases. When the client reporting decision is aggressive, however, bright-lines increase liability of auditors, regardless of compliance with industry norms.

[4] The Supreme Court interpreted the "strong inference" requirement to mean that an inference of fraud "must be more than merely plausible or reasonable—it must be cogent and at least as compelling as any opposing inference of non-fraudulent intent" (*Tellabs, Inc. v. Makor Issues & Rights, Ltd.*, 551 U.S. 308 (2007)).

[5] Preventing discovery greatly reduces the cost of non-meritorious suits. However, if a motion to dismiss fails, then corporations are subject to discovery, requiring high-level officials to submit to depositions and document production. Thus, even if a company believes it would prevail at trial, it is cost-efficient to settle to avoid litigation costs and the large potential damages associated with losing a class action case (e.g., Pritchard and Sale 2005).

III. HYPOTHESES DEVELOPMENT

We review the academic literature, popular press articles, and U.S. case law to distill arguments on the relation between rules-based accounting standards and litigation into two competing theories. Figure 1 outlines the two theories (described in Row A). We label the first the "protection" theory, which incorporates all arguments that make the firm less likely to be sued or less likely to lose if they are sued. In other words, the protection theory contends that rules-based standards decrease the threat of litigation. We label the second the "roadmap" theory, which includes all arguments that make plaintiffs more likely to file a suit or prevail in a suit conditional on its filing. In other words, this theory contends that rules-based standards increase the threat of litigation.

Figure 1 next provides a discussion of the elements of each theory (described in Row B), and relates these elements to observable events—whether a restatement has been issued by the firm (described in Row C). Linking the theories to these observable events is important because the protection theory offers predictions in both non-restatement and restatement cases (detailed on the left side of Figure 1), while the roadmap theory only deals with detected misstatements and thus only offers a prediction for restatement cases (detailed on the right side of Figure 1). Therefore, we organize our hypotheses and tests around whether a restatement is present. We begin with non-restatement cases for which our only hypothesis comes from the protection theory, due to the inapplicability of the roadmap theory in such cases.

Non-Restatement Cases

In cases not involving a restatement, plaintiffs must argue that defendants misstated their financial statements and that the misstatement was intentional. These cases typically arise following the announcement of bad news that results in a significant stock price drop. Plaintiffs usually allege that a firm's financial statements were misstated in violation of GAAP and did not accurately portray the underlying economic state of the firm prior to the stock price drop.[7] Plaintiffs must be

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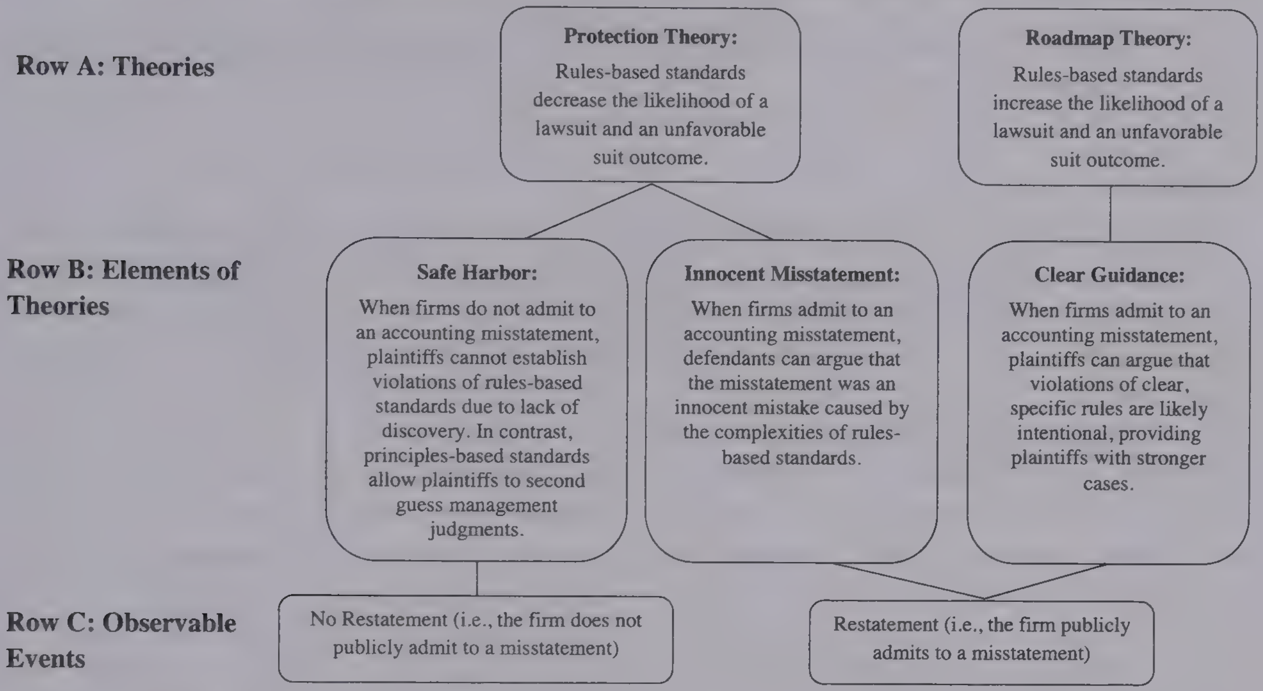
[6] Firms may also be liable under Section 11 of the 1933 Securities Act, but only in connection with the public offering of securities. The elements of a Section 11 violation are similar to Rule 10b-5, except that the intent standard is lower.

[7] In these cases, defendants do not admit to a misstatement and do not restate their financial statements, even if the case is ultimately settled. Settlement in these cases is generally undertaken to avoid the cost of a trial (and a potentially sizable jury verdict) once the court denies a motion to dismiss. The defendant generally does not admit wrongdoing, even in the settlement agreement (Alexander 1991).

FIGURE 1
Summary of Theories Relating Rules-Based Accounting Standards and Litigation


Row A: Theories
Protection Theory:
Rules-based standards decrease the likelihood of a lawsuit and an unfavorable suit outcome.
Roadmap Theory:
Rules-based standards increase the likelihood of a lawsuit and an unfavorable suit outcome.
Row B: Elements of Theories
Safe Harbor:
When firms do not admit to an accounting misstatement, plaintiffs cannot establish violations of rules-based standards due to lack of discovery. In contrast, principles-based standards allow plaintiffs to second guess management judgments.
Innocent Misstatement:
When firms admit to an accounting misstatement, defendants can argue that the misstatement was an innocent mistake caused by the complexities of rules-based standards.
Clear Guidance:
When firms admit to an accounting misstatement, plaintiffs can argue that violations of clear, specific rules are likely intentional, providing plaintiffs with stronger cases.
Row C: Observable Events
No Restatement (i.e., the firm does not publicly admit to a misstatement)
Restatement (i.e., the firm publicly admits to a misstatement)

This figure summarizes the theories that suggest a relation between rules-based standards and litigation. The protection theory applies regardless of whether the firm publicly admits to a misstatement. The roadmap theory only applies when firms publicly admit to a misstatement. Absent such an admission, plaintiffs cannot determine whether firms followed the rules because discovery is not permissible until after the dismissal decision. Thus, without a restatement, plaintiffs generally cannot establish violations of specific rules as required by the roadmap theory.

able to "plead" (present in the complaint) facts that support these elements without access to company records via discovery. Thus, plaintiffs rely on publicly available information to make their initial case, absent a whistleblower.

The arguments for the protection theory as they apply in non-restatement cases are summarized in Figure 1, Row B in the "Safe Harbor" box. Specifically, the protection theory argues that rules-based accounting standards decrease the threat of litigation in non-restatement cases. Under this argument, the detailed guidance provided by rules-based standards provides firms with "safe harbor" protection from litigation when firms follow the rules (Schipper 2003; SEC 2003). Reinforcing this aspect of the protection theory, it is also difficult for potential plaintiffs to allege a specific violation of a rules-based standard due to the lack of discovery. Without discovery, plaintiffs are unable to observe sufficient detail about a firm's transactions to know whether a rules-based accounting standard was violated. For example, it would be difficult to determine whether a bright-line was crossed, the requirements of an exception were met, or whether the transaction fit into a particular area of implementation guidance without either specific records of transactions or testimony from individuals involved in the transaction.

On the other hand, principles-based standards are subject to more judgment and have less objective criteria compared to rules-based standards, so plaintiffs can more persuasively allege that their application was improper and "second guess" accounting decisions after the fact. Even if a firm attempts to follow a principles-based standard, its decision may still be questioned by plaintiffs due to the discretion involved.

Overall, in cases not involving a restatement, the protection theory predicts that rules-based standards will tend to shield firms from litigation and plaintiffs will thus tend to allege violations of principles-based standards. We state our first hypothesis in the alternative:

> **H1:** Plaintiffs are less likely to allege violations of rules-based accounting standards in non-restatement cases.

Restatement Cases

We next consider restatement cases for which the protection and roadmap theories offer competing predictions. In restatement cases, defendant firms admit to an accounting misstatement via a restatement before or soon after the filing of a lawsuit. Thus, the misstatement element is not disputed and the safe harbor protection that accompanies compliance with rules-based standards is inapplicable.

The protection theory suggests that rules-based standards shield firms from litigation, even after a restatement. Rules-based standards include exceptions, high levels of detail, and large amounts of implementation guidance, making such standards more complex and difficult to apply than principles-based standards. Managers can therefore utilize the complex nature of rules-based standards to argue that violations of rules-based standards were "innocent misstatements." Both standard-setters and courts agree that complex standards are more difficult to apply and can lead to errors. For instance, the former Chairman of the IASB, David Tweedie (2007, 5), stated that rules-based standards "frequently baffle many accountants so much so that few audit partners can complete an audit without relying on the advice of experts within the firm." In the Microstrategy case (*In re Microstrategy, Inc. Securities Litigation*, 115 F. Supp. 620, 652 (E.D. Va. 2000)), the court held that the more complex the accounting standard, the lower the inference of fraudulent intent. Furthermore, courts generally assume misstatements are innocent errors, absent convincing evidence of intent (scienter) and "the mere publication of inaccurate accounting figures, or a failure to follow GAAP, without more, does not establish scienter" (*Dsam Global Value Fund v. Altris Software, Inc.*, 299 F.3d 385 (9th Cir. 2002)). These arguments are summarized in Figure 1, Row B, in the "Innocent Misstatement" box.

Proponents of the "roadmap" theory offer an alternative view, contending that rules-based standards provide plaintiffs with a guide to successful litigation. The idea behind the roadmap theory is that violations of rules-based standards help demonstrate intent, as plaintiffs can argue that it is implausible that executives would unintentionally violate a specific rule that provides clear guidance. For example, Leone (2009) cites a veteran securities litigator who notes that "in a case based on a principle, it's 'really hard' to prove that executives or directors acted in bad faith." The roadmap view also predicts that the objectivity and specificity of rules-based standards make it easier to identify accounting misstatements, triggering restatements. Since restatements tend to increase the threat of a lawsuit and lead to more favorable outcomes for plaintiffs (Johnson et al. 2007), the roadmap theory predicts that rules-based (principles-based) standards increase (decrease) litigation risk. For example, Bogoslaw (2008) states that "the big accounting firms that are drawn to [a more principles-based] IFRS believe they'll get sued less since it will be harder to point to their mistakes."[8] These arguments are summarized on the right side of Figure 1, Row B, in the "Clear Guidance" box. Overall, the roadmap theory predicts that violations of rules-based standards will increase the threat of litigation, while the protection theory predicts the opposite. Although both arguments seem plausible, the "innocent misstatement" defense of the protection theory is more strongly supported by U.S. case law. We therefore state our second hypothesis as follows:

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[8] Under this portion of the roadmap theory, it is possible that principles-based standards may shield firms from litigation if violations of these standards are more difficult to detect and, thus, are less likely to result in restatements, but testing this aspect of the roadmap theory is beyond the scope of our study because we are unable to model the restatement decision (i.e., we cannot observe potential misstatements that did not result in a restatement).

> **H2:** Violations of rules-based standards are negatively associated with the incidence of litigation in restatement cases.

Although we focus primarily on the lawsuit filing decision, we also examine case outcomes to present a more complete picture of the litigation process. From the discussion above, the roadmap theory predicts that violations of rules-based standards will lead to better outcomes for plaintiffs, including a greater settlement probability in restatement cases. The protection theory again predicts the opposite in restatement cases, as shown in the "Innocent Misstatement" and "Clear Guidance" boxes of Figure 1, Row B. Again, since the "innocent misstatement" defense of the protection theory appears to have more support in case law, we state our third hypothesis as follows:

> **H3:** Violations of rules-based standards are negatively associated with the plaintiffs' outcome in restatement cases.

If plaintiffs' attorneys consider the effect of rules-based standards on case outcomes at the filing stage, then this could lead to no relation between rules-based standards and suit outcomes *among filed cases*. For example, if H2 is true and cases involving rules-based standards have poorer prospects for plaintiffs on average, those cases involving rules-based standards that plaintiffs do choose to file may have relatively high prospects along other dimensions. Thus, among cases actually filed, this selection effect could lead to no relation between rules-based standards and lawsuit outcomes.

IV. EMPIRICAL ANALYSIS

Measurement of Rules-Based Standards

To determine the extent to which a standard is "rules-based," we utilize the Rules-Based Continuum (RBC) score from Mergenthaler (2010) to measure the presence of rules-based characteristics for accounting standards in our sample. Because many accounting standards are based upon broad principles from the FASB conceptual framework, we avoid subjectively classifying standards as being rules-based or principles-based. Instead, we adopt an empirical perspective and focus on the presence or absence of rules-based characteristics identified by prior literature.

The RBC score measures the extent to which standards contain the rules-based characteristics noted in Section II, including (1) bright-line thresholds, (2) scope and legacy exceptions, (3) large volumes of implementation guidance, and (4) a high level of detail. The RBC score increases by 1 for each such characteristic a standard contains.[9] Thus, the value of the RBC score ranges from 0 (a standard contains none of the characteristics, representing a principles-based standard) to 4 (a standard contains all of the characteristics, representing a highly rules-based standard).[10]

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[9] The number of FASB "interpretive pronouncements" is the proxy for the amount of implementation guidance. Standards ranked in the upper decile of interpretive pronouncements are classified as having large volumes of implementation guidance. The number of words in a standard measures detail, with standards in the upper word count decile classified as having a high level of detail. We also examine whether our results hold when we change our definition of large volumes of implementation guidance and detail to be the upper quartile of the distribution and find that our results are robust to this alternative way of defining these characteristics.

RBC Validation

We validate the RBC score in three ways to ensure that we are capturing the underlying construct of rules-based accounting standards. First, we demonstrate that the four dimensions of *RBC* are all positively correlated. Panel A of Table 1 provides the correlations between the four dimensions that comprise the RBC score in our sample. Standards with bright-line thresholds, for example, tend to have more implementation guidance ($\rho = 0.55$). An untabulated factor analysis reveals only one significant factor in the data, consistent with the notion that the four elements of the RBC score proxy for a common construct. Thus, we aggregate the four characteristics into one measure.

Second, Panel B of Table 1 outlines the RBC score for select standards the SEC (2003) classified as "principles-based" or "rules-based" and demonstrates a consistent overall relation between the RBC score and the SEC's perception of "rules-based" accounting standards. In fact, the RBC score for almost all of the standards the SEC classifies as "principles-based" ("rules-based") have a low (high) RBC score. For instance, ARB 43-4 (inventory pricing), a standard the SEC referred to as "principles-based," has an RBC score of 0, indicating that ARB 43-4 does not contain any of the aforementioned rules-based characteristics. On the other hand, SFAS 13 (leases), a "rules-based" standard per the SEC, has a score of 4, indicating that it contains all of the rules-based characteristics and is therefore more rules-based. SFAS 133 (derivatives) has an RBC score of 3, also consistent with relatively high rules-based content and the SEC classification.

There are three standards in which the SEC's classification of a standard as "principles-based" does not match well with the RBC score: business combinations (SFAS 141), goodwill (SFAS 142), and long-lived asset impairment (SFAS 144). While none of these standards contain bright-line thresholds, they each contain scope exceptions and a high level of implementation guidance. In addition, each standard is very detailed, with total lengths of 19,298 words (SFAS 141), 18,824 words (SFAS 142), and 21,259 words (SFAS 144). These standards have fairly high RBC scores as they exhibit all rules-based characteristics other than bright-line thresholds. Thus, despite the inconsistency with the SEC treatment, the *RBC* classification of these standards as relatively rules-based seems reasonable.[11]

Third, for each U.S. standard examined by the SEC (2003), we compute the RBC score for the corresponding IFRS standard. After tabulating the RBC score, we compare the RBC score of each IFRS standard to that of the corresponding U.S. GAAP standard. This comparison is meaningful because IFRS is commonly perceived to be less rules-based than U.S. GAAP. Thus, if the RBC score correctly measures whether a standard is rules-based, then RBC scores should be higher for U.S. GAAP than for IFRS. In almost all cases, the RBC score for the IFRS standard is less than or equal to the RBC score for the corresponding U.S. GAAP standard. An untabulated test of differences in the median (mean) RBC score reveals that the median (mean) RBC score of the IFRS standards presented is significantly lower (z-statistic $= -2.29$, one-sided p-value $= 0.01$; t-statistic $= -1.95$, one-sided p-value $= 0.03$) than the RBC score of the corresponding U.S. GAAP standards. We view the higher RBC score of U.S. GAAP relative to IFRS as a particularly interesting finding because the underlying transactions are held constant across accounting regimes. Thus, the RBC

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[10] *RBC* is calculated each year for each standard because standards change over time as detail, exceptions, and implementation guidance are added.

[11] Schipper (2003) discusses SFAS 141 and 142 as being based on principles, but suggests that additional guidance provided by the FASB makes these standards more rules-based.

TABLE 1

Rules-Based Continuum Construct Validity

Panel A: Pearson Correlations among Rules-Based Characteristics

	Detail	Implementation Guidance	Exceptions
Implementation Guidance	0.62 (< 0.0001)		
Exceptions	0.25 (< 0.0001)	0.65 (< 0.0001)	
Bright-Line Thresholds	0.19 (< 0.0001)	0.55 (< 0.0001)	0.38 (< 0.0001)

Panel A displays Pearson correlations between the individual characteristics of rules-based standards.

Panel B: Comparison of Select IFRS and U.S. GAAP Standards

Description	IFRS Standard	IFRS *RBC*	U.S. Standard	U.S. *RBC*	SEC Classification
Recognition of Financial Assets/Liabilities	IAS 39	3	SFAS 140	4	Rules-Based
Other Post-Retirement Benefits	IAS 19	2	SFAS 106	4	Rules-Based
Taxes	IAS 12	2	SFAS 109	4	Rules-Based
Stock-Based Compensation	IFRS 2	2	SFAS 123	4	Rules-Based
Leases	IAS 17	2	SFAS 13	4	Rules-Based
Pensions	IAS 19	2	SFAS 87	4	Rules-Based
Derivatives and Hedging	IAS 39	3	SFAS 133	3	Rules-Based
Accounting for the Sale of Real Estate	IAS 18	2	SFAS 66	3	Rules-Based
Consolidation	IAS 27	1	ARB 51	3	Rules-Based
Business Combinations	IFRS 3	2	SFAS 141	3	Principles-Based
Goodwill	IAS 38	2	SFAS 142	3	Principles-Based
Impairment of Long-Lived Assets	IAS 36	2	SFAS 144	3	Principles-Based
Asset Retirement Obligations	IAS 37	2	SFAS 143	2	Principles-Based
Foreign Currency	IAS 21	1	SFAS 52	2	Principles-Based
Exit or Disposal Costs (Restructuring)	IAS 37	2	SFAS 146	0	Principles-Based
Inventory	IAS 2	1	ARB 43-4	0	Principles-Based
Interest Capitalization	IAS 23	1	SFAS 34	0	Principles-Based
Impairment of Long-Lived Assets	IAS 36	2	SFAS 121	1	Principles-Only

Panel B presents data on the median RBC score for standards the SEC classified as "rules-based" or "principles-based" and presents the RBC score of the corresponding IFRS standards.

RBC measures whether a standard contains bright-line thresholds, scope and legacy exceptions, large volumes of implementation guidance, and a high level of detail. *RBC* increases by 1 when a standard contains any one of the above-mentioned criteria. *RBC* is calculated each year for each standard because standards change over time as detail, exceptions, and implementation guidance are added.

score captures differences in the characteristics of accounting standards independent of the transactions underlying the standards.

Collectively, results from the correlations between the components of *RBC*, the factor analysis, the comparison to standards classified by the SEC, and the comparison to IFRS validate the RBC score and suggest that it is a reasonable measure of whether a standard contains rules-based characteristics.

Sample

We select all resolved securities class action lawsuits filed from 1996 to 2005 alleging GAAP violations from the Securities Class Action Services (SCAS) database from RiskMetrics Group. We exclude suits based purely on disclosure theories outside financial reporting. We end the sample in 2005 because we require settlement outcomes in later tests, and cases generally take several years to resolve. Starting the sample in 1996 ensures that all suits occur within a similar legal framework under the PSLRA.

We examine lawsuits involving common stockholders and require defendant firms to have coverage in I/B/E/S and CRSP. Requiring analyst coverage aids in ensuring reasonably efficient incorporation of information into stock prices (Piotroski and Roulstone 2004). Requiring CRSP coverage excludes thinly traded OTC stocks. Together, these restrictions ensure that sued firms are of interest to a broad range of investors and are economically significant. We also require firms to have non-missing values for the variables in our empirical tests.

For firms that meet these data requirements, we also screen for an available plaintiff complaint in the Stanford Securities Class Action Clearinghouse. We read each complaint and note the accounting standards allegedly violated, and collect additional variables used in our empirical analysis.[12] This procedure yields 353 cases that cite specific accounting standards, as show in the top box of Figure 2. Figure 2 provides a summary of our samples, including the portion of the lawsuit sample and the designated control sample utilized in each test.

From the 353 total cases that cite accounting standards, 164 do not involve an admitted accounting misstatement by the defendant firm. We identify these non-restatement suits by searching the complaint for the term "restat*" and initially classify cases not containing this term as non-restatement cases. To verify that these suits did not involve a restatement, we further search all 10-K and 10-Q filings for up to one year after the class period end date to verify that the firm did not restate their financial statements prior to the plaintiffs' filing decision. This sample of 164 lawsuits without restatements is located on the bottom left of Figure 2. We use this sample to test H1 by comparing standards cited in the suit to the firm's significant accounting policies (described in more detail below), which applies to the "Safe Harbor" element of rules-based standards as detailed in the "Hypotheses Development" section and in Figure 1, Row B.

The remaining 189 cases involve an admitted misstatement in the form of a restatement of class period financial statements by the defendant firm. As a control sample, we select all restatements identified by the Government Accountability Office (GAO) from the same time period that did not result in litigation, but whose announcements were accompanied by a decline in market value of equity of at least $50 million, resulting in 84 restatements.[13] We use the $50 million cutoff because

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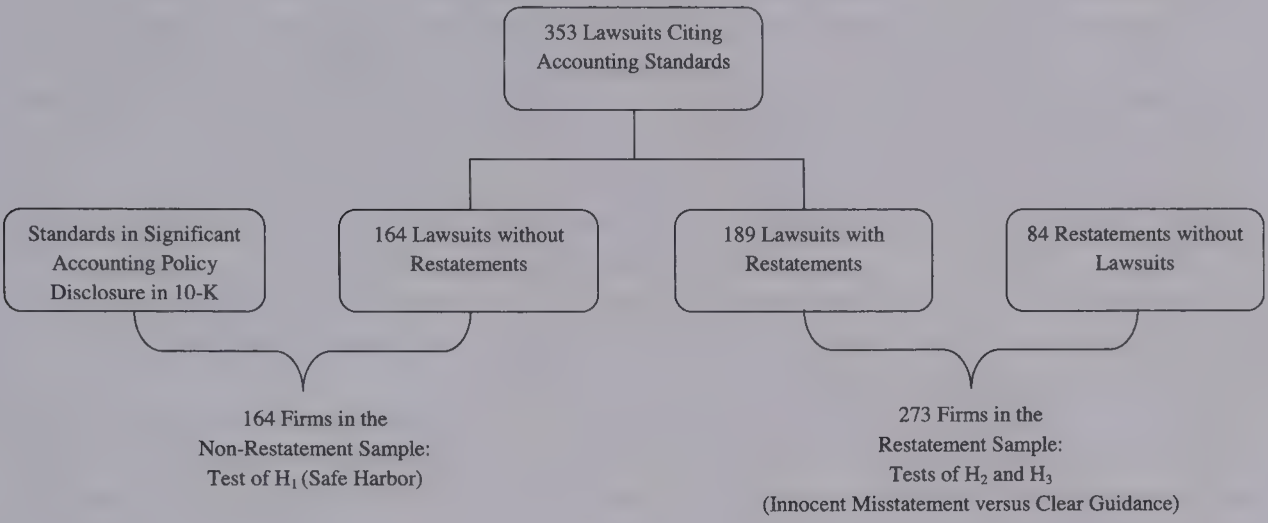
[12] To determine if plaintiffs strategically cite certain standards, we selected a random sample of 50 restatement cases. We then compared the violations noted in the complaint to those noted in the restatement. Of the 50 cases we looked at, 48 cite every GAAP violation noted in the restatement. The two remaining cases cited all standards except standards noted as "minor errors" in the restatement. Thus, the plaintiff appears to cite all GAAP violations noted in the restatement filing.

[13] We obtain these restatements from the 1997–2006 GAO sample, which is used in the Hennes et al. (2008) study and is available in machine readable format on Andrew Leone's website (http://sbaleone.bus.miami.edu/).

FIGURE 2
Sample Overview


353 Lawsuits Citing Accounting Standards
Standards in Significant Accounting Policy Disclosure in 10-K
164 Lawsuits without Restatements
189 Lawsuits with Restatements
84 Restatements without Lawsuits
164 Firms in the Non-Restatement Sample: Test of H_1 (Safe Harbor)
273 Firms in the Restatement Sample: Tests of H_2 and H_3 (Innocent Misstatement versus Clear Guidance)

This figure outlines the samples used in our various analyses. We start with 353 lawsuits that cite accounting standards. We then distinguish between lawsuits involving restatements and those not involving restatements. Plaintiffs have different information about the alleged GAAP violation in restatement and non-restatement cases because discovery is not allowed until after the dismissal decision. Thus, in non-restatement cases, plaintiffs must build their cases on publicly available information without direct admission of a misstatement. Our first analysis focuses on lawsuits not involving a restatement. Our sample consists of 164 non-restatement lawsuits, with each sued firm serving as its own control. We compare the standards cited in the lawsuit to those mentioned in the firms' significant accounting policy disclosures. Our second set of tests focus on instances where a restatement is observed. In our sample of lawsuits citing accounting standards, there are 189 lawsuits involving restatements. We then select a sample of "potential lawsuits" involving restatements where the firm restated earnings and the firm's market value dropped $50 million or more around the announcement of the restatement. The 84 restatements in the control sample are obtained from the 1997–2006 GAO sample, which is used in the Hennes et al. (2008) study and is available in machine readable format on Andrew Leone's website (http://sbaleone.bus.miami.edu/). We compare the standards cited in the lawsuits to those disclosed in connection with the non-sued firms' restatements.

potential damages are an important consideration in the filing decision (Coffee 2006; Alexander 1991). We use these 273 restatement observations (189 that triggered lawsuits and 84 that did not, as detailed on the bottom right of Figure 2) to test H2 and H3, which deal with testing the competing "Innocent Misstatement" and "Clear Guidance" aspects of rules-based standards, as detailed in the hypothesis development and in Figure 1, Row B.

Test of H1

To test H1, we search the complaint document to identify each alleged violation and ensure that the firm did not restate earnings. We then read the firm's significant accounting policy disclosure in the firm's 10-K from the alleged violation period to identify the standards that the firm labels "significant." Classifying the significant accounting standards for each firm provides a sample of standards important to the firm that the plaintiff could observe and thus could have alleged the firm violated. We then compare the average RBC score across all of the alleged violations for each lawsuit to the average RBC score across all of the standards contained in the significant accounting policies. Because each sued firm serves as its own control, we conduct our analysis on a univariate basis. A significantly lower RBC score for the alleged violations in lawsuits would indicate that plaintiffs tend to allege more principles-based standards when firms do not admit to a GAAP violation.

Appendix A provides one example of a firm's significant accounting policy disclosure and documents how we classified this disclosure to identify the firm's significant accounting standards. In this example of Policy Management Systems Corp., we list the identified standards and their RBC measures immediately before the paragraphs discussing each significant accounting policy.

Panel A of Table 2 provides descriptive statistics for the non-restatement cases. The mean RBC score of the alleged violations is 0.87, suggesting that alleged GAAP violations contain roughly one rules-based characteristic. Consistent with expectations, the disclosure of bad news at the end of the class period elicits an average return of negative 25.08 percent. In untabulated analysis, we find that the most common events triggering these cases include (with the incidence in parentheses): earnings warnings (48.2 percent), earnings announcements revealing poor performance (17.7 percent), announcements of internal or external investigations (11.0 percent), and announcements of bankruptcy/insolvency issues (4.9 percent).[14] Finally, 41 percent of these firms come from high litigation risk industries, as identified by Francis et al. (1994).

Panel B of Table 2 provides the 15 most commonly cited accounting standards in plaintiff complaints along with the 15 most common standards associated with defendants' significant accounting policies. The most commonly cited standards in non-restatement lawsuits include APB 22 (disclosure of accounting policies, $RBC = 0$), SFAS 5 (contingencies, $RBC = 1$), SFAC 5-6 (revenue, $RBC = 1$), SFAS 48 (right of return, $RBC = 1$), and ARB 43-4 (inventory write downs, $RBC = 0$).

The most common standards significant to these defendants include ARB 51 (consolidation, $RBC = 3$), ARB 43-9 (depreciation, $RBC = 0$), SFAC 5-6 (revenue, $RBC = 1$), SFAS 109 (taxes, $RBC = 4$), and SFAS 128 (EPS, $RBC = 3$). A visual comparison of the RBC scores in Panel B suggests that standards allegedly violated in non-restatement cases are less rules-based on average than firms' significant accounting standards.

Panel C of Table 2 provides a test of the differences between the mean (median) RBC score across the two samples. The standards that were allegedly violated according to the plaintiffs' complaints contain fewer rules-based characteristics than the firms' significant accounting standards (t-statistic $= -16.90$ for mean comparison, z-statistic $= -13.00$ for median comparison, both p-values < 0.01). The difference in RBC scores suggests that when selecting from a list of potential violations in non-restatement cases, plaintiffs tend to select standards that are relatively more principles-based.[15] This evidence supports H1 and is consistent with the "safe harbor" aspect of the protection hypothesis, which argues that plaintiffs will avoid rules-based standards and target principles-based standards in litigation because plaintiffs can second-guess managements' judgment more effectively under principles-based standards.

Robustness Tests for H1

One concern with our analysis above is that firms' significant accounting policies may not be an appropriate set of standards to benchmark against standards cited in lawsuit allegations. For example, firms may be unlikely to violate significant accounting policies because these standards receive more audit scrutiny.[16] If so, then comparing the allegedly violated accounting standards to significant accounting policies may not be an appropriate test.

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[14] Other less common events include announcements for defective products or patent infringement, nonfinancial fraud or enforcement actions, analyst or rating downgrades, and auditor disputes.

[15] To ensure results are not driven by the tendency of plaintiffs to cite revenue standards, which tend to be relatively principles-based, we re-ran this comparison by recalculating *RBC* after dropping all revenue standards cited in the lawsuits and the summary of significant accounting policies. Inferences are similar to those reported above.

[16] We thank an anonymous referee for this suggestion.

TABLE 2

Tests Using Non-Restatement Sample

Panel A: Descriptive Statistics

Variable	n	Mean	Median	Std. Dev.
RBC	164	0.87	0.80	0.57
SIZE	164	6.81	6.49	1.89
ROA	164	−4.55	0.11	14.05
BIG4	164	0.88	1.00	0.32
ANN_RET (%)	164	−25.08	−23.77	19.63
MAX_DAMAGE ($MM)	164	4,789	730	13,206
LN_MAX_DAMAGE	164	6.88	6.61	1.73
LIT_RISK	164	0.41	0.00	0.49
INSIDER	164	−0.20	−0.05	0.33
ISSUE	164	0.24	0.00	0.43
REVENUE	164	0.65	1.00	0.48
SHARE_TURN	164	1.57	1.22	1.18
VOLATILITY	164	5.03	4.52	2.28
DISMISS	164	0.41	0.00	0.49
SETTLEMENT ($MM)	94	35	9	121
SETTLEMENT/MAX_DAMAGE	94	0.02	0.01	0.03
SETTLEMENT/MVE	94	0.03	0.01	0.03

Panel A presents descriptive statistics for lawsuits where the firm does not restate earnings from the alleged violation period. Hence, the plaintiff alleges that GAAP was violated, but the firm does not admit to a violation. We label these lawsuits as "non-restatement lawsuits." We obtain lawsuit data from the RiskMetrics Securities Class Action Services database and complaints from the Stanford Securities Class Action Clearinghouse. We obtain the standards mentioned in the firm's significant accounting policies from each firm's 10-K that is filed closest to the lawsuit filing date. Variables are defined in Appendix B.

Panel B: Most Common Accounting Standards

	Non-Restatement Lawsuits			Significant Accounting Policies	
Standard	Description	*RBC*	Standard	Description	*RBC*
APB 10	Revenue Recognition/Taxes	0	ARB 43-4	Inventory Pricing	0
APB 22	Disclosure of Accounting Policies	0	ARB 43-9	PP&E Depreciation and Valuation	0
APB 28	Interim Financial Reporting	0	APB 17	Intangible Assets	1
ARB 43-3a	Current Assets and Current Liabilities	0	SFAC 5-6	Revenue Recognition	1
ARB 43-4	Inventory Pricing	0	SFAS 107	Financial Instrument Disclosure	1
ARB 45	Revenue Recognition (Construction)	0	SFAS 5	Contingent Liability	1
ARB 43-1a	Revenue Recognition	1	SOP 93-7	Advertising Costs	1
SFAC 5-6	Revenue Recognition	1	SFAS 52	Foreign Currency Translation	2
SFAS 121	Long-Lived Asset Impairment	1	ARB 51	Consolidation	3
SFAS 48	Revenue Recognition (Right of Return)	1	SFAS 115	Investments in Debt and Equity Securities	3
SFAS 5	Contingent Liabilities	1	SFAS 128	EPS Calculation	3
SOP 94-6	Disclosure of Significant Risks	1	SFAS 133	Derivatives	3

(continued on next page)

TABLE 2 (continued)

Non-Restatement Lawsuits			Significant Accounting Policies		
Standard	Description	*RBC*	Standard	Description	*RBC*
SAB 101	Revenue Recognition	2	SFAS 142	Intangible Assets	3
SOP 97-2	Revenue Recognition (Software)	2	SFAS 109	Taxes	4
SFAS 115	Investments in Debt and Equity Securities	3	SFAS 123	Stock Option Compensation	4

Panel B reports the 15 standards most commonly cited in non-restatement cases and these firms' significant accounting policy disclosure. The RBC score of these standards is also presented. The RBC score measures whether a standard contains bright-line thresholds, scope and legacy exceptions, large volumes of implementation guidance, and a high level of detail. *RBC* increases by 1 when a standard contains any one of the above-mentioned criteria. *RBC* is calculated each year for each standard because standards change over time as detail, exceptions, and implementation guidance are added. The RBC score reported is the median RBC score over our sample time period (1996–2006). The table is sorted by RBC score.

Panel C: Differences in Average RBC Scores

	RBC of Alleged Violations		RBC of Significant Accounting Policies		Difference	
n	Mean	Median	Mean	Median	Mean	Median
163	0.80	0.87	1.73	1.73	0.93*** (−16.90)	0.86*** (−13.00)

*, **, *** Indicate significant differences across groups at 0.10, 0.05, and 0.01 levels, respectively (two-tailed).
Panel C presents a test of H1, the protection theory. In Panel C, we test differences in means (medians) across groups and present the t-statistic (z-statistic) below the Difference column. We were unable to obtain a 10-K for one non-restatement lawsuit firm and thus the tests performed in Panel C are performed with a sample of 163 firms.

We conduct two additional tests to address this concern. First, we calculate the proportion of all non-restatement lawsuits that allege a violation of at least one significant accounting policy. We find that this proportion is 75 percent (untabulated). Using a random sample of 50 observations from our restatement lawsuits, we find very similar results with 72 percent of these firms admitting to violating at least one significant accounting policy (untabulated). Overall, it appears fairly common for lawsuits to involve alleged or admitted violations of significant accounting policies.

For our second test, we use a benchmark sample other than significant accounting policies to compare against the *RBC* of non-restatement lawsuit allegations. Specifically, we use the *RBC* of sued and non-sued restatements. Unlike the summary of significant accounting policies, these observations represent standards that firms admit to violating. In untabulated tests, we find that mean and median RBC scores for sued and non-sued restatements are significantly higher than the RBC score of allegations in our non-restatement lawsuit sample. Overall, this evidence suggests that allegations in non-restatement lawsuits are relatively principles-based, regardless of the benchmark used for comparison, consistent with H1 and the "safe harbor" element of the protection theory.

Test of H2

To test H2, concerning the effect of rules-based accounting standards on the filing of lawsuits when a GAAP violation (restatement) occurs, we estimate the following logistic regression:

$$Prob(Sued = 1) = F\Big(\alpha + \beta_1 RBC + \beta'(Controls)\Big). \qquad (1)$$

Sued is a dichotomous variable that is set to 1 if a lawsuit is filed, and is set to 0 otherwise. *F* is the cumulative distribution function of the logistic distribution, and *RBC* is as defined above for the accounting standard that was violated. For restatements not resulting in a lawsuit, we manually review the restatement footnote from the applicable 10-K to identify the accounting standards involved. As multiple standards are usually violated, the average *RBC* of all standards violated is used for both filed suits and non-suits. The coefficient estimate on *RBC*, β_1, provides evidence on H2. A negative β_1 coefficient estimate would suggest that firms violating rules-based standards are less likely to be sued than firms violating principles-based standards, consistent with the "innocent misstatement" aspect of protection theory. A positive β_1 coefficient estimate would suggest the opposite and be consistent with the roadmap theory.

Unlike our test of H1, we are unable to use each firm as its own control to test H2 and must utilize a sample of sued and non-sued firms. To control for differences between sued and non-sued firms, we include firm size, profitability, the presence of a Big 4 or predecessor auditor, stock returns around announcement of the restatement, shareholder damages, insider selling activity, stock issuances, share turnover, stock price volatility, and indicator variables for high litigation risk industries and whether revenue was allegedly misstated.[17] Appendix B defines these control variables that prior research finds to be related to litigation incidence (e.g., Palmrose and Scholz 2004; Francis et al. 1994; Shu 2000).

Descriptive Statistics

Panel A of Table 3 contains descriptive statistics for our sample involving restatements. The first set of columns provides descriptive statistics for restatement firms with a lawsuit, and the second set of columns provides descriptive statistics for restatement firms without a lawsuit. The difference in the mean RBC score between the lawsuit (1.25) and no lawsuit (2.63) samples is stark and significant at the 1 percent level. Restatement firms without lawsuits are larger and more profitable. Not surprisingly, these firms also have less severe announcement returns, smaller damages, fewer revenue restatements, lower share turnover, and less volatile returns.

Panel B of Table 3 tabulates the 15 most commonly violated accounting standards in restatements with lawsuits along with the 15 most commonly violated standards in restatements without lawsuits. The most commonly cited standards in restatements with lawsuits include APB 28 (interim reporting, *RBC* = 0), SFAS 5 (contingencies, *RBC* = 1), SFAC 5-6 (revenue, *RBC* = 1), SFAS 48 (right of return, *RBC* = 1), and SAB 101 (revenue, *RBC* = 2).[18]

The most common standards in restatements without lawsuits include SFAS 109 (taxes, *RBC* = 4), SAB 101 (revenue, *RBC* = 2), SFAC 5-6 (revenue, *RBC* = 1), SFAS 133 (derivatives, *RBC* = 3), SFAS 13 (leases, *RBC* = 4), and SFAS 5 (contingencies, *RBC* =1). Consistent with Panel A, visual comparison of the RBC scores in Panel B suggests that standards violated in restatements with lawsuits are less rules-based on average than standards violated in restatements without lawsuits.

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[17] We also re-ran our analysis utilizing abnormal (market-adjusted) returns. Our inferences remain unchanged using this alternative specification.

[18] As seen in Tables 2 and 3, plaintiffs commonly allege violations of APB 20 (Correction of Errors) and APB 28 (Interim Financial Reporting). Further review indicates that plaintiffs often allege these violations whenever the firm restates earnings or when the plaintiff alleges a GAAP violation. In effect, the plaintiff often alleges the firm restated and thus violated GAAP, citing APB 20, and did not report its interim financial statements in accordance with GAAP, citing APB 28. To the extent that these violations are boilerplate and without substance, including them in our analysis could bias our results, especially given that their RBC score is relatively low. Therefore, we conducted a robustness test in which we excluded APB 20 and APB 28 in calculating the average RBC score of each case. After excluding these standards, our results remain statistically significant at the 0.01 level and also remain economically significant.

TABLE 3

Tests Using Restatement Sample

Panel A: Descriptive Statistics

	Lawsuits Filed				Lawsuits Not Filed			
Variable	**n**	**Mean**	**Median**	**Std. Dev.**	**n**	**Mean**	**Median**	**Std. Dev.**
RBC	189	1.25	1.14	0.67	84	2.63***	3.00***	1.23
SIZE	189	6.77	6.72	1.84	84	7.72***	7.52***	1.46
ROA	189	−4.64	−0.53	13.29	84	0.53***	0.51***	3.38
BIG4	189	0.82	1.00	0.39	84	0.88	1.00	0.33
ANN_RET (%)	189	−21.92	−18.25	20.23	84	−9.18***	−6.43***	9.86
MAX_DAMAGE ($MM)	189	6,422	929	17,465	84	3,037	390**	12,293
LN_MAX_DAMAGE	189	6.95	6.83	1.94	84	6.18***	5.97**	1.71
LIT_RISK	189	0.44	0.00	0.50	84	0.32*	0.00*	0.47
INSIDER	189	−0.13	−0.01	0.26	84	−0.11	−0.04*	0.25
ISSUE	189	0.24	0.00	0.43	84	0.25	0.00	0.44
REVENUE	189	0.74	1.00	0.44	84	0.27***	0.00***	0.45
SHARE_TURN	189	1.28	0.96	1.04	84	0.85***	0.57**	0.66
VOLATILITY	189	4.70	4.29	2.08	84	2.96***	2.70***	1.35
DISMISS	189	0.26	0.00	0.44				
SETTLEMENT ($MM)	137	77	13	190				
SETTLEMENT/ MAX_DAMAGE	137	0.03	0.02	0.04				
SETTLEMENT/MVE	137	0.04	0.02	0.06				

*, **, *** Indicate significant differences across groups at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).
Panel A displays descriptive statistics for lawsuits involving restatements and potential lawsuits involving a restatement that were not filed. We obtain lawsuit data from the RiskMetrics Securities Class Action Services database, complaints from the Stanford Securities Class Action Clearinghouse, and restatements without lawsuits (potential suits not filed) from the 1997–2006 GA0 sample. We do note, however, that the Riskmetrics data do not contain data on the dollar magnitude of the settlement for two restatement lawsuits. Thus, we have data on the magnitude of the settlement for 137 of the 139 restatement lawsuits that were settled. We performed t-tests of differences across groups. Variables are defined in Appendix B.

Panel B: Most Common Standards Violated

	Restatements with Lawsuits			Restatements without Lawsuits	
Standard	**Description**	***RBC***	**Standard**	**Description**	***RBC***
APB 10	Revenue Recognition/Taxes	0	ARB 43-4	Inventory Pricing	0
APB 22	Disclosure of Accounting Policies	0	SFAC 5-6	Revenue Recognition	1
APB 28	Interim Financial Reporting	0	SFAS 5	Contingent Liabilities	1
ARB 43-4	Inventory Pricing	0	SFAS 95	Cash Flow Statement	0
ARB 43-1a	Revenue Recognition	1	SAB 101	Revenue Recognition	2
EITF 94-03	Restructuring Charges	1	SOP 97-2	Revenue Recognition (Software)	2
SFAC 5-6	Revenue Recognition	1	APB 18	Equity Investments	3
SFAS 121	Long-Lived Asset Impairment	1	ARB 51	Consolidation	3
SFAS 48	Revenue Recognition (Right of Return)	1	SFAS 133	Derivatives	3
SFAS 5	Contingent Liabilities	1	SFAS 141	Business Combinations	3
SFAS 86	Cost of Computer Software	1	SFAS 142	Intangible Assets	3

(continued on next page)

TABLE 3 (continued)

Restatement with Lawsuits			Restatements without Lawsuits		
Standard	**Description**	***RBC***	**Standard**	**Description**	***RBC***
APB 20	Correction of Errors and Changes in Estimates	2	APB 16	Business Combinations	4
SAB 101	Revenue Recognition	2	SFAS 109	Taxes	4
SOP 97-2	Revenue Recognition (Software)	2	SFAS 123	Stock Option Compensation	4
APB 16	Business Combinations	4	SFAS 13	Leases	4

Panel B reports the 15 most common standard violations or alleged violations obtained from filed lawsuits and potential lawsuits involving restatements. Our sample of lawsuits involving a restatement was obtained from the Securities Class Action Services database. Our sample of restatements that could have potentially resulted in a suit was obtained from the 1997–2006 GAO sample and includes instances where the firm's restatement announcement caused the firm's market value to drop by at least $50 million dollars. The RBC score of these standards is also presented. The RBC score measures whether a standard contains bright-line thresholds, scope and legacy exceptions, large volumes of implementation guidance, and a high level of detail. *RBC* increases by 1 when a standard contains any one of the above-mentioned criteria. *RBC* is calculated each year for each standard because standards change over time as detail, exceptions, and implementation guidance are added. The RBC score reported is the median RBC score over 1996–2006, the sample's time period. The table is sorted by RBC score.

Panel C: Logistic Regression Results

$$Prob(Sued = 1) = F\Big(\alpha + \beta_1 RBC + \beta'(Controls)\Big). \quad (1)$$

	All Lawsuits			Settled Lawsuits			Dismissed Lawsuits		
Variable	**Coeff.**	**t-statistic**	**Marginal Effect**	**Coeff.**	**t-statistic**	**Marginal Effect**	**Coeff.**	**t-statistic**	**Marginal Effect**
Intercept	2.39	(1.47)		1.29	(0.72)		2.34	(1.04)	
RBC	−1.32	(−5.73)***	−0.121	−1.44	(−5.08)***	−0.123	−1.03	(−3.95)***	−0.134
SIZE	−0.83	(−3.29)***	−0.076	−0.92	(−3.13)**	−0.079	−0.85	(−2.53)**	−0.111
ROA	−0.07	(−1.23)	−0.007	−0.09	(−1.26)	−0.008	−0.05	(−0.79)	−0.007
BIG4	−0.54	(−0.88)	−0.050	−0.74	(−1.03)	−0.063	−0.03	(−0.04)	−0.004
ANN_RET	−0.02	(−1.69)*	−0.002	−0.03	(−1.81)*	−0.002	−0.02	(−1.00)	−0.003
LN_MAX_ DAMAGE	0.98	(4.69)***	0.089	1.17	(4.64)***	0.100	0.83	(3.10)***	0.108
LIT_RISK	−0.25	(−0.53)	−0.023	−0.28	(−0.54)	−0.024	−0.32	(−0.58)	−0.042
INSIDER_SALE	−0.51	(−0.62)	−0.047	−0.99	(−1.04)	−0.084	−0.06	(−0.06)	−0.008
ISSUE	−0.40	(−0.82)	−0.036	−0.30	(−0.52)	−0.025	−0.41	(−0.67)	−0.053
REVENUE	0.94	(2.23)**	0.086	1.18	(2.45)**	0.101	0.72	(1.40)	0.094
SHARE_TURN	0.32	(0.96)	0.029	0.07	(0.20)	0.006	0.61	(1.51)	0.080
VOLATILITY	−0.08	(−0.40)	−0.008	0.00	(0.02)	0.000	−0.31	(−1.12)	−0.040
Pseudo-R^2	47.1%			53.5%			40.2%		
n	273			223			134		

*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

Panel C reports the results of a logistic regression investigating the impact of rules-based characteristics on the decision to file a lawsuit when the firm admits to violating GAAP (restatement lawsuit). In Equation (1), *F* is the cumulative distribution function of the logistic distribution. We utilize the *RBC* measure from Mergenthaler (2010), which increases by 1 when a standard contains any one of the four following criteria: (1) bright-line thresholds, (2) scope and legacy exceptions, (3) large volumes of implementation guidance, and (4) a high level of detail. Our sample of 189 firms with a restatement where a lawsuit was filed between 1996 and 2005 was obtained from the Securities Class Action Services database. Our sample of 84 restatements where a lawsuit was not filed was obtained from the 1997–2006 GAO sample

(continued on next page)

TABLE 3 (continued)

and includes instances where the firm's restatement announcement caused the firm's market value to drop by at least \$50 million dollars. The "All Lawsuits" sample includes 189 restatements with lawsuits and 84 restatements without lawsuits, for 273 total observations. The "Settled Lawsuits" sample includes 139 restatements where the lawsuit was settled (out of the 189 total restatements with lawsuits) and the entire sample of 84 restatements without lawsuits, for 223 total observations. The "Dismissed Lawsuits" sample includes 50 restatements where the lawsuit was dismissed (the remainder of the 189 total restatements with lawsuits) and the entire sample of 84 restatements without lawsuits, for 134 total observations. t-statistics are in parentheses. All variables that lie below (above) the 1st (99th) percentiles are winsorized. The marginal effects are calculated for each observation in our sample and then averaged over all the observations (Greene 2003).

Lawsuit Filing Regression

Panel C of Table 3 reports results from estimating Equation (1). The first set of columns presents results for the combined sample of all 189 restatements with lawsuits and 84 restatements without lawsuits. *RBC* is strongly negatively related to the probability of filing a lawsuit (t-statistic $= -5.73$, $p < 0.01$), consistent with the innocent misstatement defense of the protection theory in H2. The marginal effects tabulated in Panel C suggest that a one-unit increase in *RBC* decreases the likelihood of litigation by 12.1 percentage points. This effect is both statistically significant and economically meaningful. For example, the likelihood of being sued decreases, on average, by approximately 12 percentage points when a company violates SFAS 52, which has an *RBC* of 1, versus violating SFAS 5, which has an RBC score of 0. Overall, results for the restatement sample provide support for the protection theory, suggesting rules-based standards decrease litigation risk by providing an "innocent misstatement" defense for restatements.

Robustness Tests for H2

Presence of Fraud versus Errors

We next consider whether differences in the underlying nature of the violation between our treatment and control samples drive the results in the first set of columns of Panel C in Table 3. For example, our treatment sample of restatement firms with lawsuits could consist primarily of clear-cut cases of fraud, while our control sample could primarily represent obvious errors unlikely to trigger lawsuits. To address this concern, we perform two analyses that involve modifying our sample to assess whether differences in the nature of the violation drive our results.

First, we divide the 189 restatements with lawsuits into 139 settled lawsuits and 50 dismissed lawsuits and re-estimate Equation (1) after retaining all 84 control observations for both of the subsamples. Settled suits represent cases where the court found a strong inference of fraud, while dismissed suits represent cases in which the court did not find a strong inference of fraud. Thus, the settled cases are more credible accounting fraud cases *ex post*, while the dismissed cases are more likely to include errors. We include both sets of results for comparison. The second and third set of columns of Panel C in Table 3 present the results. The strong negative relation between *RBC* and the filing of a lawsuit remains in both subsamples, with similar estimated economic magnitudes of -12.3 percent for settled cases in the "Settled Lawsuits" column and -13.4 percent fordismissed cases in the "Dismissed Lawsuits" column, with $p < 0.01$ in each case. Thus, the negative loading on *RBC* remains even when we partition our sample to separate more clear-cut cases of accounting fraud from cases that may represent errors. Specifically, the comparison of restatements without lawsuits to dismissed cases indicates that a clear separation between fraud and errors does not drive our results.

Second, in Table 4 we report results from a sample of 131 accounting irregularity firms from Mergenthaler (2010), all of which were investigated by the SEC for questionable accounting

TABLE 4

Robustness Test for Filing Decision Using SEC Investigation Sample

$$Prob(Sued = 1) = F\left(\alpha + \beta_1 RBC + \beta'(Controls)\right). \quad (1)$$

Variable	Coeff.	t-statistic	Marginal Effect
Intercept	−0.11	(−0.07)	
RBC	−0.87	(−3.18)***	−0.129
SIZE	0.05	(0.37)	0.007
ROA	0.00	(0.13)	0.000
BIG4	0.57	(0.77)	0.084
ANN_RET	−0.04	(−2.05)**	−0.006
LN_MAX_DAMAGE	0.22	(2.13)**	0.032
LIT_RISK	−0.97	(−1.80)*	−0.143
INSIDER_SALE	1.07	(0.95)	0.158
ISSUE	−0.07	(−0.13)	−0.010
REVENUE	0.13	(0.23)	0.020
SHARE_TURN	0.81	(2.37)**	0.119
VOLATILITY	0.06	(0.37)	0.009
Pseudo-R^2	32.6%		
n	131		

*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

This table reports the results of analyses that examine whether our main results from Equation (1) are robust to an alternate specification. In this analysis we use a sample of restatements that were investigated by the SEC from Mergenthaler (2010). This classification scheme is similar to that employed by Hennes et al. (2008) to identify accounting irregularities; thus, this analysis looks at whether rules-based standards impact the likelihood of litigation when the analysis is confined to accounting irregularities. t-statistics are in parentheses. All variables that lie below (above) the 1st (99th) percentiles are winsorized. Variables are defined in Appendix B.

practices. Some of these firms were named in a class action lawsuit and some were not. Given that the SEC investigated these firms, the concern that non-sued observations represent obvious accounting errors is mitigated.[19] The relation between *RBC* and the filing decision is still significantly negative in this specification ($p < 0.01$).

Taken together, the results in Tables 3 and 4 suggest that differences in the nature of the violation in terms of frauds versus errors between the control and treatment samples do not drive our findings. Instead, it appears that plaintiffs tend to avoid filing lawsuits for violations of rules-based standards even when a potential argument for fraudulent intent exists. The key is that plaintiffs must be able to persuade the court that a *strong* inference of fraud exists, which is likely difficult with violations of rules-based standards due to the innocent misstatement defense.

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[19] We do not utilize this sample as our primary sample due to the conflicting incentives of the SEC and plaintiffs' lawyers. As we discussed in Section II, plaintiffs' lawyers are motivated by potential settlement size (Coffee 2006), which is why we screen on potential damages in our primary analysis.

Transaction Complexity and Standard Age

An additional concern with our findings is that perhaps the *RBC* measure picks up factors other than rules-based characteristics of accounting standards. For example, it is possible that the complexity of the underlying transactions, not just rules-based accounting standards, contributes to our results. As a practical matter, accounting standards tend to be more rules-based when transactions are more complex (Nelson 2003). In addition, when inferring intent, courts appear to consider both the complexity of the accounting standards (e.g., *In re Microstrategy, Inc. Securities Litigation*, 115 F. Supp. 620, 652 (E.D. Va. 2000)) and the complexity of the underlying transaction (e.g., *In re Bristol-Myers Squibb Securities Litigation*, 312 F. Supp. 2d 549, S.D.N.Y. (2004)). The negative relation between *RBC* and lawsuit filings in Table 3 could therefore be due to either the complexity of rules-based standards, the complexity of the underlying transactions associated with rules-based standards, or both.

We explore this issue by developing multiple proxies for transaction complexity. Our first proxy for transaction complexity involves searching each standard for three root words: (1) complex*, (2) complic*, and (3) elaborat*. If any of these words are found in the standard, then we read the surrounding paragraph to determine if the standard mentions the underlying transaction as complex. We then create an indicator variable that is equal to 1 if the standard refers to the underlying transaction as complex, and is equal to 0 otherwise. Untabulated analysis indicates that this indicator variable categorizes 35 percent of the underlying transactions in our sample as complex. For example, the following standards involve complex transactions using this proxy: (1) SFAS 133 (derivatives), (2) APB 14 (convertible debt), (3) SFAS 13 (leases), (4) SFAS 109 (taxes), and (5) SFAS 87 (pensions). Finally, since many of the 273 restatements in our sample involve more than one standard (and thus more than one underlying transaction), we calculate the mean of this indicator variable for each of the 273 restatements. This procedure yields our first complexity measure, which we label *COMPLEXITY_1*.

Our second proxy for transaction complexity (*COMPLEXITY_2*) is the mean of the number of words contained in the definition of the underlying transactions involved in each restatement. We obtain definitions from the glossary of the FASB Codification. For example, for SFAS 13, the measure counts the number of words needed to define a lease under the assumption that it will take more words to describe complex transactions. We utilize the log of this measure to mitigate skewness.[20]

Table 5 presents our analyses designed to isolate the effects of transaction complexity from *RBC*. In Column (1) of Table 5, we include *COMPLEXITY_1* as a covariate in our lawsuit filing analysis. After including this variable, the *RBC* coefficient remains negative and significant at the 0.01 level (t-statistic = −4.17). Interestingly, the *COMPLEXITY_1* coefficient is also negative and significant, suggesting that both transaction complexity and the rules-based nature of the standard affect the decision to file a class action lawsuit. Column (2) of Table 5 reports the results using our second measure of complexity (*COMPLEXITY_2*) as a covariate, and we find that *RBC* remains significantly negative at the 0.01 level (t-statistic = −5.71) while *COMPLEXITY_2* is also significant at the 0.01 level. Consistent with Column (1), these results suggest that both *RBC* and transaction complexity affect the filing decision, presumably because both complex rules and complex transactions make it more difficult to infer fraudulent intent.[21]

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[20] Thirteen of the 273 restatements in our sample involve no underlying transaction and deal exclusively with accounting issues (e.g., how to calculate EPS or report changes in accounting policies). For these observations, we assign them the lowest transaction complexity score obtained from all other restatements that have an underlying transaction. We do this because these restatements are not likely impacted by transaction complexity. We also re-ran our analysis after dropping these observations. Our results are unchanged using this alternative specification.

To assess the economic significance of *RBC* relative to our measures of transaction complexity, we calculated standardized regression coefficients as well as marginal effects for a one standard deviation change in either *RBC* or our two transaction complexity measures. In untabulated results, we found that a one standard deviation increase in *RBC* has roughly a 147 percent (25 percent) bigger reduction in the probability of being sued compared to our first (second) complexity measure. Thus, it appears *RBC* has a larger economic effect on the probability of being sued compared to transaction complexity. However, we caution that perfectly separating the complexity of rules-based standards from the complexity of underlying transactions is difficult to achieve from an empirical standpoint. Nevertheless, the tests above do indicate that our results do not appear to be driven exclusively or even predominantly by transaction complexity.

Finally, in the last column of Table 5, we include the average age of the standards violated as an additional covariate. In general, there has been an increase in *RBC* across all standards over time (untabulated). At the same time, newer standards, for example for derivatives, stock options, pensions, etc., tend to involve more complex transactions and to have higher RBC scores. As such, the significance of *RBC* could be related to standard age, but the results in Column (3) of Table 5 do not support this conjecture. Although the results suggest that violations of older standards are more likely to trigger lawsuits, *RBC* remains significantly negatively related to lawsuit incidence ($t = -4.79$).

Other News and Revenue

We perform two additional untabulated robustness checks for our restatement sample. First, we exclude observations involving earnings announcements within three days of the restatement announcement to avoid confounding our filing model with other news. Excluding these observations does not change the economic or statistical significance of our results. Second, we also found significantly negative coefficients on *RBC* both among restatements involving revenue and restatements not involving revenue. This finding suggests that the relation between *RBC* and the probability of litigation is significant in both revenue and non-revenue cases.

Test of H3

To test the effect of rules-based standards on litigation outcomes, we first estimate the following logistic regression:

$$Prob(Meritorious = 1) = F\Big(\alpha + \beta_1 RBC + \beta'(Controls)\Big). \tag{2}$$

Meritorious is a dichotomous variable coded 1 if the lawsuit reaches a meritorious outcome as defined below. We again include firm size and profitability as control variables since larger, more profitable firms may have access to better legal counsel and may be more successful in preventing a meritorious outcome. As discussed above, the outcome of the suit hinges on the perceived strength of the case and ability to prove scienter. Thus, we include variables based on plaintiffs' complaints

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[21] We also re-ran our primary analysis excluding SFAS 13 and SFAS 133 violations to ensure that they do not drive our results. These standards are generally viewed as extremely rules-based standards and involve complex underlying transactions. This leads to a common perception that these standards often result in unintentional errors in their application. Untabulated results are very similar when these observations are excluded. We also found that violations of SFAS 13 (leases) and SFAS 109 (taxes) are clustered in our sample during the years of 2003–05. The clustering appears to be related to regulatory enforcement by the SEC (SFAS 13) and new internal control assurances provided as a result of SOX (SFAS 109). Our inferences are also unchanged when this pair of standards is omitted from our analyses.

TABLE 5

Robustness Tests Using Restatement Sample Transaction Complexity and Standard Age

$$Prob(Sued = 1) = F\Big(\alpha + \beta_1 RBC + \beta'(Controls)\Big). \quad (1)$$

	(1)		(2)		(3)	
Variable	**Coeff.**	**t-statistic**	**Coeff.**	**t-statistic**	**Coeff.**	**t-statistic**
Intercept	2.44	(1.48)	9.42	(3.52)***	0.44	(0.24)
RBC	−1.09	(−4.17)***	−1.43	(−5.71)***	−1.13	(−4.79)***
COMPLEXITY_1	−1.34	(−1.95)*				
COMPLEXITY_2			−2.38	(−3.96)***		
AGE					0.06	(2.47)**
SIZE	−0.83	(−3.25)***	−0.67	(−2.40)**	−0.77	(−3.02)***
ROA	−0.08	(−1.24)	−0.06	(−0.87)	−0.06	(−0.95)
BIG4	−0.66	(−1.04)	−0.78	(−1.20)	−0.64	(−1.02)
ANN_RET	−0.02	(−1.65)*	−0.03	(−2.03)**	−0.02	(−1.49)
LN_MAX_DAMAGE	0.98	(4.61)***	0.91	(3.92)***	0.95	(4.49)***
LIT_RISK	−0.39	(−0.81)	−0.04	(−0.08)	−0.20	(−0.40)
INSIDER_SALE	−0.57	(−0.71)	−1.14	(−1.26)	−0.51	(−0.59)
ISSUE	−0.27	(−0.55)	−0.48	(−0.88)	−0.54	(−1.12)
REVENUE	0.85	(2.00)**	1.51	(3.20)***	1.33	(2.86)***
SHARE_TURN	0.26	(0.80)	0.08	(0.23)	0.22	(0.64)
VOLATILITY	−0.04	(−0.17)	0.01	(0.06)	0.00	(−0.02)
Pseudo-R^2	47.8%		51.6%		48.3%	
n	273		273		273	

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*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

This table reports the results of analyses that examine whether transaction complexity or the age of the standard drive our main results from Equation (1). In Column (1), we include our first complexity measure, *COMPLEXITY_1*, as an additional covariate. *COMPLEXITY_1* is a dummy variable that is equal to 1 when the transaction is described as complex, and is equal to 0 otherwise. We identify transactions described as complex by reading the paragraphs in the standard that contain the following root words or their derivatives: (1) complex*, (2) complic*, or (3) elabor*. In Column (2), we include an alternative proxy for complexity, *COMPLEXITY_2*, which is defined as the natural log of the length of the definition used to describe the underlying transaction that the standard pertains to. If the standard has no underlying transaction (e.g., FAS 128, which outlines how to calculate EPS), then we input the lowest transaction complexity score recorded for all other standards that had an underlying transaction. We do this because standards without an underlying transaction are not impacted by transaction complexity and their complexity score should thus be low because there really is no impact from transaction complexity. Finally, Column (3) includes the age of the standard as an additional covariate. All variables that lie below (above) the 1st (99th) percentiles are winsorized. Variables are defined in Appendix B.

that are plausibly positively related to the ability to prove scienter, including whether: (1) the firm's auditor was named as a defendant, (2) insider trading was alleged, (3) revenue manipulation was alleged, (4) an external whistleblower, such as the press, is mentioned in uncovering the alleged fraud, (5) an internal (i.e., board of directors) or external (i.e., SEC) investigation has taken place, (6) top executives have resigned during or after the class period, and (7) the auditor has resigned. We also include announcement stock returns and the maximum potential damage as a proxy for the extent to which fraud can be inferred. Finally, we include indicator variables for high litigation risk industries and whether the case alleges a violation of Section 11 of the 1933 Securities Act.

TABLE 6

Tests for the Effect of the Rules-Based Characteristics on the Lawsuit Outcomes

	(1) Survives Dismissal Decision		(2) Settlement of at Least 0.5% of MVE		(3) Settlement Amount (among Meritorious Settlements)	
Variable	**Coeff.**	**t-statistic**	**Coeff.**	**t-statistic**	**Coeff.**	**t-statistic**
Intercept	−0.35	(−0.34)	−0.87	(−0.90)	0.07	(3.79)***
RBC	−0.18	(−0.69)	0.29	(1.10)	0.01	(0.99)
SIZE	−0.17	(−0.91)	−0.51	(−2.65)***	−0.01	(−2.54)***
ROA	0.00	(−0.10)	−0.01	(−0.57)	0.00	(1.20)
ANN_RET	−0.01	(−1.27)	−0.02	(−2.40)**	0.00	(0.24)
LN_MAX_DAMAGE	0.21	(1.19)	0.36	(2.05)**		
AUDITOR_NAMED	1.31	(2.21)**	0.79	(1.70)*	0.02	(2.22)**
INSIDER_SALE	−0.13	(−0.33)	0.32	(0.89)	0.00	(−0.03)
LIT_RISK	0.80	(2.03)**	0.74	(2.04)**	0.00	(0.03)
REVENUE	0.27	(0.64)	0.38	(0.91)	−0.02	(−1.84)*
WHISTLE	1.56	(1.95)*	1.15	(1.94)*	0.01	(0.98)
INVESTIGATION_INTERN	0.22	(0.53)	0.20	(0.52)	0.01	(0.80)
INVESTIGATION_EXTERN	0.10	(0.21)	0.29	(0.65)	0.00	(−0.17)
EXEC_TURNOVER	−0.14	(−0.32)	0.10	(0.25)	0.00	(0.33)
AUDITOR_RESIGN	1.38	(2.25)**	0.70	(1.42)	0.01	(0.73)
SEC_11	0.02	(0.03)	0.18	(0.41)	0.01	(1.08)
R^2	15.5%		22.4%		25.8%	
n	189		189		116	

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*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

This table reports the results of examining the impact of rules-based characteristics on lawsuit outcomes. Columns (1) and (2) report the results of a logistic regression exploring the impact of rules-based characteristics on two proxies for meritorious outcomes, while Column (3) reports the results of an OLS regression examining the impact of rules-based characteristics on the magnitude of settlement amounts. Column (1) utilizes the dismissal decision as a proxy for a meritorious outcome. A suit that survives the dismissal decision is equal to 1, while a suit that is dismissed is equal to 0. Column (2) follows Johnson et al. (2007) and classifies a suit as meritorious if the settlement is at least 0.5 percent of the firm's market value of equity. Finally, Column (3) follows the Johnson et al. (2007) methodology to identify meritorious cases. Then, among the meritorious cases, it reports the results that examine the impact of rules-based characteristics on settlement amounts. Our sample of 189 suits (116 of which are termed meritorious according to the Johnson et al. [2007] methodology) was obtained from the Securities Class Action Services database. t-statistics are in parentheses. All variables that lie below (above) the 1st (99th) percentiles are winsorized. Variables are defined in Appendix B.

Table 6 contains estimates from Equation (2), using two proxies of meritorious suit outcomes. Column (1) uses the dismissal decision as a proxy, with *Meritorious* coded as 1 if the suit survives the dismissal decision, and 0 otherwise. In Column (2), we utilize the Johnson et al. (2007) measure for meritorious lawsuit outcomes, coding *Meritorious* as 1 if the settlement is greater than 0.5 percent of the firm's market value of equity, and 0 otherwise. Johnson et al. (2007) use this measure because certain cases that survive dismissal may be non-meritorious, even though the allegations were sufficient to survive a motion to dismiss. In these cases, the company may settle to avoid further litigation costs.

Inconsistent with H3, we find no significant relation between *RBC* and meritorious suit outcomes using either proxy. The insignificance of *RBC* and most other variables in Equation (2) is perhaps not surprising. Prior studies generally find that few variables help predict suit outcomes (see, Johnson et al. 2007; Pritchard and Sale 2005). The fact that many aspects of this process are unobservable or difficult to measure likely contributes to the inability of the model to explain the dismissal decision with much precision. In addition, as we discuss above, variables correlated with the strength of the case, including *RBC*, are likely to be anticipated by plaintiffs' attorneys, leading to a screening process at the filing stage. Thus, variables such as *RBC* may have little ability to predict lawsuit outcomes once weaker cases have been screened out at the filing stage.

As a final test of H3, we perform an OLS regression of settlement amounts on the covariates in Equation (2). Settlements are scaled by maximum damages during the class period and we exclude non-meritorious settlements following the Johnson et al. (2007) definition above. Results in Column (3) of Table 6 show that the coefficient on *RBC* is positive, but not significant at conventional levels ($t = 0.99$). Consistent with earlier tests, we find no significant relation between *RBC* and lawsuit outcomes.

V. LIMITATIONS

Three potential limitations merit discussion in the context of our findings. The first involves the possibility that *ex ante* litigation risk endogenously affects the rules-based characteristics of accounting standards as preparers and auditors demand more guidance in areas where litigation risk is higher. We do not attempt to model simultaneity in our data, primarily because measuring *ex ante* litigation risk across roughly 80 years of U.S. GAAP standards using only post-1996 litigation data seems infeasible. One would expect this source of endogeneity to bias the results in favor of finding a positive relation between litigation risk and rules-based accounting standards. Because we find the opposite, it does not appear that the *ex ante* effect of litigation risk on accounting standards is a significant concern for inferences in our sample.

The second limitation relates to the unobservability of key financial reporting decisions. For example, it is possible that the rules-based nature of accounting standards affects the decision to restate. Rules-based misstatements may be more easily detectable by auditors or regulators, and rules-based standards provide clear guidance that could force a firm to restate when it is outside that guidance. Both of these factors could lead to more restatements involving rules-based standards. If this is the case, then principles-based standards could help firms avoid litigation because many potential principles-based violations would be unobservable. The universe of observable misstatements (i.e., restatements) might therefore be biased toward firms with rules-based misstatements. However, the theory most relevant to this concern, the roadmap theory, predicts these rules-based misstatements should be the most likely to trigger litigation. As we find just the opposite, it does not appear this concern affects inferences in our study. Put another way, even if rules-based standards are more likely to trigger restatements, we find no evidence that these rules-based restatements increase the threat of litigation.

Other forms of unobservability are not as innocuous for our inferences. If firms choose to engage in fraudulent accounting more often in relatively principles-based areas of GAAP, then this effect could contribute to our findings. However, since the underlying incidence of fraud or earnings management is unobservable, this concern is a limitation of any archival study of litigation or earnings manipulation.[22]

The final limitation is that we do not consider all of the potential effects of a shift to a principles-based accounting system. Standard-setters are likely primarily concerned with the usefulness of information produced by the accounting system, and we do not address this issue. However, litigation is an important potential "side effect" in such a shift, particularly in the U.S. given its highly litigious nature (Schipper 2003; SEC 2003). Our consideration of litigation does not include all potential costs of a switch to principles-based standards and, thus, we cannot make a recommendation regarding whether such a switch is likely to be beneficial overall. Rather, we only provide evidence on the likely effect of rules-based standards on litigation incidence and outcomes.

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[22] Mergenthaler (2010) provides some evidence in this area, finding that the dollar magnitude of accounting violations is larger when rules-based standards are involved. As such, this should bias against our finding that principles-based standards are more likely to lead to securities litigation.

VI. CONCLUSION AND DISCUSSION

This study provides the first systematic empirical evidence regarding the association between rules-based accounting standards and securities litigation. Our evidence is important because the U.S. Congress issued a mandate in SOX requiring the SEC to conduct a study of the cost of implementing more principles-based standards in the U.S., and the SEC (2003) specifically noted in the study that such a shift could affect the incidence of litigation. More recently, the SEC (2010) issued a proposal that would require public firms in the U.S. to adopt the more principles-based IFRS.

We utilize two samples of sued firms to test the effect of principles-based standards on the incidence of litigation. First, we compare the standards cited by plaintiffs in cases that do not involve restatements to the standards identified in each sued firm's significant accounting policies. In such cases, the protection theory predicts that plaintiffs will tend to allege standards that are more principles-based due to the safe harbor protection provided by rules-based standards when firms follow the rules, as well as plaintiffs' ability to "second-guess" accounting decisions under principles-based standards. We find evidence consistent with the prediction of the protection theory, that rules-based standards provide a "safe harbor" and are thus not often cited in lawsuits.

Second, using a sample of restatements, we find that violations of rules-based standards are associated with a lower probability of litigation. This finding is inconsistent with assertions that violations of rules-based standards provide plaintiffs with a "roadmap" to successful suits. Rather, it is consistent with the protection theory in that complex, rules-based standards provide firms with an "innocent misstatement" defense. This relation remains economically and statistically significant after controlling for transaction complexity and is robust to a variety of alternative specifications.

In summary, our evidence suggests that rules-based accounting standards reduce the threat of litigation. However, the overall effects of a shift to a more principles-based accounting system are difficult to predict because "the U.S. litigation system and accounting practices [would] transition to a new equilibrium" and numerous factors would change simultaneously (Hail et al. 2010, 377). Thus, while we are leery of definitively predicting a shift in the equilibrium level of securities litigation should the U.S. shift to a more principles-based accounting system, our findings do offer insight for policymakers.

First, our evidence is consistent with the notion that increasing the extent of principles-based accounting standards would weaken safe harbor protection and give plaintiffs a wider menu of potential "judgment-based" allegations in cases not involving an admitted misstatement. This would increase the threat of litigation for firms. Second, decreasing the extent of rules-based accounting standards might make the "innocent misstatement" defense less credible given an accounting restatement, and also increasing the threat of litigation. However, increasing the extent of principles-based accounting standards could also reduce the number of restatements in general, but this development may have little effect since we find that rules-based restatements pose a relatively low litigation threat in the current legal environment. Finally, we emphasize that the U.S. court system already has built-in protections against frivolous securities litigation through the PSLRA. Thus, any increase in the threat of litigation from a switch in accounting standards may be at least partially mitigated by these protections.

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APPENDIX A

EXAMPLE OF SUMMARY OF SIGNIFICANT ACCOUNTING POLICIES FROM 10-K AND LAWSUIT ALLEGATIONS

POLICY MANAGEMENT SYSTEMS CORP, 12/31/1999
NOTE 1. SUMMARY OF SIGNIFICANT ACCOUNTING POLICIES

ARB 51 (*RBC* = 3), APB 18 (*RBC* = 3)
BASIS OF PRESENTATION

The consolidated financial statements are prepared on the basis of generally accepted accounting principles and include the accounts of the Company and its majority owned subsidiaries (collectively, the "Company"). All material intercompany balances and transactions have been eliminated. The equity method of accounting is used when the Company does not have effective control and has a 20 percent to 50 percent interest in other companies. Under the equity method, original investments are recorded at cost and adjusted by the Company's share of undistributed earnings or losses of these companies.

SFAS 5 (*RBC* = 1)
USE OF ESTIMATES

The preparation of financial statements in conformity with generally accepted accounting principles requires management to make estimates and assumptions that affect the reported amounts of assets and liabilities and disclosure of contingent assets and liabilities at the date of the financial statements, as well as the reported amounts of revenues and expenses during the reporting period. Actual results could differ from those estimates. Financial statement line items that include significant estimates include the allowance for uncollectible receivables, accrued revenues, accrued restructuring charges, goodwill and other intangibles, net, capitalized software and costs, net, and income tax balances.

CON 5-6 (*RBC* = 1), SOP 97-2 (*RBC* = 2), ARB 45 (*RBC* = 0)
REVENUE RECOGNITION

The Company's revenues are generated primarily by licensing software systems and providing outsourcing and professional services to the global insurance and related financial services industries. All revenues are recorded in accordance with Statement of Position 97-2, *Software Revenue Recognition* (SOP 97-2), and Statement of Position 81-1, *Accounting for Performance of Construction-Type and Certain Product-Type* **Contracts (SOP 81-1). [Note: details of revenue**

recognition omitted for brevity. Also, SOP 81-1 provides interpretive guidance related to ARB 45 (percentage of completion accounting), so we use that standard to calculate *RBC*.]

CASH AND CASH EQUIVALENTS

The Company considers all highly liquid investments with an original maturity of three months or less to be cash equivalents.

SFAS 115 (*RBC* = 3), SFAS 107 (*RBC* = 1)
MARKETABLE SECURITIES

Debt securities included in the Company's investment portfolio for which there is a positive intent and ability to hold to maturity are carried at amortized cost. Debt securities that may be sold prior to maturity and all marketable equity securities are classified as available-for-sale and carried at fair value. The fair value is estimated based on quoted market prices for those or similar investments. Net unrealized gains and losses, determined on the specific identification method, on securities classified as available-for-sale are carried as a separate component of accumulated other comprehensive income.

Realized gains and losses are included in net income and the cost of securities sold is based on the specific identification method. Marketable securities were sold for cash proceeds of $2.1 and $3.3 million during 1999 and 1998, respectively. There were no sales of marketable securities during 1997.

ARB 43-9b (*RBC* = 0), ARB 43-9c (*RBC* = 0)
PROPERTY AND EQUIPMENT

Property and equipment, including support software acquired for internal use, is stated at cost less accumulated depreciation and amortization. Property and equipment is depreciated on a straight-line basis over its estimated useful life.

Gains and losses on dispositions of property and equipment are determined based on the difference between the cash plus the fair value of any assets received (in the case of nonmonetary transactions) less the net book value of the asset disposed of at the date of disposition.

APB 17 (*RBC* = 1), SFAS 121 (*RBC* = 1)
GOODWILL AND OTHER ACQUIRED INTANGIBLE ASSETS

Identifiable intangible assets and goodwill are recorded and amortized over their estimated economic lives or periods of future benefit. The lives established for these assets are a composite of many factors that are subject to change because of the nature of the Company's operations. This is particularly true for goodwill that reflects value attributable to the going-concern nature of acquired businesses, the stability of their operations, market presence, and reputation. Accordingly, the Company evaluates the continued appropriateness of these lives and recoverability of the carrying value of such assets based upon the latest available economic factors and circumstances. The Company evaluates the recoverability of all long-lived assets, including specific intangible assets and goodwill, based upon a comparison of estimated future cash flows from the related operations with the then corresponding carrying values of those assets. Impairment of value, if any, is recognized in the period in which it is determined. A rate considered to be commensurate with the risk involved is used to discount the cash flows for any recognized impairment.

The Company amortizes goodwill over an estimated life of 3 to 15 years. Other identifiable purchased intangible assets are being amortized on a straight-line basis over their estimated period of benefit ranging from 3 to 10 years.

SFAS 86 (*RBC* = 1) SOP 98-1 (*RBC* = 1)
CAPITALIZED SOFTWARE COSTS

In accordance with Statement of Financial Accounting Standards No. 86, *Accounting for the Costs of Computer Software to Be Sold, Leased or Otherwise Marketed* (SFAS 86), certain costs incurred in the internal development of computer software and costs of purchased computer software, consisting primarily of software acquired through business acquisitions, are capitalized. **[Note: details omitted for brevity.]**

The Company also capitalizes certain costs in accordance with Statement of Position 98-1, *Accounting for the Cost of Computer Software Developed or Obtained for Internal Use* (SOP 98-1). **[Note: details omitted for brevity.]**

SFAS 109 (*RBC* = 4)
INCOME TAXES

The provision for income taxes and corresponding balance sheet accounts are determined in accordance with Statement of Financial Accounting Standards No. 109, *Accounting for Income Taxes* (FAS 109). Under FAS 109, the deferred tax liabilities and assets are determined based on temporary differences between the basis of certain assets and liabilities for income tax and financial reporting purposes. These differences are primarily attributable to differences in the recognition of depreciation and amortization of property, equipment and intangible assets and certain software development costs and revenues.

SFAS 128 (*RBC* = 3)
BASIC AND DILUTED EARNINGS PER SHARE

Basic and diluted earnings per share (EPS) are calculated according to the provisions of Statement of Financial Accounting Standards No. 128, *Earnings per Share* (FAS 128). Weighted average common shares outstanding for all periods have been restated to reflect the stock split in June 1998 (see Note 11). For the Company, the numerator is the same for the calculation of both basic and diluted EPS. The denominator for basic and diluted EPS is the same for the period ended December 31, 1999 as the loss from operations would otherwise cause the inclusion of common stock options to be anti-dilutive. The following is a reconciliation of the denominator used in the EPS calculations (in thousands):

> Weighted average shares for 1999 would have included 1,174,358 common stock equivalents if the Company had recorded net income. All options to purchase shares of common stock were included in the computation of diluted EPS for 1998.

SFAS 52 (*RBC* = 2)
FOREIGN CURRENCY TRANSLATION

The local currencies of the Company's foreign subsidiaries have been determined to be their functional currencies. Assets and liabilities of foreign subsidiaries are translated into United States dollars at current exchange rates and resulting translation adjustments are included as a separate component of accumulated other comprehensive income. Revenue and expense accounts of these operations are translated at average exchange rates prevailing during the year. Transaction gains and losses, which were not material, are included in the results of operations of the period in which they occur.

The effect on the Company's operating revenues of adverse foreign currency exchange fluctuations (stated as current year international revenues translated at prior year average exchange rates) was $4.3 and $8.0 million for 1999 and 1998, respectively.

RBC of significant accounting policies: 1.59

Summary of Lawsuit

This case was triggered by a series of bad news announcements in late 1999 and early 2000. The Company announced a variety of write-downs and special charges during this time frame. Plaintiffs alleged, among other things, that the financial statements during the class period violated GAAP in that these write-offs and charges should have been recorded sooner and were deliberately deferred to increase share price. Plaintiffs also alleged that software revenue was recorded improperly. The case settled for $7.75 million after the motion to dismiss was denied by the court.

Lawsuit Allegations of GAAP Violations

SFAS 5 (contingencies) *RBC* = 1
SFAS 86 (capitalization of software costs) *RBC* = 1
APB 17 (intangible assets) *RBC* = 1
APB 28 (interim reporting) *RBC* = 0
SOP 97-2 (revenue recognition (Software)) *RBC* = 2
CON 5-6 (revenue recognition) *RBC* = 1
ARB 43 1-a (revenue recognition) *RBC* = 1
APB 10 (revenue recognition) *RBC* = 0
APB 22 (disclosure of accounting policies) *RBC* = 0

RBC of allegations = 0.78

APPENDIX B

Variable Definitions

Note: For firms that were not sued, the "class period" is defined as the time period between the earnings announcement date of the first misstated quarterly period and the restatement announcement date.

AGE = the age of the accounting standard in years, based upon initial passage;
ANN_RET = cumulative return for the seven days centered on the restatement announcement date or class period end date, expressed as a percentage;
AUDITOR_NAMED = indicator variable that is equal to 1 when the auditor is named in the complaint, and is equal to 0 otherwise;
AUDITOR_RESIGN = 1 if the auditor resigns during the class period, and is equal to 0 otherwise;
BIG4 = indicator variable equal to 1 if the auditor code from Compustat is between 1 and 8;
COMPLEXITY_1 = indicator variable equal to 1 if the relevant accounting standard describes the underlying transaction using any of the three root words: (1) complex*, (2) complic*, and (3) elaborat*, and 0 otherwise;
COMPLEXITY_2 = log of the number of words contained in the definition of the underlying transaction addressed by each accounting standard; we obtain definitions from the glossary of the FASB Codification;
DISMISS = indicator variable equal to 1 if the suit is dismissed;
INSIDER = average monthly net insider selling over the class period, from Thomson Reuters, scaled by total shares outstanding, expressed as a percentage;
INSIDER_SALE = 1 when the plaintiff alleges insider selling activities, and 0 otherwise;

INVESTIGATE_EXTERN = indicator equal to 1 when the complaint indicates that there was an external investigation, and is equal to 0 otherwise;

INVESTIGATE_INTERN = indicator equal to 1 when the complaint indicates that the company conducted an internal investigation, and is equal to 0 otherwise;

ISSUE = indicator variable equal to 1 when the firm issues stock during the class period (Compustat data item SCF), and is equal to 0 otherwise;

LIT_RISK = indicator variable that is set to 1 when the firm operates in a high litigation risk industry (see Francis et al. 1994), and is equal to 0 otherwise;

LN_MAX_DAMAGE = natural logarithm of the *MAX_DAMAGE* variable;

MAX_DAMAGE = highest market value of the firm during the restatement period minus the market value of the firm on the restatement disclosure date (in millions);

MERITORIOUS = indicator variable set to 1 if the suit is meritorious, defined as either (a) a lawsuit that is not dismissed, or (b) a lawsuit that settles for more than 0.5 percent of the defendant firm's market value of equity;

MVE = market value of equity at the end of the quarter preceding the end of the class period;

RBC = average rules-based continuum score of the standards alleged to have been violated. The rules-based continuum score increases by 1 when the standard contains bright-line thresholds, scope and legacy exceptions, voluminous implementation guidance, or a high level of detail. The minimum value of *RBC* is 0 and the maximum is 4. *RBC* is calculated each year for each standard because standards change over time as detail, exceptions, and implementation guidance are added. See Mergenthaler (2010) and Table 1 for further details;

REVENUE = indicator variable equal to 1 if the plaintiff alleged that revenue was misstated, and is equal to 0 otherwise;

ROA = earnings before extraordinary items (as originally reported) for the quarter preceding the end of the class period, scaled by total assets, expressed as a percentage;

SEC_11 = indicator variable that is equal to 1 when the complaint indicates a Section 11 violation, and is equal to 0 otherwise;

SETTLEMENT = dollar amount of the settlement in the lawsuit;

SHARE_TURN = average daily turnover (volume/shares outstanding) over the class period, expressed as a percentage;

SIZE = log market value of equity at the end of the quarter preceding the end of the class period;

SUED = indicator variable equal to 1 if the restatement resulted in a class action lawsuit, and is equal to 0 otherwise;

VOLATILITY = standard deviation of daily returns over the class period, expressed as a percentage; and

WHISTLE = indicator variable that is set to 1 when the complaint mentions an external whistle blower, and is equal to 0 otherwise.

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City-Level Auditor Industry Specialization, Economies of Scale, and Audit Pricing

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ABSTRACT: We examine the effects of city-level auditor industry specialization and scale economies on audit pricing in the United States. Using a sample of Big N clients for the 2000–2007 period, and a scale measure based on percentile rankings of the number of audit clients at the city-industry level, we document significant specialization premiums and scale discounts in both the pre- and post-Sarbanes-Oxley Act (SOX) periods. However, the effects of industry specialization and scale economies on audit pricing are highly interactive. The negative effect of city-industry scale on audit fees obtains only for clients of specialist auditors. By contrast, clients of non-specialist auditors obtain scale discounts only when they enjoy strong bargaining power, suggesting that auditors are "forced" to pass on scale economies to clients with greater bargaining power.

Keywords: *industry specialization; scale economies; audit pricing; client bargaining power.*

Data Availability: *Data are available from sources identified in the article.*

I. INTRODUCTION

In this study, we investigate the effects of city-level audit-firm industry specialization and scale economies among Big 5 (4) auditors (hereafter, Big N auditors) on audit pricing in the United States. We focus on Big N auditors because they dominate the audit market in the U.S. This

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domination has two outcomes, namely, industry specialization due to the expertise developed from expanding within industries, and economies of scale arising from spreading audit investment costs over a large client base. Academic research has examined the effects of industry specialization and, to a much lesser extent, the effects of scale economies, on audit pricing.

Our investigation is relevant for three reasons. First, recent research has emphasized that the local (city) offices of the Big N are the primary units of decision-making (Ferguson et al. 2003; Francis et al. 2005), and are responsible for determining fee contracts (Francis et al. 1999). It is therefore important that researchers investigate the pricing effects of city-level industry specialization and scale economies. However, studies that have conducted city-level analyses have focused on the city-level specialization premium without controlling for city-level scale differences.[1] Our examination of the U.S. audit market reveals significant differences in scale at the city level among the Big N, although their national operations are relatively more evenly divided.[2] If these scale economies generate cost savings that are passed on to clients, then there should be cross-sectional variation in the observed fee premiums resulting from city-level specialization. Hence, we attempt to identify the separate effects of city-level industry specialization and city-level scale economies on audit pricing.

Second, it is important to examine whether (and in what settings) the benefits of scale economies are passed on to clients, particularly in light of concerns that the Big N audit firms dominate the audit market.[3] In its most recent report on this issue, the Government Accountability Office (GAO) states that "market participants ... raised concerns that splitting up these firms could reduce their economies of scale and the depth of expertise that currently allow the largest firms to effectively and efficiently audit large companies" (GAO 2008, 6). Evidence on the effects of scale economies on the fees charged to audit clients is therefore of interest to regulators, auditors, and market participants.

Third, as we discuss in the next section, not only is there an absence of studies that examine city-level scale economies, but also the studies that examine national-level scale economies employ data from the 1980s or 1990s (with the exception of Fields et al. [2004], who use year 2000 data). The audit market has undergone dramatic changes during the past decade, particularly since the enactment of the Sarbanes-Oxley Act (SOX) of 2002. These changes include the move from self-regulation to quasi-governmental regulation of the accounting profession, new audit requirements (Section 404 of SOX, in particular), a proscription on providing a number of nonaudit services in conjunction with audit services, increased requirements for audit committee interaction with auditors, and changes in other disclosure and management reporting requirements (Krishnan and Yang 2009). Several studies have documented changes in the magnitude and structure of audit fees over the years surrounding SOX (e.g., Griffin and Lont 2007; Ghosh and Pawlewicz 2009). Because industry specialization premiums and scale discounts contribute to the structure of audit fees, it is important to investigate their effects in the new audit environment.

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[1] As we discuss later, a few studies examine the audit-pricing effects of national-level scale economies. For example, Mayhew and Wilkins (2003) examine a sample of IPO firms for the 1991–1997 period and find that, after controlling for industry product differentiation (measured at the national level), audit fees are negatively associated with industry-based economies of scale. Some studies (e.g., Gist 1994; Fields et al. 2004) examine the role of scale economies in specific industries.

[2] The average national market share (based on client audit fees) for Big 4 auditors is 0.25 during our sample period, and ranges from 0.22 (for KPMG) to 0.29 (for PwC), suggesting that their national operations are evenly distributed.

[3] The Sarbanes-Oxley Act of 2002 (SOX) required the Government Accountability Office (GAO) to study the impact of audit firm consolidation "on competition, including the availability of auditor choices ... on the cost, quality, and independence of audit services" (GAO 2003, 2). The GAO (2003) concluded that due to the reduction in the number of audit firms, clients seeking industry specialists in some industries end up having only two audit firms from which to choose.

We examine the city-industry scale economies that arise from expansion within industries at the city level, as industry-specific training and other costs are spread over many clients (Mayhew and Wilkins 2003). Using a large sample of Big N clients in the 2000–2007 period, we find significant variation in the city-industry scale in the U.S. audit market. For example, in 2003, PricewaterhouseCoopers (PwC) was the industry specialist for the electrical and electronic equipment industry (i.e., standard industry classification code 36) in both San Jose and in Orange County, California, with 47 and 4 clients, respectively, in that industry (source: Audit Analytics). Therefore, PwC's industry specialization in San Jose was 11.75 times larger in city scale than that in Orange County, for the same year and same industry.

We examine whether city-industry scale differences have an impact on audit pricing. Because scale economies arise from expanding operations within a market (i.e., the city-industry audit market), we construct our city-industry scale measure as the percentile rank of the number of audit clients for each Big N auditor in city-industry combinations in each year. We test for the effects of our scale measure on audit fees, after controlling for auditor industry specialization.

For differences in scale to have an impact on audit pricing, two conditions must be met. First, the increase in scale must generate cost savings as the auditor expands within city-industries and, second, some or all of these savings must be passed on to clients.[4] Ideally, therefore, we must employ data on audit costs at the city-industry level to ascertain directly the existence of scale economies. However, in common with all researchers in this area, we do not have such cost information. Consequently, we rely on proxies for scale economies to make inferences about their impact on audit pricing. We acknowledge that interpretation of our results is linked to our measure of scale.

Consistent with prior research, our results indicate that industry specialist auditors (i.e., city-industry leaders) earn significant premiums (14.8 percent, or $226,000, on average) compared to non-specialist auditors. In addition, scale discounts are also prevalent throughout our sample period.[5] Evaluated at the sample mean, audit fees decrease by 1.7 percent (or $25,900) for a one-decile increase in city-industry scale. However, we find the effects of industry specialization and economies of scale on audit pricing to be highly interactive. The negative effect of city-industry scale on audit fees applies only to industry specialist auditors, and the audit fee premium for industry specialization is smaller for auditors with a larger city-industry scale. These results suggest that industry specialization and economies of scale are not independent forces affecting audit fees.

We also document interesting differences between the pre- and post-SOX periods: the auditor industry specialization premiums are higher in the latter period, although scale discounts are slightly smaller. However, the aforementioned interactive effects hold in both periods.

Because an auditor's incentive to charge a premium or to pass on scale economies is likely to depend on the relative bargaining power it enjoys *vis-à-vis* its clients (Mayhew and Wilkins 2003; Casterella et al. 2004), we examine whether client bargaining power moderates the effects of specialization and scale on the specialist/non-specialist premium. We find client bargaining power exerts a moderating effect on both the specialization premium and the scale discount. Both specialist and non-specialist auditors pass on some scale discounts to clients with a high degree of bargaining power. However, only specialist auditors pass on scale discounts to clients with a relatively low degree of bargaining power. Thus, our results suggest that auditors are "forced" to pass on scale economies when their clients enjoy greater bargaining power.

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[4] Note that we do not argue that there is a consistent link between audit fees (price) and cost, that is, that consistent margins are made on each audit, in each industry, at each office, of each audit firm. Instead, we acknowledge that cost savings may be passed on differentially across clients. If there are no scale economies, then we would not expect our scale measure to affect audit pricing. Likewise, the absence of an association between scale and audit pricing would not necessarily indicate the absence of scale economies; it could also mean that these economies are not passed on to clients.

[5] As we report later, these findings are robust to alternative specifications of our scale measure.

Although we focus on the scale economies that arise from expansion of auditor operations within industries in cities, scale economies can also arise more generally as the auditor expands operations within a city, regardless of the industry. In additional analysis, we examine these *city-level* scale economies rather than the *city-industry* scale economies presented as our main analysis. The results are similar to our main findings.

We contribute to an understanding of audit pricing by the Big N auditors in a number of ways. First, we add to current knowledge about the pricing effects of city-level auditor industry specialization by showing that scale effects moderate the extent of the specialization premium. Second, to the best of our knowledge, we are the first to document that city-industry-specialization premiums have increased in the post-SOX period. Possible explanations for this increase are that the perceived benefits are viewed as being higher in an environment of greater scrutiny and litigation concerns and/or that the marginal benefit of specialization is greater now, because specialists are able to conduct better Section 404 audits. Third, our finding that auditors pass on scale economies to clients provides some support for the GAO's (2008) observation that there is little evidence of Big N oligopolistic pricing. However, we also find that in some situations, for example, in which the client has a low degree of bargaining power *vis-à-vis* a non-specialist auditor, the latter's city-scale has no effect on audit pricing. Fourth, we focus on differences within Big N auditors, rather than Big N/non-Big N differences, which is particularly relevant given the dominance of Big N auditors in the audit market.

In Section II, we discuss the study's background and develop our research questions. Section III discusses the sample selection and research method. The results are discussed in Sections IV and V, and Section VI concludes.

II. BACKGROUND AND RESEARCH QUESTIONS

Prior Research on Auditor Industry Specialization and Audit Fees

Panel A of Table 1 presents a summary of prior studies that examine the fee impact of industry specialization at the national, city, and global levels. Studies that employ national-level specialization measures yield mixed results. Palmrose (1986) finds no evidence for the existence of a specialization premium in the U.S. Using survey data from 1993, Casterella et al. (2004) document a Big 6 industry specialization premium for the small client segment of the U.S. market, but not for the large client segment. By contrast, Carson and Fargher (2007) identify a premium for large, but not for small, client firms in the Australian market. Huang et al. (2007) replicate the Casterella et al. (2004) study for the years 2000, 2001, 2003, and 2004, similarly documenting a specialization premium only for the small client segment, in both the pre- and post-SOX periods, with the exception of 2003 in which they find no such premium. They speculate that 2003 is "perhaps a unique year due to the flux in the audit market following the enactment of SOX" (Huang et al. 2007, 147). Using data from Hong Kong and Australia, DeFond et al. (2000) and Craswell et al. (1995), respectively, document the presence of a premium, whereas Ferguson and Stokes (2002), using more recent Australian data, do not. At the international level, Carson (2009) examines global audit networks employing samples of firms from more than 60 countries, finding significant fee premiums for global specialists.

Recent studies have shifted attention to city-level auditor industry specialization based on the rationale that the primary audit work and decision-making involving clients occurs in the local offices of the Big N auditors (Francis et al. 2005). Consequently, city offices are likely to be better units for analyses of audit outcomes than national offices. Of the three studies listed in Table 1,

TABLE 1

Evidence from Prior Studies on the Specialization Premium and Economies of Scale

Panel A: Specialization Premium

Studies (1)	Data Characteristics (2)	National-Level Industry Specialization Premium[a] (3)	City-Level Industry Specialization Premium (4)
U.S. Data			
Palmrose (1986)	U.S. data; 1980–1981; Big 8 and non-Big 8 auditors (n = 361); Questionnaire-based study	No specialist premium	NA
Francis et al. (2005)	U.S. data; 2000–2001; Big 5 auditors (n = 3,994)	No specialist premium	City-level and joint city-national-level industry specialization premium
Casterella et al. (2004)	U.S. data; 1993; Big 6 auditors (n = 651); Survey data	Specialists earn a premium only for small companies in the sample	NA
Huang et al. (2007)	U.S. data; 2000, 2001, 2003, 2004 (n = 4,619)	Specialists earn a premium only for small companies in the sample except in year 2003	NA
International Data			
Craswell et al. (1995)	Australian data; 1987; Big 8 and non-Big 8 auditors (n = 1,484)	Specialists earn a premium	NA
Ferguson and Stokes (2002)	Australian data; 1990, 1992, 1994, 1998; Big 6/5 and non-Big 6/5 auditors (n = 955)	Limited support for the existence of specialist premium	NA
Ferguson et al. (2003)	Australian data; 1998; Big 5 auditors (n = 1,046)	No specialist premium	City-level and joint city-national-level industry specialization premium
DeFond et al. (2000)	Hong Kong data; 1992; Big 6 and non-Big 6 auditors (n = 225)	Specialists earn a premium for Big 6 clients	NA
Carson and Fargher (2007)	Australian data; 1998 (n = 558) (primary sample); 1999 and 2004 (sensitivity tests) (n = 1,154)	Specialists earn a premium in largest clients size quintile	City-industry leaders earn a premium in largest size
Carson (2009)	Sample from 62 countries in 2000 (n = 15,583) and 60 countries in 2004 (n = 14,628)	Global specialists earn a premium	NA
Studies Using Specific Settings or Industries			
Mayhew and Wilkins (2003)	U.S. IPO firms; 1991–1997; Big 6 auditors (n = 2,294)	Industry leader earns a premium	NA

(continued on next page)

TABLE 1 (continued)

Studies (1)	Data Characteristics (2)	National-Level Industry Specialization Premium[a] (3)	City-Level Industry Specialization Premium (4)
Fields et al. (2004)	U.S. Financial institutions; 2000; Big 5 and non-Big 5 (n = 277)	No specialist premium	NA
Pearson and Trompeter (1994)	U.S. insurance companies 1982–1986; Big 8 auditors (n = 241)	No specialist premium	NA

[a] The dependent variable in all of these studies is the (log of) audit fee (except in Mayhew and Wilkins [2003], who use accounting fees reported in IPO filings), because of the unavailability of audit fees for IPO firms.

Panel B: Discount from Scale Economies

Studies (1)	Data Characteristics (2)	Discount from National-Level Economies of Scale (3)	Discount from City Level Economies of Scale (4)
International Data			
DeFond et al. (2000)	Hong Kong data; 1992; Big 6 and non-Big 6 auditors (n = 225)	Fee discount for clients of a non-Big 6 firm in the property industry	NA
Studies Using Specific Settings or Industries			
Mayhew and Wilkins (2003)	U.S. IPO firms; 1991–1997; Big 6 auditors (n = 2,294)	Fee discount for greater industry market share (if not an industry leader)	NA
Gist (1994)	U.S. data; 5 regulated and 5 nonregulated industries; 1983–1985; Big 8 and non-Big 8 auditors (n = 263)	Fee discount (using client regulatory complexity as measure of EOS)	NA
Fields et al. (2004)	U.S. financial institutions; 2000; Big 5 and non-Big 5 (n = 277)	Fee discount (using proportion of clients and proportion of assets audited)	NA
Pearson and Trompeter (1994)	U.S. insurance companies 1982–1986; Big 8 auditors (n = 241)	Fee discount (using concentration ratio)	NA

Panel A that address city-level industry specialization, Ferguson et al. (2003), using Australian data, and Francis et al. (2005), using U.S. data, document that city-level industry specialists earn a premium over non-specialists.[6] However, Carson and Fargher (2007), using Australian data, find that, as with their national-level results, the specialization premium holds only for large clients at the city level.

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[6] In addition to city-level specialization, both Ferguson et al. (2003) and Francis et al. (2005) document that joint city-national specialists also earn fee premiums. However, national-level specialization alone does not result in a fee premium.

Prior Literature on Economies of Scale and Audit Fees

Table 1, Panel B, shows the relatively short list of previous studies that discuss the pricing effects of economies of scale, all of them focused on the national level. Employing population data, DeFond et al. (2000) find evidence of scale economies for the property industry in Hong Kong. Other studies examine scale economies in specific contexts. Examining the initial public offering (IPO) setting, Mayhew and Wilkins (2003) estimate an audit-fee model in which the test variables are measures of industry specialization, proxied by industry leadership, and scale economies, proxied by the auditor's industry market share. Their results show a positive association between industry specialization and audit fees. In addition, auditors' industry share has a negative coefficient, suggesting that, after controlling for industry specialization, auditors pass on some of the benefits of economies of scale. Fields et al. (2004), Pearson and Trompeter (1994), and Gist (1994) examine the role of scale economies in the financial services industry, the insurance industry, and five regulated and five unregulated industries, respectively, finding support for the existence of scale discounts.[7]

City-Level Economies of Scale

As firms expand operations within an audit market, or within a particular industry in that market, the efficiencies stemming from economies of scale can reduce unit costs (Scherer and Ross 1990; Junius 1997).[8] A larger operation allows for the better allocation of resources, sharing of knowledge, and intrafirm networking. For an audit firm, the scale economies can arise from substantial investment in general audit technology (e.g., audit software development or hardware acquisition) and human capital development (e.g., staff training), which are likely to be shared among all of their clients. Once these investments are in place, additional clients can be serviced at a lower marginal cost than the cost of servicing the first few clients.

At the city level, larger offices also enable audit firms to specialize within industries, leading to scale economies at the industry level. Mayhew and Wilkins (2003, 35–36) provide a comprehensive discussion of how scale economies arise from industry specialization. These economies arise because industry specialization enables auditors to service a larger number of firms within an industry, as they possess similar client characteristics and service needs, thereby reducing audit costs and simultaneously increasing the "service value" provided to clients. Mayhew and Wilkins (2003, 36) attribute the decrease in costs to audit firm personnel's ability to be "adept at identifying and addressing industry-specific audit issues." Auditors are also able to reduce their incremental industry-specific training costs as their number of clients increases.

We refer to the efficiencies arising from auditors' expansion within city-industries as city-industry scale economies. If a larger city-industry scale results in cost savings (scale economies), and some or all of these savings are passed on to clients, then scale differences should produce differences in audit fees. *A priori*, we expect a larger city-industry scale to be associated with lower audit fees. For example, if auditors A and B have 20 and 4 clients, respectively, in industry X in a given city, then we would expect auditor A to have greater city-industry economies of scale than auditor B (Mayhew and Wilkins 2003). However, if a larger scale does not lower costs (i.e., there are no economies of scale), or if lower costs are not passed on to clients, then we would expect there to be no association between scale and audit fees.

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[7] Francis and Stokes (1986) examine economies of scale in the context of the Big N premium rather than the industry specialization premium.

[8] Willenborg (2002) also points out that industry specialization (usually measured in terms of greater auditor size in the industry) enables an auditor to audit both "smarter" (with greater effectiveness) and "faster" (with higher efficiency).

Therefore, we examine whether auditors with a larger city-industry scale offer discounts to their audit clients. As a corollary, we also examine whether industry specialists charge lower specialization premiums to their audit clients if they enjoy city-industry scale economies (i.e., whether the specialization premium is moderated by scale).

Client Bargaining Power and Audit Fees

Auditors may not always pass on scale economies in the form of fee discounts. Casterella et al. (2004) and Mayhew and Wilkins (2003) argue that auditor/client bargaining power can influence audit pricing. Consequently, we examine whether the audit-pricing effects of industry specialization and scale economies are moderated by clients' relative bargaining power.

Casterella et al. (2004) define bargaining power as the auditor and client's importance to each other, and they measure it in both absolute terms (client size) and in terms of the client's importance in the auditor's portfolio. Mayhew and Wilkins (2003) employ a more restrictive definition of bargaining power, which considers the degree of differentiation. They argue that economies of scale are less likely to be passed on to the client when the differentiation is sufficiently marked as to give the auditor greater bargaining power *vis-à-vis* the client. Both studies find that client bargaining power lowers audit fees. Thus, *a priori*, we expect greater client bargaining power to decrease the industry specialization premium and increase the scale discount.

The Sarbanes-Oxley Act and Audit Fees

In addition to our focus on city-level specialization and scale economies, we also note that our use of a recent sample period constitutes an important extension of the prior research, because the audit market has undergone dramatic changes over the past decade, particularly after the enactment of SOX. The accounting profession has moved from self-regulation to independent regulation by the Public Company Accounting Oversight Board (PCAOB), which has assumed the standard-setting process for the audits of public companies and has instituted an inspection process for accounting firms (Kinney 2005). In addition, several changes introduced by SOX have also affected auditors, either directly by changing the audit requirements or indirectly by changing the corporate governance or management reporting requirements. Section 404 of the Act, which requires audits of clients' internal control over financial reporting in conjunction with financial statement audits, constitutes a major change in the audit process (Dickins et al. 2008). Section 201 bans the provision of many nonaudit services that had traditionally been provided by CPA firms to their audit clients. Section 301 empowers audit committees to appoint auditors, set their compensation, and oversee the audit process. SOX requirements have also increased the degree of communication between audit committees and auditors (Krishnan and Yang 2009).[9]

These changes have affected both the magnitude and structure of audit fees. Griffin and Lont (2007) and Ghosh and Pawlewicz (2009) show significant increases in audit fees during the post-SOX period. Other studies show fee increases resulting from SOX Section 404 (Raghunandan and Rama 2006; Hogan and Wilkins 2008; Hoitash et al. 2008). Fleming and Romanus (2007) provide evidence of a negative association between industry specialization and audit fee changes in the first year following SOX Section 404 implementation, but find no effect in subsequent years. They attribute this one-time negative association to the ability of industry specialist auditors to conduct more efficient internal control audits because of their superior knowledge of clients' accounting and internal control systems. Some studies show structural changes in audit fees. For example, Ghosh and Lustgarten (2006) and Ghosh and Pawlewicz (2009) document changes in fee-cutting in the pre- and post-SOX environments. Francis and Wang (2005) show that the disclosure of audit fees in proxy filings (starting in 2001) resulted in a change in the precision of audit pricing. In addition, to the extent that audit and nonaudit fees are simultaneously determined (DeFond et al. 2002; Whisenant et al. 2003), it is possible that the proscription of nonaudit services could have caused changes in the determinants of audit fees.

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[9] Other SOX requirements include CEO and CFO certifications, reconciliation of non-GAAP financial measures, and additional disclosures concerning SPEs (Griffin and Lont 2007). During the post-SOX period, there were also many accounting and auditing standards issued by the PCAOB and FASB. See Krishnan and Yang (2009) for a discussion.

These changes could also have caused changes in auditors' industry specialization premiums and/or scale discounts. The demise of Arthur Andersen likely caused adjustments in the city-industry specializations of the remaining auditors. Increased audit demands and capacity constraints, arising both from Arthur Andersen's withdrawal as a supplier of audit services and the new SOX requirements, could have caused further modifications in client portfolios, adding to potential changes in city-industry portfolios. Heightened litigation concerns could also have increased specialization premiums if the perceived benefits of auditor specialization were amplified by those concerns.

Scale discounts may have been affected due to both changes in cost structures that generate cost savings and to factors that determine whether the savings will be passed on to clients. Factors likely to have changed the underlying cost structure include the removal of many nonaudit services that were previously provided in conjunction with audit services and the introduction of integrated audits of internal controls and financial statements. The increased transparency of audit fee disclosures could also have increased scale discounts if auditors' ability to withhold these discounts was reduced. However, the demise of Arthur Andersen reduced auditor choice for clients, which could also have had implications for auditor competition and scale discounts. Due to these conflicting forces, we expect that the changed audit environment is likely to have had an impact on both specialization and scale effects, although the direction of these impacts cannot be predicted.

Summary of Research Questions

In sum, we pursue four research questions. First, we examine whether, in addition to city-industry specialization, city-industry scale affects audit pricing. All else being equal, we expect a greater city-industry scale to be associated with lower audit fees. Second, we examine whether the city-industry specialization premium is moderated by auditors' city-industry scale, implying that auditors would give up some part of the premium if there are scale effects. Third, we examine whether client bargaining power affects the city-level specialization premium and scale discount. Fourth, and finally, we examine differences in the specialization premium and scale discount in the pre- and post-SOX periods.

III. DATA AND RESEARCH METHOD

Sample

Table 2 presents the sample screening procedures. We start with all U.S. publicly listed companies with Big N auditors for which fee data for the 2000–2007 period are available on the Audit Analytics database, yielding 57,596 firm-year observations.[10] We then exclude: (1) firms unavailable on Compustat (15,552); (2) firms in the financial sector (10,927); (3) foreign firms (2,664); and (4) firms with missing Compustat data (2,237). These exclusions leave a total of

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[10] In common with other studies in this area, our data pertain to publicly listed companies, thus ignoring the specialization or scale effects of private clients. We acknowledge this as a limitation.

TABLE 2
Sample Selection

Screening	
Big N observations with audit fee data for 2000–2007 on Audit Analytics	57,596
Less:	
Companies not available on Compustat	(15,552)
Financial Sector (SIC Codes 6000–6999)	(10,927)
Foreign firms	(2,664)
Missing Compustat data	(2,237)
Missing auditor city information	(929)
City-industry-year combinations with less than two observations, or with only one Big N auditor	(8,080)
Final Sample Size	17,207

26,216 firm-year observations. In addition, because both industry expertise and auditor scale are identified at the city level, we exclude: (5) observations for which auditor city information is unavailable (929); and (6) observations for city-industry-year combinations for which there are fewer than two companies and cities with only one Big N auditor (8,080).[11] Hence, the final sample, which includes 54 two-digit SIC industries, consists of 17,207 firm-year observations, with the number of annual observations varying between 1,637 and 2,600.

Empirical Model

Following Francis et al. (2005), we estimate the following regression model to test our research questions:

$$\begin{aligned} LAF = {} & \beta_0 + \beta_1 Spec + \beta_2 Scale + \beta_3 CITYSIZE + \beta_4 LTA + \beta_5 LSEG + \beta_6 CATA + \beta_7 QUICK \\ & + \beta_8 DE + \beta_9 ROI + \beta_{10} FOREIGN + \beta_{11} OPINION + \beta_{12} YE + \beta_{13} LOSS \\ & + \beta_{14} AAClients + Fixedeffects + \varepsilon, \end{aligned} \tag{1}$$

where:

LAF = natural log of audit fees (in thousands of dollars);
$Spec$ = indicator variable coded 1 for auditors that are city industry leaders, and 0 otherwise;
$Scale$ = percentile rank of the city-industry number of audit clients (*NClients*) for each Big N audit firm (variable values range from 0.01 to 1);
$CITYSIZE$ = natural log of aggregate audit fees for all firms audited by the company's auditor for each city;
LTA = natural log of total assets (in millions of dollars);

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[11] Following Francis et al. (2005), we exclude city-industry-year combinations with less than two observations because of the possibility of measurement error in the auditor city designation, and because it is unlikely to be a competitive audit market for city-level industry specialization if there is only one client in the industry for the city. We delete cities with only one Big N auditor to better identify specialists. We are concerned that, in cities in which there is only one Big N auditor, this auditor would automatically be the specialist. As a sensitivity test, we re-run our models on a sample of 20,391 observations in which the city has only one Big N auditor. The results are similar to those reported.

LSEG = natural log of the number of unique business segments;
CATA = ratio of current assets to total assets;
QUICK = ratio of current assets (excluding inventories) to current liabilities;
DE = ratio of long-term debt to total assets;
ROI = ratio of earnings before interest and tax to total assets;
FOREIGN = proportion of total sales from foreign operations;
OPINION = indicator variable coded 1 for modified audit report, and 0 otherwise;
YE = indicator variable coded 1 for non-December 31 year-end, and 0 otherwise;
LOSS = indicator variable coded 1 if loss in current fiscal year, and 0 otherwise;
AAClients = indicator variable coded 1 if there was an auditor change and the predecessor audit firm in year t–1 was Arthur Andersen, and 0 otherwise;
Fixedeffects = year and industry indicator variables; and
ε = random-error term.

City-Industry Specialization Measure

Following Francis et al. (2005), we use audit fees to construct the specialization variables. We first compute each Big N audit firm's share in each two-digit industry group in each city, and then rank the auditors from 1 to 5 based on their city-level industry share.[12] The auditor with the largest share is designated the industry specialist (*Spec* = 1). The untabulated mean (median) difference in the market share of the industry specialist auditor and its closest competitor is 35.8 percent (32 percent) in our sample. The first quartile of the difference is 11.6 percent, indicating that, for the majority of the sample, the top two auditors are not very close in terms of market share.[13]

Table 3, Panel A, presents a summary of the number of city-industries in which each of the five auditors is an industry specialist. Not surprisingly, Arthur Andersen has the lowest number (212), because it is in our sample for only the first two years (2000–2001). The other four auditors' specialist status ranges from 835 city-industries for KPMG to 1,230 city-industries for PwC.

City-Industry Scale Measure

Unlike auditor industry specialization measures, which are widely used in the literature, proxies for scale economies are less commonly employed. The traditional measures of scale economies used in the economics literature require cost data (Scherer and Ross 1990).[14] However, these data are not generally available in the auditing context, and our measure of scale economies is thus indirect. Because scale economies are generated by expanding operations within a market (i.e., the city-industry), we base our measure on the number of clients. To construct our city-level scale measure, we first compute *NClients* (the number of clients the auditor has in each city-industry

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[12] Because of the demise of Arthur Andersen in 2002, our sample includes the Big 5 in 2000–2001 and the Big 4 in 2002–2007. As we report later, we include a variable for Andersen clients in our regressions to control for this change.

[13] In sensitivity analysis, we delete observations for which the difference between the market shares of the leading and second auditor is 5 percent or less, and re-run the regressions. The results are very similar to those reported later in our main findings. In another sensitivity test, we also define "specialist" based on a 20 percent (25 percent) market share threshold for 2000–2001 (2002–2007). The results are similar to our main findings.

[14] These measures include the price-cost margin, Lerner index (price minus marginal cost divided by price), minimum efficient plant size, and cost elasticities (e.g., Bain 1956; Waring 1996; Paul and Siegel 1999; see Junius [1997] for a review). Some studies of the financial services industry construct measures of scale economies using data from Compustat (e.g., Zacharias 2008; Hao 2009). For example, studies of the banking industry employ data on expenses along with measures of output to estimate the parameters of a production function and, therefore, economies of scale (Berger and Mester 1997).

TABLE 3

Distribution of Specialist and Scale Measures

Panel A: Specialist Status of Auditors: The Number of City-Industry Combinations for which the Auditor is a Specialist

Auditor	2000	2001	2002	2003	2004	2005	2006	2007	Total
AA	97	115							212
DT	72	112	147	138	124	123	130	128	974
EY	110	138	186	183	166	144	139	143	1,209
KPMG	73	99	131	124	125	102	101	80	835
PwC	114	162	196	174	168	159	134	123	1,230
All	466	626	660	619	583	528	504	474	4,460

Panel B: Number of Clients Audited at the City-Industry Level (*NClients*)

Auditor	Mean	Q1	Median	Q3
AA	15.73	4.00	8.00	21.00
DT	13.14	4.00	7.00	16.00
EY	14.77	4.00	8.00	20.00
KPMG	11.02	3.00	6.00	14.00
PwC	18.65	4.00	9.00	27.00
All	14.94	4.00	8.00	20.00

AA = Arthur Andersen;
DT = Deloitte;
EY = Ernst & Young;
KPMG = KPMG; and
PwC = PricewaterhouseCoopers.

combination) for each year. We rank *NClients* across all city-industry combinations. *Scale* is the percentile rank of *NClients* across all city-industry combinations for each year; its values range from 0.01 to 1.[15] The percentile transformation is desirable because *NClients* is highly skewed (Table 3, Panel B). Alternative specifications, such as *NClients* in raw form or in its logarithmic transformation, yield results similar to those reported below.

In the auditing literature, national-level scale measures (see Table 1, Panel B), include the proportion of clients audited (Fields et al. 2004) and the proportion of clients' assets audited

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[15] To illustrate, consider the following example. The number of clients (*NClients*) in 2001 for each Big 5 auditor in two-digit SIC industry 73 (business services) in Boston and two-digit SIC industry 28 (chemicals and allied products) in Philadelphia are as follows: Boston = 25, 23, 10, 7, 7; Philadelphia = 8, 6, 5, 1, 1. To construct *Scale*, we convert the city-industry numbers (i.e., 25, 23, 10, 8, 7, 7, 6, 5, 1, 1) into percentile ranks for each year. We use the PROC RANK procedure in SAS to do this, but because the procedure produces ranks between 0 and 99, we add 1 to the ranks generated by it to obtain ranks between 1 and 100. Thus, *Scale* takes the value of 0.91 (the SAS-generated rank + 1 scaled by 100) for the auditor in Boston with 25 clients, and 0.14 for the auditors in Philadelphia with one client. Note that we do not construct the measure within each city-industry combination, as doing so could lead to undesirable results. For example, using the foregoing numbers, if we ranked auditors within each city-industry combination, then we would have two large-scale auditors (25 and 8), although 8 is a relatively small-scale operation. With our measure, an auditor with eight clients in a city-industry would be considered a relatively small-scale auditor, regardless of where it operates.

(Mayhew and Wilkins 2003). However, these market share-based measures are not conducive to city-level analysis. For example, a 50 percent share in city-industry A could comprise five clients, whereas a 50 percent share in city-industry B could comprise 25 clients. By contrast, our measure provides uniform scaling across all city-industry combinations. We later report sensitivity tests using these alternative measures of scale, pooling them across city-industry combinations, as we do with our primary measure.

Table 3, Panel B, presents the distribution of *NClients*, which forms the basis for our scale variable. We observe considerable variation in the number of clients that each auditor has across city-industry combinations. The interquartile range for the number of clients in city-industry combinations varies from 11 for KPMG to 23 for PwC.

Control Variables

We include control variables based on previous studies. Recent studies (Francis and Yu 2009; Choi et al. 2010) suggest that larger city offices provide higher quality audits. Francis and Yu (2009) find larger Big N offices to be more likely to issue going-concern audit reports and clients in larger offices to be associated with less aggressive earnings management. Choi et al. (2010) find that office size is positively associated with audit fees, which is consistent with the quality-differentiation argument. Therefore, we include *CITYSIZE*, measured as the (logarithm of) aggregate audit fees for all firms audited by the company's auditor for each city.

Other control variables, following Francis et al. (2005), include firm size (*LTA*), audit opinion (*OPINION*), a variable indicating a December fiscal year-end (*YE*), and measures of firm complexity and audit risk. The firm-complexity variables are the number of business segments (*LSEG*) and the extent of foreign operations (*FOREIGN*). The audit-risk variables include *CATA*, *DE*, *LOSS*, *QUICK*, and *ROI*. We also include *AAClients* to examine the incremental effect on audit fees for auditor changes involving Arthur Andersen. Finally, we include year and industry indicator variables.[16]

IV. RESULTS

Univariate Analyses

The descriptive statistics for all variables, which are reported in Table 4, are generally comparable with those reported in previous studies (e.g., Francis et al. 2005). The mean (median) audit fee is $1,525.34 thousand ($559.61 thousand).[17] Correlations between the variables are presented in Panels B and C of Table 4. All correlations between independent variables are less than 0.5, except for those between *ROI* and *LOSS* (–0.57) and between *LTA* and *CATA* (–0.52).[18]

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[16] The year indicator variables serve to control for the numerous policy changes that occurred during our sample period, particularly the changes necessitated by SOX Sections 302 and 404.

[17] This is a higher figure than that in some prior studies. For example, Francis et al. (2005) report that the mean (median) audit fee is $502,000 ($196,000). We attribute the discrepancy to the fact that the sample period in Francis et al. (2005) covers the first-time fee disclosures during 2000–2001, whereas our sample includes the post-SOX years 2002–2007, during which audit fees are known to have increased (Ettredge et al. 2007). For our 2000–2001 observations, the mean (median) audit fee is $558,000 ($202,000), which is close to the mean (median) figure in Francis et al. (2005).

[18] We re-run our models after eliminating *ROI*, *LOSS*, *LTA*, and *CATA* (one at a time), and the results remain unchanged. *CITYSIZE* is positively correlated with *Scale*, although neither the magnitude of the correlation (0.35) nor the VIF values for the two variables raise serious concerns about multicollinearity. Nevertheless, we examine the effects of alternative forms of *CITYSIZE* (i.e., an indicator variable for large and small city size and quartile and quintile rankings) on our results. In all cases, the results are similar to those reported.

TABLE 4
Descriptive Statistics

Panel A: Sample Statistics (n = 17,207)

Variable	Mean	Median	Std. Dev.	Q1	Q3
Audit fees	1,525.340	559.610	3,466.880	220.000	1,417.080
LAF	6.379	6.327	1.332	5.394	7.256
Assets	2,658.640	348.476	9,901.370	90.301	1,410.140
LTA	5.906	5.854	1.998	4.503	7.251
SEG	2.036	1.000	1.571	1.000	3.000
LSEG	0.477	0.000	0.643	0.000	1.099
CATA	0.505	0.507	0.256	0.298	0.713
QUICK	2.493	1.505	2.946	0.933	2.808
DE	0.190	0.113	0.232	0.000	0.298
ROI	−0.026	0.055	0.284	−0.046	0.108
FOREIGN	0.144	0.000	0.230	0.000	0.248
OPINION	0.045	0.000	0.207	0.000	0.000
YE	0.277	0.000	0.447	0.000	1.000
LOSS	0.406	0.000	0.491	0.000	1.000
AAClients	0.029	0.000	0.167	0.000	0.000
CITYSIZE	17.226	17.380	1.337	16.379	18.120
Spec	0.462	0.000	0.499	0.000	1.000
Scale	0.506	0.520	0.284	0.140	0.760
Power	0.505	0.510	0.289	0.260	0.760

Panel B: Correlations from *LTA* to *OPINION* (n = 17,207)

	Variable	*LTA*	*LSEG*	*CATA*	*QUICK*	*DE*	*ROI*	*FOREIGN*	*OPINION*
(1)	*LAF*	0.78 (< 0.01)	0.39 (< 0.01)	−0.31 (< 0.01)	−0.26 (< 0.01)	0.19 (< 0.01)	0.31 (< 0.01)	0.27 (< 0.01)	−0.11 (< 0.01)
(2)	*LTA*		0.39 (< 0.01)	−0.52 (< 0.01)	−0.24 (< 0.01)	0.28 (< 0.01)	0.47 (< 0.01)	0.16 (< 0.01)	−0.21 (< 0.01)
(3)	*LSEG*			−0.21 (< 0.01)	−0.20 (< 0.01)	0.10 (< 0.01)	0.20 (< 0.01)	0.20 (< 0.01)	−0.06 (< 0.01)
(4)	*CATA*				0.46 (< 0.01)	−0.46 (< 0.01)	−0.26 (< 0.01)	0.10 (< 0.01)	0.01 (0.57)
(5)	*QUICK*					−0.25 (< 0.01)	−0.15 (< 0.01)	0.01 (0.16)	−0.08 (< 0.01)
(6)	*DE*						0.11 (< 0.01)	−0.08 (< 0.01)	−0.01 (0.21)
(7)	*ROI*							0.09 (< 0.01)	−0.38 (< 0.01)
(8)	*FOREIGN*								−0.03 (< 0.01)

(continued on next page)

TABLE 4 (continued)

Panel C: Correlations from *YE* to *Power* (n = 17,207)

	Variable	*YE*	*LOSS*	*AAClients*	*CITYSIZE*	*Spec*	*Scale*	*Power*
(1)	*LAF*	−0.07	−0.27	−0.05	0.39	0.23	0.01	0.30
		(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(0.83)	(< 0.01)
(2)	*LTA*	−0.07	−0.37	0.01	0.21	0.22	−0.05	0.48
		(< 0.01)	(< 0.01)	(0.73)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)
(3)	*LSEG*	0.01	−0.19	−0.01	0.06	0.10	−0.12	0.22
		(0.72)	(< 0.01)	(0.28)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)
(4)	*CATA*	0.15	0.20	−0.01	−0.01	−0.08	0.13	−0.38
		(< 0.01)	(< 0.01)	(0.07)	(0.14)	(< 0.01)	(< 0.01)	(< 0.01)
(5)	*QUICK*	−0.01	0.18	−0.01	−0.02	−0.05	0.19	−0.24
		(0.12)	(< 0.01)	(0.12)	(0.01)	(< 0.01)	(< 0.01)	(< 0.01)
(6)	*DE*	−0.12	0.02	0.01	0.01	0.07	−0.10	0.27
		(< 0.01)	(0.03)	(0.24)	(0.16)	(< 0.01)	(< 0.01)	(< 0.01)
(7)	*ROI*	0.06	−0.57	0.02	0.08	0.07	−0.16	0.37
		(< 0.01)	(< 0.01)	(0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)
(8)	*FOREIGN*	0.07	−0.06	−0.02	0.12	0.06	0.11	−0.06
		(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)
(9)	*OPINION*	−0.03	0.22	0.01	−0.04	−0.02	0.01	−0.11
		(< 0.01)	(< 0.01)	(0.66)	(< 0.01)	(<0.01)	(0.72)	(< 0.01)
(10)	*YE*		−0.07	−0.01	0.01	−0.04	−0.04	−0.03
			(< 0.01)	(0.41)	(0.11)	(< 0.01)	(< 0.01)	(< 0.01)
(11)	*LOSS*			0.01	−0.08	−0.06	0.14	−0.27
				(0.49)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)
(12)	*AAClients*				−0.01	−0.01	0.02	0.01
					(0.06)	(0.31)	(0.01)	(0.92)
(13)	*CITYSIZE*					0.07	0.35	−0.04
						(< 0.01)	(< 0.01)	(< 0.01)
(14)	*Spec*						0.19	0.02
							(< 0.01)	(0.03)
(15)	*Scale*							−0.49
								(< 0.01)
(16)	*Power*							1.00

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p-values (two-tailed) are reported in parentheses.

Variable Definitions:
Audit fees = audit fees (in thousands of dollars);
LAF = natural log of audit fees (in thousands of dollars);
Assets = total assets (in millions of dollars);
LTA = natural log of total assets (in millions of dollars);
SEG = number of unique business segments;
LSEG = natural log of the number of unique business segments;
CATA = ratio of current assets to total assets;
QUICK = ratio of current assets (excluding inventories) to current liabilities;
DE = ratio of long-term debt to total assets;
ROI = ratio of earnings before interest and tax to total assets;
FOREIGN = proportion of total sales from foreign operations;
OPINION = indicator variable coded 1 for modified audit report, and 0 otherwise;
YE = indicator variable coded 1 for non-December 31 year-end, and 0 otherwise;
LOSS = indicator variable coded 1 if loss in current fiscal year, and 0 otherwise;
AAClients = indicator variable coded 1 if there was an auditor change and the predecessor audit firm in year *t*−1 was Arthur Andersen, and 0 otherwise;

(continued on next page)

TABLE 4 (continued)

CITYSIZE = natural log of aggregate audit fees for all firms audited by the company's auditor for each city;
Spec = indicator variable coded 1 for auditors that are city industry leaders, and 0 otherwise;
Scale = percentile rank of the city-industry number of audit clients (*NClients*) for each Big N audit firm (variable values range from 0.01 to 1); and
Power = percentile ranks of bargaining power measure computed as the natural log of company sales divided by the sum of the natural log of sales for all firms audited by the company's auditor for each city-industry combination (variable values range from 0.01 to 1).

Multivariate Analyses

Table 5, Panel A, reports the results from estimating Equation (1). We cluster standard errors by year and two-digit SIC industry (Petersen 2009; Gow et al. 2010) to address potential problems associated with the non-independence of the panel observations.[19] The adjusted R^2 is 0.79, and the control variables are statistically significant in the expected directions, comparable to those reported by Francis et al. (2005).[20]

Consistent with Francis et al. (2005), the coefficient on *Spec* is positive and significant ($p < 0.01$). In addition, the coefficient on *Scale* is negative and significant ($p < 0.01$). The coefficient values indicate that, all else equal, a specialist charges a 14.8 percent higher audit fee than a non-specialist. A one-decile increase in scale is associated with a 1.7 percent decline in audit fees. This suggests that, on average, a larger-scale auditor at the city-industry level is likely to pass on a portion of the scale economies to its audit clients by charging a lower audit fee.

Because our sample period is marked by many changes in the financial and regulatory environment that could have affected audit fee patterns, we also estimate the regression for the pre- and post-SOX periods and for each of our sample years. Panel B of Table 5 presents the estimates for the pre- and post-SOX periods. Interestingly, both the specialization premium and scale discount exist in both periods, although the former is significantly stronger in the post-SOX period, as indicated by the F-test ($p < 0.01$). All else being equal, the specialization premiums are 9.5 percent and 16.2 percent in the pre- and post-SOX periods, respectively. The coefficients for *Scale* for the two periods indicate that the scale discount is significantly weaker in the post-SOX period ($p < 0.01$). However, the magnitude of the difference in the scale discount between the two periods is not as large as the difference in the specialization premium. A one-decile increase in city-scale is associated with a decline in audit fees of 1.8 percent in the pre-SOX period and 1.6 percent in the post-SOX period. Thus, whereas the increase in the importance of the specialization premium appears to be both statistically and economically significant, the decrease in the scale discount is statistically significant but is not as economically significant.

Table 5, Panel C, presents the yearly coefficients for *Spec* and *Scale*. The specialization premium is significant throughout the period and, as in Panel B, generally higher in the post-SOX period. The scale discount is also significant in all years except 2006, for which the p-value is 0.11.

Alternative Measures of Scale

Table 5, Panels D and E, presents alternative versions of our scale measure, based on the proportion of clients audited (Fields et al. 2004) and the proportion of clients' assets audited

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[19] As an alternative approach to dealing with non-independent observations, we also test the significance of our coefficients using the Fama-MacBeth statistic, and the results are similar to those reported.

[20] One interesting result is that the coefficient on *AAClients* is negative and significant, suggesting that Arthur Andersen-related auditor changes are associated with a lower audit fee.

TABLE 5

Multivariate Analyses of Industry Specialization and Economies of Scale among Big N Auditors

Panel A: Full Sample Results—Dependent Variable: *LAF*

	Estimate	p-value
Spec	0.138	(< 0.01)
Scale	−0.172	(< 0.01)
Intercept	1.926	(< 0.01)
LTA	0.519	(< 0.01)
LSEG	0.138	(< 0.01)
CATA	0.511	(< 0.01)
QUICK	−0.055	(< 0.01)
DE	0.068	(0.04)
ROI	−0.315	(< 0.01)
FOREIGN	0.360	(< 0.01)
OPINION	0.248	(< 0.01)
YE	−0.178	(< 0.01)
LOSS	0.094	(< 0.01)
AAClients	−0.081	(0.04)
CITYSIZE	0.095	(< 0.01)
Fixedeffects	Included	
Adjusted R^2	0.794	
n	17,207	

Panel B: Pre-SOX and Post-SOX Period Results—Dependent Variable: *LAF*

	Pre-SOX Period (2000–2001)		Post-SOX Period (2002–2007)	
	Estimate	p-value	Estimate	p-value
Spec	0.091	(< 0.01)	0.150	(< 0.01)
Scale	−0.184	(< 0.01)	−0.160	(< 0.01)
Intercept	1.264	(< 0.01)	1.853	(< 0.01)
LTA	0.500	(< 0.01)	0.529	(< 0.01)
LSEG	0.143	(< 0.01)	0.133	(< 0.01)
CATA	0.498	(< 0.01)	0.514	(< 0.01)
QUICK	−0.062	(< 0.01)	−0.050	(< 0.01)
DE	0.142	(0.06)	0.040	(0.28)
ROI	−0.387	(< 0.01)	−0.299	(< 0.01)
FOREIGN	0.262	(< 0.01)	0.380	(< 0.01)
OPINION	0.295	(< 0.01)	0.215	(< 0.01)
YE	−0.089	(< 0.01)	−0.199	(< 0.01)
LOSS	0.041	(0.14)	0.119	(< 0.01)
AAClients	−0.174	(0.58)	−0.082	(0.05)
CITYSIZE	0.092	(< 0.01)	0.095	(< 0.01)
Fixedeffects	Included		Included	
Adjusted R^2	0.749		0.776	
n	4,235		12,972	

F-tests for difference in coefficients between the pre-SOX and post-SOX periods:
Spec: p-value = (< 0.01).
Scale: p-value = (< 0.01).

(continued on next page)

TABLE 5 (continued)

Panel C: Coefficients on *Spec* and *Scale* for Yearly Regressions—Dependent Variable: *LAF*

Year		*Spec*	*Scale*
2000	Estimate	0.086	−0.217
	p-value	(< 0.01)	(< 0.01)
2001	Estimate	0.094	−0.161
	p-value	(< 0.01)	(< 0.01)
2002	Estimate	0.170	−0.108
	p-value	(< 0.01)	(0.08)
2003	Estimate	0.133	−0.173
	p-value	(< 0.01)	(< 0.01)
2004	Estimate	0.174	−0.204
	p-value	(< 0.01)	(< 0.01)
2005	Estimate	0.141	−0.163
	p-value	(< 0.01)	(0.03)
2006	Estimate	0.108	−0.095
	p-value	(< 0.01)	(0.11)
2007	Estimate	0.139	−0.170
	p-value	(< 0.01)	(0.01)

Panel D: Alternative Measure of *Scale*—Dependent Variable: *LAF*

	***Scale* Defined Using Proportion of Clients Audited**					
	Full Sample		**Pre-SOX Period (2000–2001)**		**Post-SOX Period (2002–2007)**	
	Estimate (1)	**p-value (2)**	**Estimate (3)**	**p-value (4)**	**Estimate (5)**	**p-value (6)**
Spec	0.181	(< 0.01)	0.142	(< 0.01)	0.194	(< 0.01)
Scale	−0.203	(< 0.01)	−0.242	(< 0.01)	−0.201	(< 0.01)
Adjusted R^2	0.795		0.750		0.777	
n	17,207		4,235		12,972	

F-tests for difference in coefficients between the pre-SOX and post-SOX periods:
Spec: p-value = (< 0.01).
Scale: p-value = (< 0.01).

Panel E: Alternative Measure of *Scale*—Dependent Variable: *LAF*

	***Scale* Defined Using Proportion of Clients' Assets Audited**					
	Full Sample		**Pre-SOX Period (2000–2001)**		**Post-SOX Period (2002–2007)**	
	Estimate (1)	**p-value (2)**	**Estimate (3)**	**p-value (4)**	**Estimate (5)**	**p-value (6)**
Spec	0.191	(< 0.01)	0.132	(< 0.01)	0.207	(< 0.01)
Scale	−0.233	(< 0.01)	−0.184	(< 0.01)	−0.246	(< 0.01)
Adjusted R^2	0.793		0.749		0.778	
n	15,744		4,019		11,725	

F-tests for difference in coefficients between the pre-SOX and post-SOX periods:
Spec: p-value = (< 0.01).

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TABLE 5 (continued)

Scale: p-value = (< 0.01).

The models in Panels C, D, and E are estimated with all control variables and year and industry fixed effects. Only the variables of interest are reported for brevity. p-values (two-tailed) are reported in parentheses, which are based on robust standard errors clustered by firm.

See Table 4 for other variable definitions.

(Mayhew and Wilkins 2003). We compute these two alternative measures at the city-industry level and construct percentile rankings of auditors, similar to our *Scale* measure. For brevity, we do not report the coefficients for the control variables. The results are similar to those in Panels A and B. Both measures suggest the existence of a significant specialization premium and a significant scale discount for the full sample, as well as for the pre- and post-SOX periods.[21] In addition, as previously discussed, alternative functional forms of our primary scale variable, such as the raw number of clients (*NClients*) and the logarithm of the number of clients, yield similar results.[22]

Interaction between *Spec* and *Scale*

In Table 6, we examine whether there are interactions in the effects of *Spec* and *Scale*. That is, is the scale discount more likely to be passed on when there is a significant specialization premium? We present an augmented model, including the interaction *Spec* × *Scale*, for the full sample (Columns 1–2), the pre-SOX period (Columns 3–4), and the post-SOX period (Columns 5–6).

The coefficient on *Spec* is positive and statistically significant in all three regressions, indicating the presence of a specialization premium even in the absence of scale effects. However, the coefficient on *Scale* is insignificant, and the interaction *Spec* × *Scale* is significant and negative ($p < 0.01$ in Column 2). Thus, scale discounts are passed on to clients only when the auditor enjoys a specialist premium. The coefficient estimates in Column 1 yield a specialization premium of 14.8 percent, estimated at the median value of *Scale*. In addition, an increase in an auditor's city-industry scale by one decile lowers the specialization premium by 3.8 percent.[23]

To explore potential nonlinearities in the interactive effects of *Spec* and *Scale*, we estimate an extended model including three variables—indicating specialists in large offices (*SpecLargeoff*), specialists in small offices (*SpecSmalloff*), and non-specialists in large offices (*nonSpecLargeoff*)—using the median value of *Scale* to demarcate large and small offices. We find that specialists in both large and small offices earn an industry specialization premium (i.e., both *SpecLargeoff* and *SpecSmalloff* are positive and significant), but specialists in larger offices earn a lower premium than their counterparts in smaller offices, and the difference is significant (F-value = 106.5, $p < 0.01$, untabulated). However, for non-specialists, the audit fees are the same regardless of office size (i.e., *nonSpecLargeoff* is not significant), suggesting that only specialists in large offices are likely to pass on the scale discounts to their clients.

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[21] The sample size in Table 5, Panel E (for *Scale* defined using proportion of clients' assets audited) is smaller than in other tables because of the unavailability of asset information on Audit Analytics (the database used to compute our *Scale* measures) due to fewer city-industry combinations for which *Scale* can be computed.

[22] We also test an alternative scale measure based on audit fees, measured by the percentile rank of auditors' city-industry-level sum of total audit fees. We find this variable to be positively related to audit fees. We note, however, that the use of a fee-based measure for economies of scale is problematic in our study because the dependent variable is a fee-based measure (log of audit fee). Our identification of industry specialists (*Spec*) is also based on audit fees.

[23] Denoting the coefficients on *Spec* and *Spec* × *Scale* by a and b respectively, the specialization premium is given by $[\exp(a + b \times Scale) - 1]$. Further, denoting the mean value of *Scale* by *MScale*, we estimate the premium at the mean value by $[\exp(a + b \times MScale) - 1]$, and the change in premium for a change in scale ($\Delta Scale$) by $\{[\exp(a + b \times (MScale + \Delta Scale) - 1] - [\exp(a + b \times (MScale) - 1)]\}$.

TABLE 6

Interactive Effects of Specialization and Economies of Scale on Big N Audit Fees Dependent Variable: *LAF*

	Full Sample (2000–2007)		**Pre-SOX Period (2000–2001)**		**Post-SOX Period (2002–2007)**	
	Estimate (1)	**p-value (2)**	**Estimate (3)**	**p-value (4)**	**Estimate (5)**	**p-value (6)**
Spec	0.312	(< 0.01)	0.287	(< 0.01)	0.322	(< 0.01)
Scale	−0.024	(0.57)	−0.036	(0.54)	−0.009	(0.87)
Spec × *Scale*	−0.334	(< 0.01)	−0.386	(< 0.01)	−0.327	(< 0.01)
Intercept	1.833	(< 0.01)	1.144	(< 0.01)	1.764	(< 0.01)
LTA	0.516	(< 0.01)	0.496	(< 0.01)	0.525	(< 0.01)
LSEG	0.136	(< 0.01)	0.141	(< 0.01)	0.132	(< 0.01)
CATA	0.508	(< 0.01)	0.491	(< 0.01)	0.512	(< 0.01)
QUICK	−0.054	(< 0.01)	−0.061	(< 0.01)	−0.049	(< 0.01)
DE	0.070	(0.04)	0.134	(0.06)	0.044	(0.23)
ROI	−0.306	(< 0.01)	−0.380	(< 0.01)	−0.289	(< 0.01)
FOREIGN	0.355	(< 0.01)	0.253	(< 0.01)	0.375	(< 0.01)
OPINION	0.246	(< 0.01)	0.289	(< 0.01)	0.216	(< 0.01)
YE	−0.177	(< 0.01)	−0.090	(< 0.01)	−0.198	(< 0.01)
LOSS	0.092	(< 0.01)	0.039	(0.15)	0.118	(< 0.01)
AAClients	−0.083	(0.04)	−0.152	(0.62)	−0.085	(0.03)
CITYSIZE	0.098	(< 0.01)	0.096	(< 0.01)	0.097	(< 0.01)
Fixedeffects	Included		Included		Included	
Adjusted R^2	0.795		0.751		0.778	
n	17,207		4,235		12,972	

F-tests for difference in coefficients between the pre-SOX and post-SOX periods:
Spec: p-value = (< 0.01).
Scale: p-value = (0.79).
Spec × *Scale*: p-value = (< 0.01).
Year and industry fixed effects variables are included but not reported for brevity. p-values (two-tailed) reported in parentheses are based on robust standard errors clustered by firm.
See Table 4 for variable definitions.

The results for the full sample carry over to the pre-SOX (Table 6, Columns 3–4) and post-SOX (Columns 5–6) periods. In both periods, *Spec* is significant and positive, *Scale* is insignificant, and the interaction *Spec* × *Scale* is significant and negative. Thus, the specialization premium is significantly smaller for a larger city-industry scale, but clients enjoy no scale discounts when their auditor is not a specialist.[24]

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[24] To check whether industry leadership is an important factor in audit pricing, we exclude observations with *Spec* = 1 and define the second ranked auditor as the specialist (*Spec2*), which reduces the sample to 9,264 observations. The results are significantly weaker for *Spec2*: the coefficient drops from 0.138 (see Table 5) to 0.024, and is marginally significant ($p = 0.093$). The coefficient on *Scale* is negative and significant (coefficient = −0.180, $p < 0.01$). In the model with the interaction between *Spec2* and *Scale*, the coefficient on *Scale* is negative and significant (−0.159, $p < 0.01$), but neither *Spec2* nor the interaction term is significant. These results suggest that industry leadership is important for auditors to generate a specialization premium.

TABLE 7

Client Size, Industry Specialization, and Economies of Scale among Big N Auditors
Dependent Variable: LAF

	Below Median Client Size		Above Median Client Size	
	Estimate	p-value	Estimate	p-value
Spec	0.254	(< 0.01)	0.329	(< 0.01)
Scale	−0.079	(0.29)	0.014	(0.83)
Spec × *Scale*	−0.312	(< 0.01)	−0.330	(< 0.01)
Adjusted R^2	0.435		0.708	
n	8,604		8,603	

F-tests for difference in coefficients between below median and above median Client Size:
Spec: p-value = (< 0.01).
Scale: p-value = (0.21).
Spec × *Scale*: p-value = (< 0.01).
Client Size is measured based on client's total assets. All control variables are included but not reported for brevity. p-values (two-tailed) reported in parentheses are based on robust standard errors clustered by firm.
See Table 4 for variable definitions.

Additional Tests

City-Level Scale

Our focus above is on scale economies that arise at the city-industry level. Scale economies can also arise when an auditor expands its operations within a city, regardless of the industry.[25] In supplementary analysis, we examine the effect of these city-level scale economies, rather than the city-industry scale economies examined in our main analysis. We construct a more aggregative measure of scale, *Scalecity*, computing it at the city-level, rather than the city-industry level. The results are similar to our main findings, with the coefficient on *Spec* (*Scalecity*) being significantly positive (negative) at the 1 percent level. The specialization premium is 12.64 percent, and the decline in audit fees for a one-decile increase in scale is 2.69 percent. When we include the interaction term *Spec* × *Scalecity*, we find, as in Table 6, that the specialization premium is significantly smaller for auditors with a larger city-level scale.[26]

Differences across Client Size Groups

To assess whether our results differ across client size segments, we partition the sample into large and small client segments, using the median of *LTA* as the cutoff, and estimate the regression for each group. Table 7 shows the results for the test variables. For both size segments, *Spec* is positive, *Scale* is insignificant, and *Spec* × *Scale* is negative.[27] Thus, consistent with Table 6, specialists charge a premium that declines with *Scale*, whereas non-specialists do not offer a fee discount for increases in *Scale*. The moderating effect of *Scale* on the specialization premium is greater for large clients than for small clients ($p < 0.01$). The decline in the premium for a one-decile increase in scale is 3.37 percent (3.8 percent) for small (large) firms.

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[25] For example, city-level scale economies can arise from general investments in audit technology, such as audit software development and hardware acquisition. Similarly, human capital development (e.g., staff training) can be shared among all of a firm's clients.

[26] Although our focus is on the effects of city-level auditor specialization and scale, we also examine whether our results hold after controlling for national-level specialization and scale. We construct national specialization (*NATSPEC*) and scale (*NATSCALE*) measures, similar to our city-level measures. We find that, after controlling for *NATSPEC* and *NATSCALE*, our results for both the effects of city-industry specialization and scale continue to hold. However, we note that because we restrict our analyses to Big N clients, the national-level measures include only four or five observations in each industry. Variability in national-level scale across the Big N is low. The Big N market shares are also less variable at the national level (see footnote 2). Therefore, we do not present the tests for *NATSPEC* and *NATSCALE*.

Effects of Auditor Changes

To examine the general effects of auditor changes, we replace *AAClients* with an indicator variable that captures all auditor changes in our sample (*AuChange*). We find that our main findings hold with the inclusion of this auditor change measure. The coefficient (p-value) on *Scale* is –0.171 (< 0.01), and that on the interaction term is –0.336 (< 0.01). The coefficient on *AuChange* is negative and significant, consistent with previous studies that show price-cutting following auditor changes. Moreover, the coefficient on *AuChange* (–0.145, $p < 0.01$) is more negative than that on *AAClients* (–0.081, $p < 0.01$). This finding suggests that, although successor auditors, in general, offer discounts to new clients, this discount is smaller for ex-Arthur Andersen clients, possibly because they are perceived to be riskier (e.g., Cahan and Zhang 2006; Blouin et al. 2007; Kohlbeck et al. 2008; Landsman et al. 2009).

V. CLIENT BARGAINING POWER

In Table 8, we examine the effect of client bargaining power on the fee effects of specialization and scale. Following Casterella et al. (2004), we construct a client bargaining-power measure (*Power*) based on the relative importance (in the city) of the audit client to the auditor, measured as the percentile rank of the natural logarithm of company sales, divided by the sum of the natural logarithm of sales for all firms audited by the company's auditor at the city-industry level.

We split the sample based on the median *Power*, and present model estimates for high- and low-*Power* subsamples.[28] The results point to one interesting finding. For the sample with strong client bargaining power (Columns 3–4), *Spec*, *Scale*, and *Spec* × *Scale* are all significant. Thus, unlike the results for the full sample in Table 6, we see that both specialist and non-specialist auditors pass on a fee discount to their clients when these clients enjoy a relatively high degree of bargaining power. The specialization premium declines by about 4.2 percent for a one-decile increase in scale. For non-specialists, the audit fee declines by 1.1 percent for a one-decile increase in scale.

For the sample with weak client bargaining power (Columns 1–2), the results are similar to those in Table 6. *Spec* and *Spec* × *Scale* are positive and negative, respectively, indicating that specialist auditors enjoy a premium, but forgo some of that premium when scale economies are present. The specialization premium declines by 3.2 percent for a one-decile increase in scale. The coefficient on *Scale* is not significant, suggesting that when their clients' degree of bargaining power is relatively low, non-specialists offer no discounts even when their operations are larger in

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[27] Carson and Fargher (2007) use Australian data for 1998 (and 1999 and 2004 in sensitivity tests), and find differences in the specialization premium across client size groups. Following Carson and Fargher's (2007, Table 4, Column 5), we divide our sample into small, medium, and large firms. Our model is more complicated than theirs because we need to interact these categories with *Spec*, *Scale*, and their interaction. We find that the main effect (*Scale*) is stronger for larger firms, and the interaction terms (*Spec* × *Scale*) are negative and significant for all categories, consistent with the main findings. We obtain similar results when we construct our size categories (large, medium, and small) within industries, within cities, and within industry-city combinations.

[28] This measure captures the client's importance in the city-industry. To test the robustness of our findings, we also use total assets or audit fees to measure *Power*. The untabulated results are similar to our main findings.

TABLE 8

Effect of Client Bargaining Power on Industry Specialization and Economies of Scale among Big N Auditors

Panel A: Results Based on High and Low Bargaining Power—Dependent Variable: *LAF*

	Below Median *Power*		Above Median *Power*	
	Estimate (1)	p-value (2)	Estimate (3)	p-value (4)
Spec	0.287	(< 0.01)	0.295	(< 0.01)
Scale	0.095	(0.12)	−0.108	(< 0.01)
Spec × *Scale*	−0.283	(< 0.01)	−0.324	(< 0.01)
Adjusted R^2	0.759		0.817	
n	8,604		8,603	

F-tests for difference in coefficients between below median and above median *Power*:
Spec: p-value = (< 0.01).
Scale: p-value = (< 0.01).
Spec × *Scale*: p-value = (< 0.01).

Panel B: Pre-SOX and Post-SOX Period Results Partitioned on Bargaining Power—Dependent Variable: *LAF*)

	Pre-SOX Period (2000–2001)				Post-SOX Period (2002–2007)			
	Below Median *Power*		Above Median *Power*		Below Median *Power*		Above Median *Power*	
	Estimate (1)	p-value (2)	Estimate (3)	p-value (4)	Estimate (5)	p-value (6)	Estimate (7)	p-value (8)
Spec	0.310	(< 0.01)	0.218	(< 0.01)	0.290	(< 0.01)	0.327	(< 0.01)
Scale	−0.018	(0.83)	−0.096	(0.24)	0.133	(0.09)	−0.112	(0.03)
Spec × *Scale*	−0.231	(0.03)	−0.410	(< 0.01)	−0.262	(< 0.01)	−0.366	(< 0.01)
Adjusted R^2	0.663		0.781		0.741		0.799	
n	1,943		2,292		6,661		6,311	

F-tests for differences in coefficients between below median and above median *Power* for the pre-SOX [post-SOX] period:
Spec: p-value = (< 0.01) [(< 0.01)].
Scale: p-value = (0.23) [(0.03)].
Spec × *Scale*: p-value = (0.03) [(< 0.01)].
All control variables are included but not reported for brevity. p-values (two-tailed) reported in parentheses are based on robust standard errors clustered by year and industries.
See Table 4 for variable definitions.

scale. In addition, the coefficient on *Spec* × *Scale* is significantly more negative ($p < 0.01$) for clients with a high degree of bargaining power (−0.324) than for those with a low degree of bargaining power (−0.283).

Panel B of Table 8 reports the effect of bargaining power on the specialization premium and scale discounts separately in the pre- and post-SOX periods. For the high bargaining power sample, the results in both time periods are similar to those for the full sample in Panel A. The specialist premium is moderated by auditor scale. The premium declines by 4 and 4.1 percent in the pre- and post-SOX periods, respectively, for a one-decile increase in scale.

For the low bargaining power sample, the results for both periods indicate that the specialist premium is moderated by auditor scale. The magnitude of the moderating effect is less than that for the sample with high bargaining power: a one-decile increase in scale reduces the specialization premium by 2.8 percent and 3 percent, respectively, in the pre- and post-SOX periods. Consistent with Table 8, Panel A (Columns 1–2), the coefficient on *Scale* is insignificant in the pre-SOX period. However, in the post-SOX period (Panel B, Columns 5–6) the coefficient on *Scale* is unexpectedly positive, albeit with a marginal p-value of 0.09. In sum, specialization premiums are moderated by scale discounts in both periods and for clients with both high and low degrees of bargaining power. However, the extent of moderation is lower for the low-power subsample than for the high-power subsample. Clients who enjoy a high degree of bargaining power with non-specialist auditors obtain scale discounts in both periods.

VI. CONCLUSION

Many audit-pricing studies have documented fee premiums for industry specialist auditors, whereas a much smaller number of studies have examined and documented the discounts due to the economies of scale that accompany auditor expansion within industries.[29] In this paper, we extend the audit-pricing literature by examining auditor industry specialization premiums and scale discounts, including the way in which they are affected by client bargaining power. In line with recent developments in the literature (e.g., Ferguson et al. 2003; Francis et al. 2005), we examine city-level rather than national-level specialization and scale economies. This is particularly insightful because, although the Big N firms have large and fairly comparable national operations, their scale in individual city markets can differ significantly. Whereas previous research has examined scale economies and audit pricing for specific industries or for IPO firms, we examine a large sample of U.S. firms for a recent time period (2000–2007) in which there were significant structural changes in the audit market due to the demise of Arthur Andersen and the enactment of the Sarbanes-Oxley Act of 2002.

We document significant city-industry specialization premiums and scale discounts throughout our sample period, although the specialization premium is larger and the scale discount slightly lower in the post-SOX period than in the pre-SOX period. However, the fee effects of industry specialization and scale are highly interactive. The benefits that arise from a larger scale are passed on to clients only by specialist auditors. The clients of non-specialists do not benefit from a large scale. Further, client bargaining power plays an important role in influencing the fee effects of both specialization and scale. When clients enjoy greater bargaining power, both specialist and non-specialist auditors of a larger scale are likely to pass on the scale economy benefits to their audit clients.

The results of this study have implications for policymakers. The consolidation of audit firms from the Big 6 to the Big 5 in 1998, and then from the Big 5 to the Big 4 in 2002, has raised concerns about the lack of competition in the large-client market (GAO 2003). It is possible that this consolidation has either reduced competition and increased prices, or created efficiencies that have led to reduced prices (Sullivan 2002). Although we do not address the effect on competition of the Big 5 to Big 4 transition (see Kohlbeck et al. [2008] for further insights into the issue), our sample period covers the years both prior to and following the demise of Arthur Andersen. Our results suggest that there is some merit in the "efficiency" argument that larger firms pass on an economies-of-scale discount to their clients.

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[29] The premium is generally supported by evidence showing that industry specialization is associated with higher audit quality, using such measures of quality as discretionary accruals or earnings response coefficients (e.g., Krishnan 2003; Balsam et al. 2003; Gul et al. 2009).

Our study is subject to several limitations. First, economies of scale relate to cost savings. However, as in other studies in this area, our scale proxy is based on the number of audit clients rather than audit costs, due to the unavailability of data on the latter. Thus, we acknowledge that our conclusions concerning the effects of scale economies are drawn from indirect evidence, i.e., price rather than cost differences, and should therefore be interpreted with some degree of caution. Second, we focus only on publicly listed companies due to lack of available data on private companies. Third, we do not attempt to examine specific factors, such as audit methodology and staff size, that could potentially affect audit efficiency directly. Although many of these factors might be captured by the audit-firm scale measures used in this study, to the extent that they are not, they could contribute to the differential scale effects enjoyed by the large accounting firms. Future research should examine the impact of changes in the scale of audit firms over time (as opposed to the cross-sectional approach taken in this study) on changes in audit fees and quality.

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Identity, Incentives, and the Value of Information

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ABSTRACT: We examine the impact of identity preferences on the interrelation between incentives and performance measurement. In our model, a manager identifies with an organization and loses utility to the extent that his actions conflict with effort-standards issued by the principal. Contrary to prior arguments in the literature, we find conditions under which a manager who identifies strongly with the organization receives stronger incentives and faces more performance evaluation reports than a manager who does not identify with the organization. Our theory predicts that managers who experience events that boost their identification with the firm can decrease their effort in short-term value creation. We also find that firms are more likely to employ less precise but more congruent performance measures, such as stock prices, when contracting with managers who identify little with the organization. In contrast, they use more precise but less congruent measures, such as accounting earnings, when contracting with managers who identify strongly with the firm.

Keywords: *identity; incentives; multi-task agency; performance measure congruity; performance standards.*

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I. INTRODUCTION

Agency theory is one of the most important theoretical paradigms in accounting research (Indjejikian 1999; Lambert 2001). It portrays a model of human motivation in which individuals are opportunistic maximizers of personal utility, resulting in a divergence of interest between self-interested and cooperative behavior. The theory posits that, in general, imperfect signals and conflicting interests between organizational members will lead to efficiency losses and reduced firm value (Jensen and Meckling 1976). Research in behavioral economics, on the other hand, provides substantial evidence that people have pro-social preferences and are not purely self-interested (Camerer 2003). In particular, organizational research suggests that people can take an identity as a part of an organization that inclines them to support the goals of the organization (Akerlof and Kranton 2000; Van Maanen and Schein 1979).[1]

Psychologists refer to "identity" as both a person's choice of a social reference category, such as a group, community, or organization with which to identify as well as its "self-image" or personal mental model with respect to that social reference category (Akerlof and Kranton 2000). A person's identity becomes a source of motivation when individuals internalize behavioral prescriptions so that they become internal evaluative standards to which the individual feels committed to satisfy (Akerlof and Kranton 2010; Cialdini and Trost 1998). Violating internalized standards creates anxiety and discomfort (Akerlof and Kranton 2000) and can result in strong negative emotions such as guilt and loss of self-esteem (Shamir 1990; Tangney and Dearing 2004; Tracy et al. 2007). Consequently, people who take an identity as part of an organization perceive a corresponding duty to act in concert with the standards of the organization.

The possibility that people identify with an organization opens new avenues for firms to influence their employees' decisions. By issuing standards, an organization can appeal to the sense of duty in its members.[2] This study investigates the optimal design of standards, incentives, and information systems when managers identify with an organization. Specifically, we examine the impact of identity preferences on the interrelation among optimal standards, incentives, and performance-measure characteristics.

The business community recognizes the importance of identity as an essential determinant of work-related behavior. Recent studies indicate that the military, universities, consulting firms, and high-tech companies systematically engage in initiatives to influence employees' behavior via their identities (Akerlof and Kranton 2010; Alvesson and Robertson 2006; Gioia et al. 2010; Ravasi and Phillips 2011). West Point drills and routines "inculcate non-economic motives" into cadets to align their objectives with those of the U.S. Army (Akerlof and Kranton 2005, 9). Similarly, some consulting firms cultivate an elite identity to generate "affective commitment" and "responsible autonomy" such that consultants will work hard even without traditional forms of control (Alvesson and Robertson 2006, 214).

Given the importance of identities for organizational behavior, researchers have done surprisingly little work on the use of identities as a form of control (Anteby 2008). This question is an intriguing research venue for accounting researchers for two reasons. First, identity preferences may constitute a low-cost substitute for costly incentive and monitoring systems.[3] Second, and more important, identity preferences may have direct implications for the decision-influencing role of accounting because the value of accounting information for performance evaluation and compensation purposes may be reduced when agents exhibit strong identity preferences.

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[1] Studies of directors, accountants, investments bankers, and consultants suggest that organizational members pursue "fostered and desired occupational identities" (Anteby 2008, 202; see, e.g., Covaleski et al. 1998; Golden-Biddle and Rao 1997; Ibarra 1999). Moreover, research indicates that the strength with which people identify with an organization impacts cooperation, production, commitment, satisfaction, and turnover (Hillman et al. 2008; see, e.g., Ashforth and Mael 1989; Boivie et al. 2011; Dutton et al. 1994; Foreman and Whetten 2002; Golden-Biddle and Rao 1997).

[2] For example, at the Kellogg Graduate School of Management, the dean addresses new professors and doctoral student instructors to explain what the school considers to be appropriate behavior (Casadesus-Masanell 2004, 389). More generally, Akerlof (1982) suggests that *any* labor contract includes implicit or explicit standards.

We consider an agency setting in which a principal hires an agent or manager. The principal offers a contract to the agent that includes a standard for the agent's effort level. The agent takes an identity as a part of the organization and loses utility to the extent that his actions conflict with standards issued by the organization. Empirical studies suggest that managers differ with respect to the identity they take in an organization (Boivie et al. 2011).[4] Critical events such as corporate identity initiatives or hostile takeovers can strengthen or undermine managers' identities.[5] We account for such variance in identity by considering variations in the degree to which agents internalize their organization's standards.[6]

A manager who internalizes a standard feels distress when deviating from it. This distress arises even though the manager's effort level is non-observable. Identity preferences do not reduce the disutility of effort; rather, they motivate a manager to provide effort *despite* the disutility of effort.[7] As a consequence, the manager does not fully reap the rewards of opportunism afforded by incomplete contracts. This sharply contrasts with the behavior of a classical self-interested agent who maximizes personal utility without considering the consequences for the organization.

Our model is associated with recent economic literature on guilt aversion and integrity, which has found that individuals have a basic disposition to experience distress when "disappointing people" or "breaking their own word" (Battigalli and Dufwenberg 2007; Charness and Dufwenberg 2006). Our model is also related to recent literature on work ethics (Casadesus-Masanell 2004; Carlin and Gervais 2009; Stevens and Thevaranjan 2010) that analyzes optimal employment contracts when agents experience disutility if their effort departs from a pre-defined standard set by the principal.[8]

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[3] For example, Hillman et al. (2008, 443) argue that "the more strongly an individual identifies with the organization, the more he will work to benefit it, because he sees himself as partly defined by the organization." With respect to contracting costs, Brickley et al. (2008, 620) write that if "everyone were to voluntarily reduce opportunistic behavior . . . then the resources devoted to monitoring and enforcing exchanges could be used in more productive pursuits." In a similar vein, Akerlof and Kranton (2005, 11) state that "An employee who identifies himself as an insider in an organization needs little monetary inducement to perform a job well."

[4] Survey scales to measure organizational identification have been used and validated by Ashforth et al. (1998), Dukerich et al. (2002), and Shamir and Kark (2004), among others. Empirical studies suggest that people tend to identify more strongly with an organization the more they have "positive experiences of the organization . . . perceive that the organization possesses distinctive and positive attributes . . . perceive that the organization possesses a positive image likely to enhance their own; and . . . perceive that the organization possesses values which are positive and congruent with their own" (Empson 2004, 763–764).

[5] See Ackerlof and Kranton (2005) and Ravasi and Phillips (2011) for examples of initiatives to strengthen identity; and see Giessner et al. (2011) for examples in which takeovers undermine individual identification with the firm.

[6] Throughout the study, we assume that all actors are perfectly informed about the actors' preferences such as their risk tolerance or the extent of their identification with the organization.

[7] Therefore, the notion of identity differs from the concept of intrinsic motivation based on needs for self-actualization (Maslow 1954), competence (White 1959), personal growth (Alderfer 1972), flow stimulation (Csikszentmihalyi 1975), self-determination (Deci and Ryan 1985), or achievement (McClelland 1988). Whereas an intrinsically motivated manager derives pleasure from performing an activity, a manager motivated by identity preferences does not necessarily enjoy performing the activity *per se*. Rather, the manager is motivated by a sense of duty or obligation toward the organization.

[8] Existing studies have also considered other motives as to why people adhere to standards. Kandel and Lazear (1992), Daido (2004, 2006), and Fischer and Huddart (2008) analyze models in which social norms are enforced by peer pressure. In their settings, norms can be subject to moral hazard and can therefore influence the optimal design of an organization. Conversely, in our study, standards are set by the principal and cannot be manipulated by agents. Furthermore, agents abide by standards because they identify with an organization and not because peer pressure forces them to do so. Hence, contrary to models of social norms and peer pressure, we analyze agents who *voluntarily* act within self-imposed confines.

These studies show that, in a single-task setting, contracts that account for an agent's nonmonetary preferences generally employ weaker incentives than the standard agency model. However, the information system in these studies reflects a single exogenous performance measure. This modeling choice abstracts away the relative usefulness of different performance measures and their incentive properties for the agent. Clearly, these issues become more important in settings in which agents engage in multiple activities. We attempt to add to the literature by investigating single- and multi-task settings with information systems that can produce multiple performance measures. This allows us to analyze the relative importance of performance-measure characteristics, the optimal incentive mix for a manager, and the value of incremental decision-influencing information for contracting purposes. Our study extends prior literature by investigating the impact of a manager's identity preferences on the organization's incentive system *and* its information system.

Our main findings are that if managers face a single task, then incentive rates and the value of additional reports for performance evaluation strictly decrease in the strength with which a manager identifies with the organization. This result matches common intuition and arguments made in the literature. In a multi-task setting, however, we find conditions under which a manager who identifies strongly with the organization receives stronger incentives and faces more performance evaluation reports than a manager who identifies less with the organization.[9]

We discuss applications of our theory and derive empirical predictions for settings in which managers significantly differ in their degree of identity or experience critical events that boost or undermine their identification with the firm. Our model predicts an inverse U-shaped relation between the number of performance measures and the strength of a manager's identity. Specifically, firms that inspire a strong (but not too strong) identity are more likely to use a mix of performance measures than less inspiring firms. Furthermore, firms are more likely to employ less precise, but more congruent, performance measures (e.g., stock price) when contracting with managers who identify little with their organization, while they use more precise but less congruent measures (e.g., accounting earnings) when contracting with managers who identify strongly with the firm. Our theory also predicts that managers who experience critical events that strongly boost their identification with the firm decrease their effort aimed at short-term value creation. Firms optimally react to these large identity shifts by providing steeper incentives while re-balancing relative incentive weights from more congruent but less precise to less congruent but more precise performance measures.

Overall, our model helps explain heterogeneity in performance measurement and incentive contracting within industries as a function of the identity managers take in their firms. More generally, our findings suggest that empirical compensation studies should proxy for a manager's identity to control for endogeneity.

The remainder of the paper is organized as follows: Section II presents the basic single-task, single-performance-measure model to characterize the optimal linear contract and standard that the principal offers to the agent. It demonstrates that contract parameters are not separable from the agent's identity preferences and finally investigates the value of an additional performance measure. Section III analyzes the impact of the agent's identity preferences on optimal incentive rates, effort choice, and the stewardship value of additional performance measures in a multi-task setting. Section IV concludes.

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[9] In particular, these conditions refer to a setting in which a manager who does not identify with the organization exerts effort beyond the first-best on one task but less than first-best on the other task. In an agency setting, such conditions can arise if the performance measure is non-congruent and the agent is sufficiently risk-tolerant.

II. BASIC MODEL OF IDENTITY PREFERENCES IN A SINGLE-TASK SETTING

Agent's Action, Performance Measure, and Compensation

At date 0, the principal, acting on behalf of the firm's risk-neutral owners, hires an agent to provide personally costly effort $a \in \Re$ at date 1 in return for compensation at date 2. The agent's personal cost of effort is $\kappa(a) = ½\, a^2$. At date 0, the principal also specifies a standard, s, for the agent's effort. We assume that the agent identifies with the organization and thus internalizes the standard such that the agent suffers an additional personal cost if he deviates from the standard:

$$\kappa_s(s,a) = ½\, \gamma(s-a)^2,$$

where $\gamma \geq 0$ scales the loss of identity utility from deviating from the standard. We interpret γ as a measure of the strength with which the agent identifies with the organization.[10] Although violation of the standard is generally not observable by the principal, any deviation causes a personal cost to the agent. For simplicity, we assume that both a positive and a negative deviation from the standard result in a loss of identity utility, which increases with the level of violation.[11]

The agent's identity cost function, $\kappa_s(s, a)$, captures the notion that identity utility depends on the standard of the social reference category. When an agent identifies with an organization, he partly defines himself in terms of the organization and thus is sensitive to the externalities his actions exert on the organization. As a consequence, he perceives a duty to act in accord with the organization's standards and suffers emotional distress if he fails to do so. For such an agent, the standard defines the ideal effort level to serve the organization's best interest. Accordingly, the agent suffers a loss of identity utility because he is either "shirking" when providing effort that falls short of the standard or "ripping off the organization" when providing unnecessary but *costly* effort in excess of the standard. In the first case, the agent feels guilty because he does not contribute as is expected of him. In the second case, the agent feels guilty because he "games" the organization's compensation system by deliberately deviating from the effort the organization wants him to provide to increase his compensation.[12]

At date 2, performance measure y is publicly reported as $y = ma + \varepsilon$, where m is the report's sensitivity to the agent's effort and ε represents uncontrollable events, with $\varepsilon \sim N(0,\sigma^2)$.

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[10] Throughout the paper, we use the terms "identification with the organization" and "internalization of the standard," interchangeably. Implicitly, our analysis is restricted to a range of standards that the agent perceives to be "reasonable." In our setting, the principal sets the standard equal to the agent's first-best action, which can be regarded as a reasonable standard.

[11] Symmetric loss functions for deviations from standards are also used by Akerlof and Kranton (2005), Brueggen and Moers (2007), Casadesus-Masanell (2004), and Fischer and Huddart (2008). Furthermore, in a single-task setting, the principal will never set a standard below the agent's action, making the assumption of a symmetric loss function innocuous in the single-task setting.

[12] Examples of such reasoning include a surgeon who performs more surgery than necessary in terms of hospital-standards to increase his income, a CEO who accelerates the company's acquisition strategy because his compensation package increases in firm size, or a manager who is compensated based on annual earnings and who increases production without an according increase in sales to shift fixed costs to following periods to increase his current bonus. Aside from higher compensation costs, an agent's deviation from the standard may trigger additional costs for the firm in terms of an increased demand for coordination, slack, or even waste. These additional costs make the assumption about the agent's identity cost function more robust because an agent who identifies with the organization would feel even guiltier when purposely imposing additional costs on the firm with which he identifies. For example, if storing an excessive amount of units causes additional storage costs while increasing annual earnings, a manager feels more guilt about increasing production to boost his compensation. For simplicity, we do not explicitly consider these additional costs.

At date 0, the principal offers the agent a contract, represented by $z = (f,v,s)$, where f is the agent's fixed wage, v is the incentive rate for performance measure y, and s is the standard specified by the principal. Hence, given report y and contract z, the agent's compensation is $w(y,z) = f + vy$.

Agent's Preferences and Action Choice

We assume that the agent has a negative exponential utility function, which is multiplicatively separable in the utility from compensation, u_w, the disutility from effort, u_a, and the distress from not meeting the standard, u_s. Specifically:

$$u(z,a) = u_w(z,a)u_a(a)u_s(s,a) = -\exp[-r\Big(w - \kappa(a) - \kappa_s(s,a)\Big)],$$

where r denotes the agent's coefficient of absolute risk aversion. Maximizing the agent's expected utility is equivalent to maximizing the certainty equivalent, which is characterized by:

$$\begin{aligned} CE(z,a) &= E[w|z,a] - \kappa(a) - \kappa_s(s,a) - ½\, rVar[w|z,a] \\ &= f + v\, ma - ½\, a^2 - ½\, \gamma(s-a)^2 - ½\, rv^2\sigma^2. \end{aligned} \tag{1}$$

Differentiating (1) with respect to a, given contract z, provides the agent's incentive compatibility constraint:

$$vm + \gamma(s - a^\dagger) = a^\dagger, \tag{2}$$

where $a^\dagger$ denotes the action induced by contract z. The benefit to the agent of marginally increasing effort consists of an increase in compensation and, for $s > a$, a reduced identity utility loss because the agent's effort is closer to the standard. Hence, the agent's effort is motivated by monetary incentives, vm, and by identity incentives, $\gamma(s - a)$, which are substitutes with respect to the agent's effort choice. All else equal, Expression (2) implies that as the agent's identification with the organization increases, the standard becomes relatively more important in motivating the agent's effort.

Finally, the agent's individual rationality constraint reflects that he will only participate in the firm if his contract z provides him with his reservation certainty equivalent, which is normalized to zero:

$$CE(z, a^\dagger) \geq 0. \tag{3}$$

Principal's Preferences

We assume that the terminal value of the firm, which is not contractible, is a linear function of the agent's effort, $E[x|a] = ba$, where $b \in \Re$ is the agent's productivity. The expected net terminal value of the firm is:

$$\Pi(z,a) = E[x - w|z,a]. \tag{4}$$

In the first-best case in which the agent's action is contractible, no incentive risk is necessary to induce the agent to undertake a desired action, and it is optimal for the principal to specify first-best effort, $a^* = b$, and to set the standard equal to the first-best effort, $s^* = a^*$. Here, the optimal standard s^* does not depend on the agent's identification with the organization, γ, because the agent implements any action selected by the principal. If the principal sets $s^* = a^*$, then the agent will not suffer a loss of identity utility, implying that the agent's identity preferences are irrelevant.[13]

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[13] This result indicates that a benefit of our modeling choices is that the first-best expected net profit is not affected by the strength of the agent's identity preferences. Consequently, we can investigate the agency loss for varying degrees of identity without the need to adjust for differences in first-best profits.

Optimal Linear Contract

We now consider the impact of an agent's identity preferences under second best. The principal chooses z to maximize her expected net payoff, (4), subject to the agent's incentive compatibility and individual rationality constraints, (2) and (3). Lemma 1 characterizes the agent's effort choice and the optimal contract offered by the principal.

Lemma 1: The principal's decision problem is separable in the contract's incentive parameters, incentive rate v and standard s. The agent's effort choice, $a^\dagger$, the optimal incentive rate, $v^\dagger$, and the optimal standard, $s^\dagger$, are characterized by (proofs are in Appendix A):

$$a^\dagger(v,s) = \frac{vm + \gamma s}{1+\gamma}, \tag{5a}$$

$$v^\dagger = \frac{bm}{m^2 + r(1+\gamma)\sigma^2}, \text{ and} \tag{5b}$$

$$s^\dagger = b. \tag{5c}$$

Although the optimal incentive rate depends on the agent's internalization of the standard, γ, it is independent of the standard itself (i.e., $\partial v^\dagger/\partial\gamma \neq 0$ and $\partial v^\dagger/\partial s = 0$). From the agent's individual rationality constraint, (3), with an optimal contract, the agent's expected compensation equals the sum of effort costs, identity costs, and risk premium. Substituting the induced action $a^\dagger$, (5a), into the sum of the agent's effort costs, identity costs, and risk premium yields:

$$\kappa(a^\dagger) + \kappa_s(a^\dagger, s) + \tfrac{1}{2} rVar[w|z, a^\dagger] = \tfrac{1}{2}[(1+\gamma)^{-1}m^2 + r\sigma^2]v^2 + \tfrac{1}{2}\gamma(1+\gamma)^{-1}s^2. \tag{6}$$

Expression (6) implies that the marginal cost to the principal of setting incentives via the incentive rate v varies with the agent's identity, γ, but is independent of the standard, s. Intuitively, increasing v yields an effort that is closer to the standard. For $a < s$, this increase in the agent's effort reduces the marginal cost of deviating from the standard, weakening identity incentives. Essentially, weaker identity incentives entail an additional cost to providing monetary incentives. Given a quadratic identity cost function, while this cost increases with the degree γ with which the agent internalizes the standard, it is independent of the action's deviation from the standard.

Lemma 1 also implies that the principal does not adjust the standard based on the strength of the agent's identification with the organization.[14] Consequently, the principal sets the same standard for agents with identical payoff productivity, b, even if the agents differ with respect to their identification with the organization, their risk tolerance, or the characteristics of their performance measures.[15]

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[14] Following (6), the marginal cost to the principal of setting standard s is $s\gamma(1+\gamma)^{-1}$. Using (5a), the marginal benefit is $b\gamma(1+\gamma)^{-1}$. Equating marginal benefit and marginal cost implies that the optimal standard, $s^\dagger$, is the agent's payoff productivity, b, which equals the agent's first-best action.

[15] In Appendix B we generalize this result by showing that in a multi-task LEN setting with quadratic effort cost and identity cost functions, the optimal standards are the agent's first-best actions. Intuitively, the marginal cost to the principal of setting the standards continues to be independent from the marginal cost of providing monetary incentives. Importantly, this result is independent of the number and properties of contractible performance measures available to the principal. While we can show that all of our main results hold with an identity cost function that increases linearly in the deviation from the standard, the result of a constant standard stems from the quadratic identity cost function. The latter analysis is available from the authors upon request.

The above discussion assumes that the principal sets a standard that exceeds the agent's effort. Proposition 1 next shows that this assumption is without loss of generality.

Proposition 1: In a single-task agency setting:

(i) the principal chooses a standard that exceeds the agent's effort (i.e., $s^{\dagger} > a^{\dagger}$);
(ii) an agent who fully identifies with the firm supplies first-best effort (i.e., $\lim_{\gamma \to \infty} a^{\dagger} = b$);
(iii) an agent's effort increases in his identity (i.e., $\partial a^{\dagger}/\partial \gamma > 0$); and
(iv) identity incentives and monetary incentives are substitutes (i.e., $\partial v^{\dagger}/\partial \gamma < 0$).

Proposition 1(i) in conjunction with $s^{\dagger} = b$ illustrates that identity incentives are generally not sufficient to achieve first-best because the moral hazard problem is not resolved when the agent suffers a loss of identity utility when deviating from a standard. This implies that the principal sets a standard from which the agent will surely deviate, i.e., the principal offers a contract that imposes identity costs on the agent.

When setting the standard, the principal trades off the identity costs and the benefits of identity incentives that motivate the agent to supply more effort.[16] Part (ii) of Proposition 1 shows that if the agent fully identifies with the firm, $\gamma \to \infty$, then he chooses the first-best action and thus suffers no identity costs.

By setting $s^{\dagger} > a^{\dagger}$, the principal imposes identity costs on the agent, which increase in the agent's desire to meet the standard. To avoid a loss of identity utility, the agent works harder. In turn, the principal reduces monetary incentives because the induced identity incentives more than compensate for the reduction in monetary incentives. This result confirms the finding of prior literature that the principal reduces incentives when contracting with an agent who strongly identifies with the firm (Akerlof and Kranton 2005).

Finally, the principal's expected net profit in the second-best case is given by:

$$\Pi^{\dagger}(\eta_1) = ½ \frac{\gamma}{1+\gamma} b^2 + ½ \frac{1}{1+\gamma} \cdot \frac{b^2 m^2}{m^2 + r(1+\gamma)\sigma^2}, \tag{7}$$

where η_1 indicates an information system that generates a single performance measure, y. Using (7), it is straightforward to show that the principal benefits from contracting with an agent who identifies with the organization and that the benefit increases in the agent's identity, γ. However, except in the knife-edge case where the agent fully identifies with the firm, $\gamma \to \infty$, the principal cannot achieve first-best. Therefore, the principal may want to reduce agency costs by contracting on multiple performance measures.

Stewardship Value of Additional Performance Measures

In many organizations, a principal has access to multiple performance measures. Earlier agency literature has provided extensive characterizations of the relative incentive weights for these measures and the stewardship value of additional performance measures (Lambert 2001). We next study how these characterizations change when the agent has identity preferences.

We extend the previous model by assuming that the principal may contract on a second signal, y_e, that reports on uncontrollable events affecting signal y. More specifically, information system η_2

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[16] This result is similar to the argument that, in an adverse-selection setting, distorted performance measures may be beneficial to the principal. Following Maggi and Rodríguez-Clare (1995), given a convex cost of distortion, motivating distortion in equilibrium results in a higher marginal cost of distortion, which may provide a countervailing incentive that eases an agent's self selection. We thank Brian Mittendorf for pointing out this connection.

generates two signals, y and y_e, with $y = ma + \varepsilon$, $\varepsilon \sim N(0,\sigma^2)$, $y_e = \varepsilon_e$, $\varepsilon_e \sim N(0,\sigma_e^2)$, and $\mathrm{Cov}[y,y_e] = \rho\sigma\sigma_e$.[17] The agent's compensation is given by $w = f + vy + v_e y_e$. Lemma 2 summarizes the optimal incentive rates and the principal's expected net payoff.

> **Lemma 2:** Consider a single-task agency setting with two performance measures, y and y_e. The optimal incentive rates and the principal's expected net payoff become:

$$v^\dagger = \frac{bm}{m^2 + r(1+\gamma)(1-\rho^2)\sigma^2}, \tag{8a}$$

$$v_e^\dagger = -\rho\frac{\sigma}{\sigma_e}v^\dagger, \text{ and} \tag{8b}$$

$$\Pi^\dagger(\eta_2) = ½\frac{\gamma}{1+\gamma}b^2 + ½\frac{1}{1+\gamma}\cdot\frac{b^2m^2}{m^2 + r(1+\gamma)(1-\rho^2)\sigma^2}. \tag{8c}$$

Here the optimal standard is independent of the monetary incentives and hence is set to equal the first-best effort. By comparing the agent's performance in y with the one in y_e, the principal can filter out the impact of uncontrollable events on the agent's compensation (Antle and Demski 1988). Following (8a) and (8b), the agent's identity preferences affect the two incentive rates $v^\dagger$ and $v_e^\dagger$, such that both incentive rates decrease in the agent's identity, γ. Given that identity incentives substitute for monetary incentives, the incentive risk imposed on the agent via $v^\dagger$ decreases with γ. Consequently, less filtering via $v_e^\dagger$ is required for a stronger identification with the organization.

We define the value of an additional performance measure as the difference in expected net payoff to the principal with and without this measure. Specifically, given information system η_1 with a single performance measure, y, and information system η_2 with signals y and y_e, using (7) and (8c) the value of y_e becomes:

$$V^\dagger = \Pi^\dagger(\eta_2) - \Pi^\dagger(\eta_1) = ½\frac{b^2m^2r\rho^2\sigma^2}{(m^2 + r(1+\gamma)\sigma^2)(m^2 + r(1+\gamma)(1-\rho^2)\sigma^2)}. \tag{9}$$

Proposition 2 summarizes the consequences of varying strength of the agent's identity on the optimal incentive ratio, $IR^\dagger = v_e^\dagger / v^\dagger$, and the value of an additional performance measure, $V^\dagger$.

> **Proposition 2:** Consider a single-task agency setting with two performance measures:
>
> (i) the ratio of the two incentive rates is independent of the agent's identity (i.e., $\partial IR^\dagger/\partial\gamma = 0$); and
>
> (ii) the value of an additional performance measure is decreasing in the agent's identity (i.e., $\partial V^\dagger/\partial\gamma < 0$).

Proposition 2(i) implies that both incentive rates decrease at the same rate when the agent identifies more strongly with the organization. The key to this result is that optimal risk filtering via $v_e^\dagger$ is directly proportional to the risk imposed on the agent via $v^\dagger$ from (8b). Hence, while the relevance of monetary incentives declines with an agent's identification with the organization, the relative importance of an additional signal on uncontrollable events is independent of the agent's identity.

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[17] Note that assuming y_e is not action-informative, $\partial E[y_e]/\partial a = 0$, is without loss of generality. We can always transform any signal y_a with $\partial E[y_a]/\partial a \neq 0$, into y_a', where $\partial E[y_a']/\partial a = 0$, such that (y,y_a') is an equivalent statistic for (y,y_a).

By filtering out the impact of uncontrollable events on an agent's compensation, the principal reduces the agent's risk premium. Consequently, stronger monetary incentives are more attractive to the principal. On the other hand, a stronger identification with the organization entails weaker monetary incentives, which reduces the value of an additional signal that can be used to filter out the impact of uncontrollable events.

For the single-task setting, our result matches the common intuition that identity incentives and monetary incentives are substitutes. Moreover, while the relative incentive weights placed on the measures are unaffected by an agent's perceived pressure to meet the standard, the value of an additional performance measure decreases with an agent's identity. Regarding the design of performance measurement systems, the latter results suggest that empirical studies on the relative importance of performance signals for incentive purposes do not need to control for an agent's identity when the set of measures is held constant. However, the set of performance measures decreases in an agent's identity, suggesting that neglecting an agent's identity in empirical studies introduces the possibility of correlated omitted variables.

III. IDENTITY PREFERENCES IN A MULTI-TASK SETTING

In this section, we extend our initial analysis to a multi-task setting. In multi-task agency settings, performance measure noncongruity introduces an additional efficiency loss to the principal (Feltham and Xie 1994). Accordingly, in this section, we explore the impact of an agent's identity on the interrelation between monetary incentives and performance measurement when an agent is responsible for multiple tasks with a single performance measure and multiple performance measures.

Two-Task Setting with a Single Performance Measure

Optimal Linear Contract

The impact of an agent's identity preferences becomes more subtle if the agent is responsible for multiple tasks. The agent now controls two tasks, a_1 and a_2, at cost $\kappa(a_1,a_2) = ½(a_1{}^2 + a_2{}^2)$. The expected gross value of the firm is represented by $E[x|a_1,a_2] = b_1a_1 + b_2a_2$. As in the previous section, we initially consider an information system, η_1, that generates a single performance measure, $y = m_1a_1 + m_2a_2 + \varepsilon$, with $\varepsilon \sim N(0, \sigma^2)$.

The principal specifies a standard, s_i, for each action a_i, $i = 1,2$. To keep the analysis simple while retaining the same model structure, we assume that the agent's disutility of deviating from the standards is homogeneous, such that the agent has no direct preference for a specific standard, and separable, such that compliance with one standard does not affect the cost of deviating from the other standard:

$$\kappa_s(s_1, s_2, a_1, a_2) = ½\gamma\left\{(s_1 - a_1)^2 + (s_2 - a_2)^2\right\}.$$

Using this parametrization, Lemma 3 characterizes the optimal contract and the principal's net payoff.

Lemma 3: Given a two-task agency setting with a single performance measure, the optimal standards, incentive rate, and the principal's expected net payoff become:

$$s_1^\dagger = b_1 \text{ and } s_2^\dagger = b_2, \tag{10a}$$

$$v^{\dagger} = \frac{b_1 m_1 + b_2 m_2}{m_1^2 + m_2^2 + r(1+\gamma)\sigma^2}, \text{ and} \tag{10b}$$

$$\Pi^{\dagger}(\eta_1) = \frac{1}{2}\frac{\gamma}{1+\gamma}(b_1^2 + b_2^2) + \frac{1}{2}\frac{1}{1+\gamma} \cdot \frac{(b_1 m_1 + b_2 m_2)^2}{m_1^2 + m_2^2 + r(1+\gamma)\sigma^2}. \tag{10c}$$

Several insights from the single-task setting regarding the impact of identity preferences remain valid for the two-task setting. For example, the standards are set equal to the first-best action, the incentive rate decreases with an agent's identification with the organization, γ, and the principal's expected net payoff increases with γ. However, while an agent's effort increases with γ in the single-task setting, the relation is more subtle in a multi-task setting.

Identity Preferences and Induced Effort

To understand the consequences of an agent's identity preferences in a multi-task setting, we first analyze the relative effort levels that can be induced by the principal. If there are two tasks, then the first-best effort ratio is $ER^* = a_1^*/a_2^* = b_1/b_2$. Under second best, the agent's effort allocation is characterized by:

$$ER^{\dagger}(v, s_1, s_2) = \frac{a_1^{\dagger}(v, s_1)}{a_2^{\dagger}(v, s_2)} = \frac{m_1 v + \gamma s_1}{m_2 v + \gamma s_2}. \tag{11}$$

When the agent does not identify with the organization, $\gamma = 0$, the effort ratio reflects the signal's sensitivity to the agent's actions (i.e., $ER^{\dagger} = m_1/m_2$ if $\gamma = 0$). Thus, the performance measure limits the inducible effort ratio such that a congruent performance measure, $m_1/m_2 = b_1/b_2$, is a necessary and sufficient condition for the principal to implement the first-best effort ratio. In contrast, when the agent identifies with the organization, $\gamma > 0$, the principal can induce any pair of actions. However, by setting optimal standards the principal does not, in general, induce the first-best effort ratio.[18] Specifically, using (10a) and (10b) in (11), the principal chooses to implement an effort ratio $ER^{\dagger}$ that reflects the agent's identity as well as the agent's risk aversion, the noisiness of the performance measure, and the agent's payoff productivity in both tasks. With a noncongruent performance measure, $m_1/m_2 \neq b_1/b_2$, identity incentives and monetary incentives induce divergent effort allocations. While identity incentives induce a first-best effort allocation such that $ER^{\dagger} = s_1^{\dagger}/s_2^{\dagger} = b_1/b_2$ if $v = 0$, monetary incentives induce an effort allocation along the sensitivity of the performance measure (i.e., $ER^{\dagger} = m_1/m_2$ if $\gamma = 0$), and the relative strength of monetary incentives varies with the incentive rate from (10b).

Considering the influence of identity preferences on an agent's effort in a multi-task setting, similar to the single-task setting, the agent's effort in each task is driven by identity and monetary incentives. While identity incentives increase in an agent's identity, monetary incentives decrease in γ. In a single-task setting, the first effect dominates, implying that an agent's effort increases with γ. Proposition 3 illustrates that the relation is more subtle in a multi-task setting.[19]

Proposition 3: In a two-task agency setting, the impact of an agent's identity on the effort induced by the principal is ambiguous. Necessary and sufficient conditions for a negative impact of identity on the effort for task i are:

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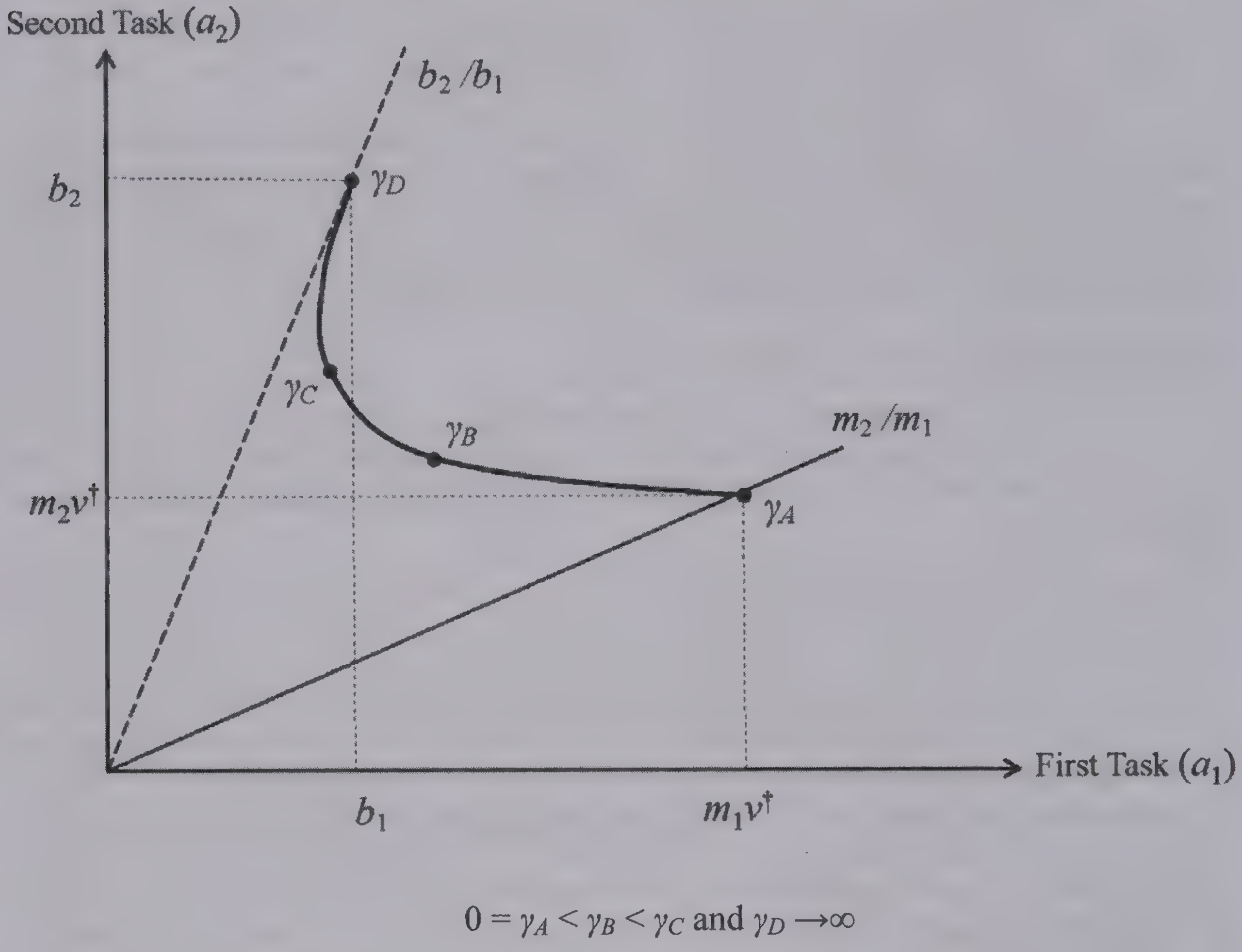
[18] The first-best effort ratio is achieved only for knife-edge cases where the performance measure is congruent with the principal's payoff, the principal does not offer monetary incentives, or the agent fully identifies with the organization.

[19] For expositional brevity, we characterize all cutoff values, such as r_c and γ_c in the respective proofs.

FIGURE 1
Impact of an Agent's Identity on his Effort Allocation



(i) the performance measure places a relatively larger weight on task i as compared with task l (i.e., $m_i/m_l > b_i/b_l$, $i, l = 1,2$ and $i \neq l$);
(ii) the agent is sufficiently risk tolerant (i.e., $r < r_c$); and
(iii) the agent only weakly identifies with the firm (i.e., $\gamma < \gamma_c$).

Proposition 3 is illustrated in Figure 1. The solid line depicts the agent's effort allocation for varying degrees of identity, with γ increasing from γ_A to γ_D. Intuitively, under the conditions outlined in Proposition 3(i) and (ii), the second-best effort exceeds the first-best effort for one of the agent's tasks. In Figure 1, at γ_A, while the agent's effort in task 2 is below the standard $s_2^\dagger$ (i.e., $a_2^\dagger < b_2$ if $\gamma = 0$), the effort in task 1 exceeds the standard $s_1^\dagger$ (i.e., $a_1^\dagger > b_1$ if $\gamma = 0$). Applying the logic of Proposition 1(ii) to a multi-task setting, the agent supplies first-best effort if he fully identifies with the firm (i.e., $a_i^\dagger = b_i$ at $\gamma_D \to \infty$, $i = 1,2$). For a stronger identity, e.g., when increasing γ from γ_B to γ_C, the agent's effort allocation "moves closer" to first-best. Then, following the intermediate value theorem, the effort in one task necessarily decreases in the agent's identity.

A negative impact of identity on effort can also apply in settings where the agent's effort in his multiple tasks is below the standards, but the effort allocation is skewed toward one of the tasks.

Such a situation is depicted at γ_C, where $a_1^{\dagger} < b_1$ but, at the same time, $\partial a_1^{\dagger}/\partial\gamma < 0$. Recall that identity incentives increase in an agent's identity and the principal optimally reduces monetary incentives for higher γ. If the agent's risk aversion is neither too low nor too high, then this reduction of monetary incentives can dominate the increase in identity incentives.[20]

To summarize, in a multi-task setting, the impact of an agent's identity on effort is ambiguous. If effort is very hard to motivate with actions well below the standards, then an agent's effort in each task increases in his identity. Otherwise, the relation between an agent's effort and his identification with the organization can be U-shaped such that the effort first decreases in the agent's identity but then increases once the identity is sufficiently strong.

Our findings have interesting implications for an organization that ties incentive compensation to a single performance measure such as accounting earnings. If the measure is inherently noncongruent and does not reflect a manager's contribution to firm value adequately, a firm risks inducing an unbalanced allocation of effort across tasks. For example, activities aimed at increasing short-term value are generally assumed to be better reflected in performance measures than those aimed at increasing long-term value (Horngren et al. 2010; Ittner and Larcker 2003).[21] Recently, identity initiatives that foster employees' identification with their organization have been suggested as a remedy to the effort-allocation problem (Alvesson and Robertson 2006; Alvesson and Willmott 2002; Ravasi and Phillips 2011). Our model predicts that successfully implemented identity initiatives in firms that inspired a low identification before the initiative can undermine (boost) managers' effort in short-term (long-term) value generation. As a consequence, when short-term performance is critical to firm survival, e.g., due to short-term liquidity issues, this effect may constitute an important boundary condition to fostering identification in practice.

Accordingly, our model suggests that over the short run, a firm's performance measures may provide contrarian signals about the economic success of an identity initiative. This can trigger nontrivial costs for the firm if analysts and investors misinterpret a reduction in short-term performance as an indication that the initiative was not successful or even detrimental to firm value. Thus, our model also highlights the potential for the performance measurement system to reflect the economic success of identity initiatives only after a significant time lag.

Our theory further predicts that events that strongly weaken the identification with the firm,[22] can boost (undermine) employees' effort for tasks that are well (not well) reflected in the firm's performance measurement system. For example, after a merger employees of an acquired firm often identify less with their new organization than the employees of the acquirer (Giessner et al. 2011; Ullrich and van Dick 2007). Here our theory predicts that employees of the acquired firm will exert more effort on tasks that the new organization's performance measurement system measures well and provide less effort on tasks that are hard to measure relative to employees of the acquiree. Consequently, after a merger, employees of an acquired firm tend to engage more in short-term than in long-term value generation. More generally, our model predicts cross-sectional variation across firms with respect to the effort that managers exert on short-term versus long-term value creation as a function of their identification with the organization.

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[20] More precisely, $\exists\, r \in (r_u, r_c)$, where r_u yields $a_i^{\dagger}|_{\gamma=0} = b_i$, $i = \{i | m_i/m_l > b_i/b_l,\ i, l = 1,2 \text{ and } i \neq l\}$, and r_c is given in the proof to Proposition 3, such that $a_i^{\dagger} < b_i$ and $\partial a_i^{\dagger}/\partial\gamma < 0$ for γ is sufficiently small. This result shows that our assumption of an identity cost function where the agent experiences distress when he supplies effort above the standard is not a necessary condition for the ambiguous impact of identity on effort.

[21] In our model, one could think of a_1 as reflecting short-term effort and a_2 as reflecting long-term effort. Consequently, short-term effort being better reflected in the performance measure implies $m_1/m_2 > b_1/b_2$.

[22] Such events could include fear of downsizing, dim career perspectives, corporate restructuring, mergers and takeovers, strategic change, or continuing losses (Petriglieri 2011; Terry et al. 2001; Ullrich and van Dick 2007).

Identity Preferences and Performance Measure Choice

In this subsection, we consider the impact of identity preferences on the relative importance of precision and congruity, two key indicators of the contracting usefulness of performance measures. For the two-task setting, the expected net payoff to the principal under second best is given by (10c), and the first-best expected net payoff is $\Pi^* = ½(b_1{}^2 + b_2{}^2)$. Therefore, the loss of surplus to the principal is:

$$L = \Pi^* - \Pi^\dagger = ½\frac{\dfrac{(b_2m_1 - b_1m_2)^2}{(1+\gamma)} + r(b_1^2 + b_2^2)\sigma^2}{m_1^2 + m_2^2 + r(1+\gamma)\sigma^2}. \tag{12}$$

According to Feltham and Xie (1994), $(b_2m_1 - b_1m_2)^2$ reflects the loss due to performance measure noncongruity, while the second term in the numerator of (12) reflects the loss due to performance measure noise. The total loss of surplus decreases in an agent's identity, γ. More importantly, Expression (12) suggests that γ affects the relative size of the two types of losses, in turn affecting the relative importance of performance measure congruity and precision for the solution of the agency conflict.

Proposition 4: In a two-task agency setting with a single performance measure, the loss due to noncongruity relative to the loss due to performance measure noise decreases with an agent's identification with the organization.

Key to Proposition 4 is that identity incentives induce the first-best effort allocation and substitute for monetary incentives. Thus, the second-best effort allocation induced by a noncongruent performance measure becomes less relevant for total effort allocation when an agent more strongly identifies with the organization.

Proposition 4 shows that identity preferences affect a principal's choice among performance measures. To illustrate this impact, consider a setting in which the principal may choose between two information systems, $\eta_1{}'$ and $\eta_1{}''$. While $\eta_1{}'$ discloses a congruent report $y_1{}'$, signal $y_1{}''$ of $\eta_1{}''$ is noncongruent but more precise. Specifically, $y_1{}' = b_1a_1 + b_2a_2 + \varepsilon_1$, with $\varepsilon_1 \sim N(0,\sigma_1{}^2)$, and $y_2{}'' = m_1a_1 + m_2a_2 + \varepsilon_2$, with $\varepsilon_2 \sim N(0,1)$, $\mathrm{Var}[y_1{}'] = \sigma_1{}^2 > \mathrm{Var}[y_1{}''] = 1$, and $m_1/m_2 \neq b_1/b_2$.

Using this parametrization in (10c) yields the principal's expected net payoffs $\Pi^\dagger(\eta_1{}')$ and $\Pi^\dagger(\eta_1{}'')$. The principal strictly prefers $\eta_1{}'$ over $\eta_1{}''$ if $\Pi^\dagger(\eta_1{}') > \Pi^\dagger(\eta_1{}'')$. This condition is equivalent to:

$$\sigma_1^2 < \sigma_c^2 \equiv \frac{(b_1^2 + b_2^2)\left((b_1m_2 - b_2m_1)^2 + r(1+\gamma)(b_1^2 + b_2^2)\right)}{r(1+\gamma)(b_1m_1 + b_2m_2)^2}, \tag{13}$$

where $\sigma_c{}^2$ equates $\Pi^\dagger(\eta_1{}')$ and $\Pi^\dagger(\eta_1{}'')$. Intuitively, the principal prefers the congruent system, $\eta_1{}'$, over the noncongruent system, $\eta_1{}''$, if the congruent measure is not too noisy. From (13), the precision of y_1 that is necessary for the principal to prefer $\eta_1{}'$ over $\eta_1{}''$ increases with an agent's identity (i.e., $\partial\sigma_c{}^2/\partial\gamma < 0$).

This section shows that, when organizations employ a single performance measure, the more employees identify with the firm, the less important is the measure's congruity relative to its precision. Therefore, our model predicts that firms are more likely to employ less precise but more congruent performance measures, such as stock prices, when contracting with managers who identify very little with their organization, while they will use more precise but less congruent measures, such as accounting earnings, when contracting with managers who strongly identify with their firm. This prediction is in accord with insights about incentive contracts of newly appointed CEOs who were either promoted from inside or hired from outside the firm (Bouwens and Kroos

2010). We would argue that managers who are promoted from the inside identify more strongly with the firm than managers who are hired from the outside.[23] As a consequence, it seems important to provide newly hired CEOs from the outside with congruent incentives, even if those incentives may rely on relatively noisy measures. In line with our predictions, Bouwens and Kroos (2010) find that incentives for new "outside" CEOs are based less on accounting earnings and more on stock market returns when compared with incentives for CEOs who were promoted from inside the firm.

Additional Performance Measures in a Multi-Task Setting

Optimal Linear Contract

To demonstrate how identity preferences affect the relative weights placed on performance measures and the value of additional performance measures in a multi-task setting, we extend the previous analysis by assuming that information system η_2 generates two performance measures, y and y_a. While y is an aggregate measure of the agent's effort in each task, a_1 and a_2, assume that y_a only captures effort for task a_1. Specifically, $y = m_1 a_1 + m_2 a_2 + \varepsilon$, $y_a = m_a a_1 + \varepsilon_a$, $\varepsilon \sim N(0,\sigma^2)$, $\varepsilon_a \sim N(0,{\sigma_a}^2)$, and $\mathrm{Cov}[\varepsilon,\varepsilon_a] = 0$. With η_2, the agent's compensation is given by $w = f + vy + v_a y_a$.

Using this parametrization, Lemma 4 characterizes the optimal contract and the principal's net payoff.

Lemma 4: In a two-task agency setting with two performance measures, y and y_a, the optimal standards, incentive rates, and the principal's expected net payoff become:

$$s_1^\dagger = b_1, \text{ and } s_2^\dagger = b_2, \tag{14a}$$

$$v^\dagger = D^{-1}\left[\left(m_a^2 + r(1+\gamma)\sigma_a^2\right)(b_1 m_1 + b_2 m_2) - b_1 m_1 m_a^2\right], \tag{14b}$$

$$v_a^\dagger = D^{-1}\left[-m_1 m_a (b_1 m_1 + b_2 m_2) + \left(m_1^2 + m_2^2 + r(1+\gamma)\sigma^2\right) b_1 m_a\right], \text{ and} \tag{14c}$$

$$\begin{aligned}\Pi^\dagger(\eta_2) &= ½\frac{\gamma}{1+\gamma}(b_1^2 + b_2^2) \\ &\quad + ½\frac{1}{1+\gamma} D^{-1}\left[(b_1^2 + b_2^2) m_2^2 m_a^2 + r(1+\gamma)\left(b_1^2 m_a^2 \sigma^2 + (b_1 m_1 + b_2 m_2)^2 \sigma_a^2\right)\right],\end{aligned} \tag{14d}$$

where:

$$D = \left(m_1^2 + m_2^2 + r(1+\gamma)\sigma^2\right)\left(m_a^2 + r(1+\gamma)\sigma_a^2\right) - m_1^2 m_a^2.$$

Recall that, in a single-task setting, the incentive weight assigned to either a single or multiple performance measures decreases with an agent's identity, γ. Likewise, in a multi-task setting, the incentive weight for a single performance measure decreases with γ. To the contrary, in a multi-task setting with multiple performance measures, the impact of an agent's identity on the incentive rates is more subtle. Moreover, while the ratio of incentive rates, $IR^\dagger = v^\dagger / {v_a}^\dagger$, is independent of an agent's identity in a single-task setting, it varies in a nontrivial way with γ in a multi-task setting.

Proposition 5: In a two-task agency setting with two performance measures, y and y_a:

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[23] See also Boivie et al. (2011) for a similar argument.

(i) the impact of an agent's identity on the incentive rates is ambiguous. Specifically, when an agent weakly identifies with the firm, the incentive rates increase in an agent's identity (i.e., $\partial v^{\dagger}/\partial\gamma > 0$ for $\gamma < \gamma'$ and $\partial v_a{}^{\dagger}/\partial\gamma > 0$ for $\gamma < \gamma''$); and

(ii) for an increasing identity, the principal assigns a relatively larger weight on performance measure y_a as compared with measure y if the latter is biased toward measuring task 1 (i.e., $\partial IR^{\dagger}/\partial\gamma < 0$ if $m_1/m_2 > b_1/b_2$).

Proposition 5 demonstrates how the triangular relation between managerial identity, performance measure congruity, and the principal's calibration of incentive rates to managerial identity and performance-measure congruity influences an agent's effort choice. The ambiguous impact of identity preferences on monetary incentives is driven by two disparate, though intertwined, effects. First, absent identity preferences, noncongruent performance measures impose a discount on incentives and thus tend to flatten incentives as compared with more congruent performance measures (Feltham and Xie 1994). Second, stronger identity preferences tend to reduce the bias imposed by noncongruent performance measures such that, for a given level of monetary incentives, stronger identity preferences reduce the "noncongruity discount" on monetary incentives. The reduced discount can make the use of stronger monetary incentives more attractive for the principal. Therefore, to improve a manager's effort allocation over multiple tasks, the principal will offer stronger incentives to managers who partially identify with the organization.

While the impact of an agent's identity on the incentive ratio is generally ambiguous, Proposition 5(ii) characterizes conditions where the principal assigns, for increasing γ, a relatively larger weight on y_a as compared with y. In a multi-task setting with non-aligned performance measures, varying the incentive ratio induces a variation in the agent's effort allocation. Specifically, for the conditions characterized in Proposition 5(ii), the principal places a relatively larger weight on performance measure y_a, which captures task 1, even though performance measure y is (also) biased toward measuring task 1. Again, the intuition for this result follows from the impact of identity preferences on the noncongruity discount on incentives.

The results of this section indicate that, when firms use multiple performance measures for incentive compensation, the level of a manager's identification with the firm influences both the absolute intensity of the manager's incentives and the relative incentive weights assigned to the different measures.[24] Employing a mix of performance indicators inevitably involves the use of some measures that are more congruent than others. Our model provides insight concerning how to choose the optimal intensity of the manager's incentives and how to re-balance the incentive weights assigned to more or less congruent performance measures as a function of the manager's identification with the firm. For example, many firms base their incentive compensation on some mix of stock price and accounting earnings. Our model predicts that the more managers identify with their organization, the more firms will put a higher weight on accounting earnings *vis-à-vis* stock prices in managers' incentives. Furthermore, we expect identity initiatives in low-identification firms to be associated with steeper incentives and a re-balancing of incentive weights from stock price to accounting earnings.

Stewardship Value of Additional Performance Measures

The expected net payoff to the principal increases as agents identify more strongly with the firm. In a single-task setting, the increase in net payoff with an information system that generates a single performance measure is larger than the increase with an information system that generates two performance measures. Consequently, the stewardship value of an additional performance measure decreases with an agent's identity, implying that a principal will use fewer performance measures when contracting with an agent who identifies more strongly with the firm.

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[24] More generally, our study suggests that incentive weights on performance measures vary non-trivially with a manager's "pro-social" or "other regarding preferences." Relatedly, Bouwens and van Lent (2010) find that a measure of group conformity asymmetrically influences the incentive weights on financial and nonfinancial measures of performance.

In a multi-task setting, given that information system η_1 generates y and information system η_2 generates y and y_a, using (10c) and (14d), the value of y_a, $V^\dagger = \Pi^\dagger(\eta_2) - \Pi^\dagger(\eta_1)$, is characterized by:

$$V^\dagger = ½(1+\gamma)^{-1}\left(m_1^2 + m_2^2 + r(1+\gamma)\sigma^2\right)^{-1} D^{-1}[m_2(b_1 m_2 - b_2 m_1) - r(1+\gamma)\sigma^2 b_1]^2 m_a^2. \tag{15}$$

Proposition 6 shows that, in a multi-task setting, the value of an additional performance measure does not necessarily decrease in how strongly the agent identifies with the firm, suggesting an overall ambiguous relation between the number of signals used for performance evaluation and an agent's identification with the firm.

Proposition 6: In a two-task agency setting with two performance measures, y and y_a, the impact of an agent's identification with the firm on the value of an additional signal, y_a, is, in general, ambiguous.

(i) For a risk-neutral agent, the value of an additional performance measure decreases with the agent's identity (i.e., $\partial V^\dagger/\partial\gamma < 0$ if $r \rightarrow 0$);
(ii) For an extremely risk-averse agent, the value of an additional performance measure is unaffected by the agent's identity (i.e., $\partial V^\dagger/\partial\gamma = 0$ if $r \rightarrow \infty$); and
(iii) A sufficient condition for a positive impact of an agent's identity on the value of an additional performance measure is that the initial performance measure, y, is sufficiently misaligned (i.e., $m_1/m_2 - b_1/b_2$ is sufficiently positive).

The key to Proposition 6(i) is that the principal can use the two performance measures generated by η_2 to construct an "overall" performance measure, which is congruent with her expected payoff. Agent risk-neutrality implies that, despite the noisiness of this constructed performance measure, the principal can still achieve first-best. Since $\Pi^\dagger(\eta_1)$ increases in γ, $V^\dagger$ decreases in the agent's identity. Intuitively, for a risk-neutral agent, efficiency losses relate only to performance measure noncongruity, and the relevance of this efficiency loss decreases with γ. On the other hand, for an extremely risk-averse agent, the principal refrains from offering monetary incentives. Under η_1 and η_2, the agent's effort is driven only by identity incentives, implying that the value of generating an additional performance measure is zero.

Proposition 6(i) and (ii) suggest that the value of an additional performance measure, $V^\dagger$, may increase in an agent's identity only for a sufficient risk tolerance (i.e., $\partial V^\dagger/\partial\gamma > 0$ only if $r \in (0, r_{max})$). Proposition 6(iii) states a sufficient condition for a positive impact of γ on $V^\dagger$ that complements the necessary condition on the risk tolerance.[25]

The key to Proposition 6(iii) is that the principal trades off marginal costs and benefits from contracting on the additional measure y_a. Assuming that the initial performance measure y is biased toward task 1, $m_1/m_2 > b_1/b_2$, a positive incentive weight on y_a increases the incentives for the agent to deviate further from the first-best effort allocation as it shifts the incentives toward task 1. However, as the two performance measures are not correlated, an increase in v_a enables the principal to decrease v and thus reduce the risk imposed on the agent. In turn, a negative incentive weight on y_a has the opposite effect, because it partly removes the incongruence associated with the initial measure. This allows the principal to increase v accordingly, thereby increasing the risk

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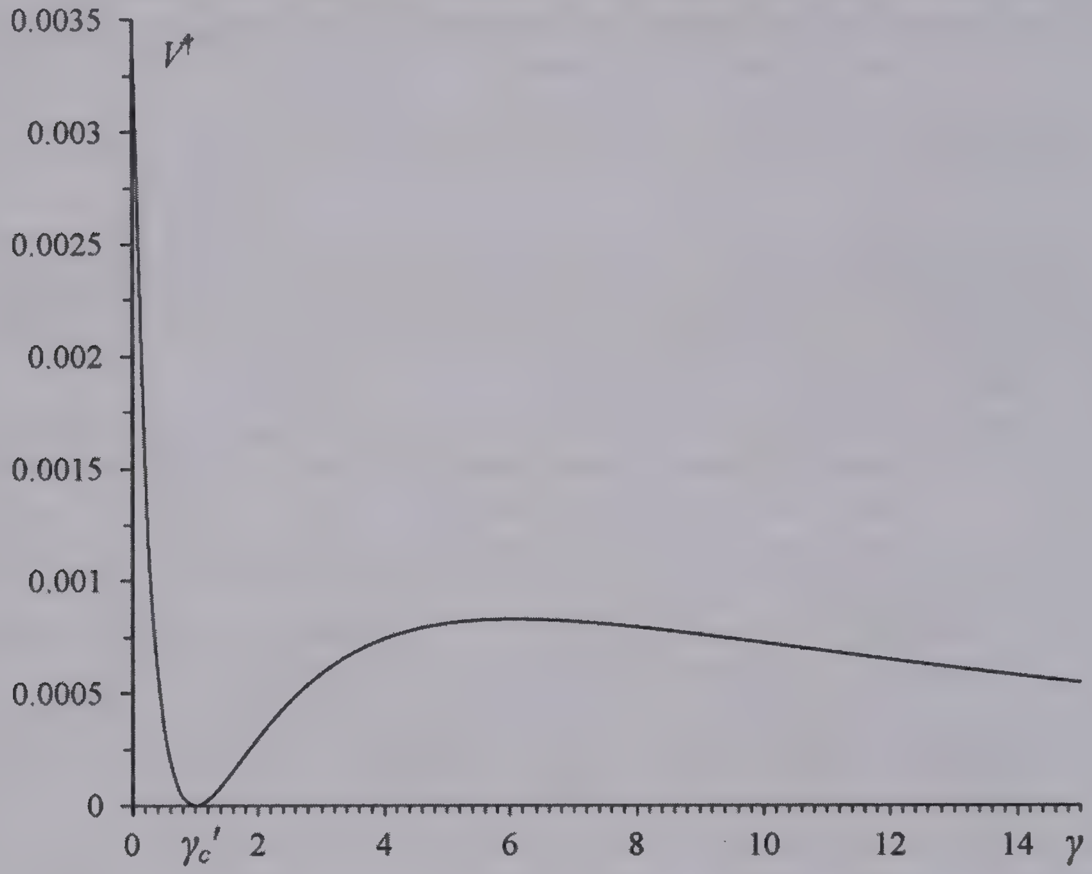
[25] While the necessary condition of intermediate levels of risk tolerance is straightforward from Proposition 6(i) and (ii), characterizing this interval requires solving a fourth-order polynomial.

FIGURE 2

Impact of the Agent's Internalization of the Standard, γ, on the Value $V^{\dagger}$ of Generating an Additional Performance Measure in a Two-Task Setting



imposed on the agent. While the principal faces a trade-off between noncongruence and imposed risk even in the absence of identity preferences, the analysis in the section "Identity Preferences and Performance Measure Choice" indicates that, the stronger the agent's identity, the less important is the congruity of a performance measure to evaluate the agent. Thus, when the initial measure is biased toward task 1 and the agent is sufficiently risk-tolerant, absent identity preferences, the principal chooses $v_a^{\dagger} < 0$ to reduce the noncongruence associated with the initial performance measure. As γ increases, reducing the noncongruence becomes less important such that $v_a^{\dagger}$ approaches zero and, for some γ, the principal will choose $v_a^{\dagger} = 0$.[26] If γ increases further, then the noncongruence induced by monetary incentives becomes less consequential such that the principal uses the additional measure to reduce the induced risk imposed on the agent by setting $v_a^{\dagger} > 0$ while also reducing $v^{\dagger}$. Importantly, $V^{\dagger} = 0$ if $v_a^{\dagger} = 0$, implying an ambiguous relation between $V^{\dagger}$ and γ.

Figure 2 illustrates Proposition 6(iii) for parameter values $b_1 = b_2 = 1$, $m_1 = 3$, $m_2 = m_a = 1$, $r = 1$, and $\sigma = \sigma_a = 1$. Following (14c), $v_a^{\dagger} = 0$ for $\gamma = \gamma_c' = (m_2/b_1)(b_2 m_1 - b_1 m_2)/(r\sigma^2) - 1$, implying that $V^{\dagger} = 0$ if $\gamma = \gamma_c'$. For $\gamma < \gamma_c'$, the principal selects $v_a^{\dagger} < 0$ to construct an overall performance measure that is more congruent with her payoff. Here, the value of an additional measure decreases in γ because identity preferences reduce the need for the principal to construct a more congruent overall performance measure. For $\gamma > \gamma_c'$, the principal selects $v_a^\dagger > 0$. While this yields an overall performance measure that is even less congruent with her payoff than y, the relatively strong identity incentives impair the agent's monetary incentives to select an effort allocation that differs substantially from first-best. In turn, choosing a positive incentive rate $v_a^\dagger$ enables the principal to reduce the incentive rate $v^\dagger$ for performance measure y, resulting in a beneficial reduction of the agent's risk premium. Finally, for sufficiently high γ (for $\gamma > 6.04$ in the example), identity incentives dominate the agent's action choice, monetary incentives are relatively less important, and the value of an additional performance measure decreases in the agent's identity.

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[26] More generally, using (14c), the principal chooses $v_a^{\dagger} = 0$ if $-r\sigma^2 + m_2/b_1(1+\gamma)^{-1}(b_2 m_1 - b_1 m_2) = 0$, where the first term reflects the marginal cost from changing the agent's risk premium and the second term reflects the marginal benefit from reducing the loss due to non-congruity. It follows that $m_1/m_2 > b_1/b_2$ is a necessary condition for $v_a^{\dagger} = 0$.

The findings in this section pertain to a firm's decision whether to tie incentive compensation to the single most congruent measure available or to employ a mix of more or less congruent performance indicators. As shown, the solution to this question depends on the level of the manager's identification with the firm and the diversity of tasks assigned to the manager. Unlike in the single-task setting, the stewardship value of the additional measure is not unambiguously decreasing in the manager's identification with the firm. In particular, our analysis indicates that firms with an intermediate level of identification are more likely to apply some mix of performance measures than low-identification firms. The intuition for this result follows our previous finding that the congruity of a performance measure *vis-à-vis* its precision becomes less important in a multi-task setting, the more a manager identifies with his firm. More generally, given that managers vary considerably with respect to how strongly they identify with firms (Boivie et al. 2011), our findings provide a possible explanation for the observed heterogeneity in the number and type of performance measures used within industries.

IV. CONCLUDING REMARKS

This study extends a LEN model to allow a risk- and effort-averse agent to identify with an organization such that the agent's utility declines as his actions deviate from effort-standards set by the principal. We analyze the impact of the agent's identity preferences on optimal performance standards, incentive rates and ratios, the agent's effort, and the value of information for performance evaluation and compensation.

Our model yields comparative statics that provide predictions concerning how additional factors will affect performance-contingent compensation. In practice, managers vary considerably with respect to how strongly they identify with their organization (Boivie et al. 2011). For example, a manager's identification is likely to increase in his job tenure as managers adopt organizational standards to reduce cognitive dissonance and emotional distress rather than leaving the organization. We also expect identification to be increasing in the strength and the saliency of an organization's ideology or culture as managers self-select into organizations whose values and standards they share and in the resources an organization spends to influence its members' identities. Moreover, critical events such as corporate identity initiatives or hostile takeovers can boost or undermine managers' identity. Thus, firm owners face an additional reason to customize contracts to managerial characteristics.

Our analysis reveals that a positive impact of identity on monetary incentives is driven by the multi-task environment. Therefore, our findings suggest that the necessity to control for identity becomes more important as the managers' activities become more diverse. Assuming that higher-level managers are more often confronted with multiple tasks, we would expect that complementarities between identity preferences and monetary rewards are more pronounced at higher hierarchical levels. An interesting extension to our setting is the endogenous nature of a firm's task assignment problem. In particular, we would expect that employees who strongly identify with their firms are generally assigned more diverse tasks than employees who identify weakly or not at all with their firm. For the same reason, we would expect firms to invest more in fostering their employees' identification with them, the more their business model requires their employees to take care of more diverse tasks that are difficult to measure.

Our model is a first step toward the analysis of identity preferences and the design of incentive and information systems. Understanding the interplay among these factors may provide guidance for future empirical work involving managers' identities. Clearly, our results are subject to the caveat of being based on simple representations of the agent's identity preferences, together with the restriction to linear contracts that is common to LEN models. Despite this limitation, we expect the economic mechanisms that cause identity incentives and monetary rewards to be partial substitutes or partial complements to continue to hold for optimal contracts.

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APPENDIX A
PROOFS

Lemma 1

Setting (2) equal to zero and re-arranging yields (5a). Setting (1) equal to zero and solving for f yields $f^{\dagger}$. Substituting $f^{\dagger}$ in (4) and using (5a) yields the principal's unconstrained maximization problem:

$$\Pi(f^{\dagger}, v, s, a^{\dagger}) = b(1+\gamma)^{-1}(vm+\gamma s) - \tfrac{1}{2}\gamma(1+\gamma)^{-1}s^2 - \tfrac{1}{2}[(1+\gamma)^{-1}m^2 + r\sigma^2]v^2. \tag{A1}$$

Note that $\Pi(f^{\dagger},v,s,a^{\dagger})$ is separable in s and v. Differentiating (A1) with respect to s and v, setting the derivatives equal to zero and solving for s and v yields (5b) and (5c). ■

Proposition 1

(i) Substituting (5b) and (5c) into (5a) yields:

$$a^{\dagger} = \left(m^2 + r(1+\gamma)\sigma^2\right)^{-1}(m^2 + r\gamma\sigma^2))b \leq b. \tag{A2}$$

(ii) Applying l'Hôspital's rule yields $\lim_{\gamma\to\infty} a^{\dagger} = b$.

(iii) and (iv) Differentiating (A2) and (5b) with respect to γ establishes the claim. ■

Lemma 2

The agent's certainty equivalent with two performance measures is characterized by:

$$CE(z,a) = f + (vm + \gamma s)a - \tfrac{1}{2}(1+\gamma)a^2 - \tfrac{1}{2}\{r(v^2\sigma^2 + 2vv_e\rho\sigma\sigma_e + v_e^2\sigma_e^2) + \gamma s^2\}. \tag{A3}$$

The agent's action choice is characterized by the solution to $\partial CE(z,a)/\partial a = 0$. Setting (A3) equal to zero and solving it for f yields $f^\dagger$. Substituting $f^\dagger$ and $a^\dagger$ in (4) yields the principal's unconstrained optimization problem. Solving the first-order conditions with respect to v, v_e, and s for the three variables yields (8a), (8b), and $s^\dagger = b$. Substituting these results in the unconstrained maximization problem yields (8c). ■

Proposition 2

(i) Dividing (8b) by (8a) yields $v_e{}^\dagger / v^\dagger = -\rho\sigma/\sigma_e$. Taking the derivative with respect to γ, and, for (ii), differentiating (9) with respect to γ establishes the claim. ■

Lemma 3

The agent's certainty equivalent in the two-task setting is given by:

$$CE(z,a_1,a_2) = f + v(ma_1 + m_2a_2) - \tfrac{1}{2}(a_1^2 + a_2^2) - \tfrac{1}{2}\gamma[(s_1 - a_1)^2 + (s_2 - a_2)^2] - \tfrac{1}{2}rv^2\sigma^2. \tag{A4}$$

Differentiating (A4) with respect to a_1 and a_2 yields the agent's effort choices $a_1{}^\dagger$ and $a_2{}^\dagger$. Setting (A4) equal to zero, solving for f, and substituting the result in (4) yields the principal's optimization problem:

$$\Pi(\eta_1, s_1, s_2, v, a_1, a_2) = b_1a_1 + b_2a_2 - \tfrac{1}{2}(a_1^2 + a_2^2) - \tfrac{1}{2}\gamma[(s_1 - a_1)^2 + (s_2 - a_2)^2] - \tfrac{1}{2}rv^2\sigma^2. \tag{A5}$$

Substituting $a_1{}^\dagger$ and $a_2{}^\dagger$ in (A5), differentiating the result with respect to v, s_1, and s_2, and solving the first-order conditions for these variables yields (10a) and (10b). Substituting $a_1{}^\dagger$ and $a_2{}^\dagger$ plus (10a) and (10b) in (A5) yields (10c). ■

Proposition 3

The proof is organized as follows: We first show the derivative of a_i with respect to γ and characterize the condition for $\partial a_i{}^\dagger/\partial\gamma < 0$, $i = 1,2$. Using this characterization, we establish the sufficient condition in (iii) and then show that the two necessary conditions in (i) and (ii) must hold. Differentiating (11a) with respect to γ yields:

$$\frac{\partial a_i^\dagger}{\partial\gamma} = \frac{b_i(m_l^2 + r(1+\gamma)\sigma^2)^2 + b_i m_1^2 m_2^2 - b_l m_1 m_2(m_1^2 + m_2^2 + 2r(1+\gamma)\sigma^2)}{(1+\gamma)^2(m_1^2 + m_2^2 + r(1+\gamma)\sigma^2)^2}, \tag{A6}$$

$i, l = 1, 2$ and $i \neq l$.

Setting $\partial a_i{}^\dagger/\partial\gamma < 0$ and rearranging terms yields the following condition for $\partial a_i{}^\dagger/\partial\gamma < 0$:

$$\frac{b_i}{m_i} < \frac{b_l}{m_l} \cdot \frac{m_l^2(m_1^2 + m_2^2 + 2r(1+\gamma)\sigma^2)}{(m_l^2 + r(1+\gamma)\sigma^2)^2 + m_1^2 m_2^2}. \tag{A7}$$

(iii) The right-hand side of (A7) is monotonically decreasing in γ and goes to zero for $\gamma \to \infty$.

This indicates that for $\partial a_i{}^\dagger/\partial\gamma < 0$ to hold, γ cannot exceed a certain threshold, γ_c. Setting (A7) to equal zero and solving for γ provides γ_c:

$$\partial a_i^\dagger/\partial\gamma = 0 \text{ if } \gamma = \gamma_c = \frac{b_l m_1 m_2 - b_i(m_l^2 + r\sigma^2) + \sqrt{m_1 m_2 (b_l m_i - b_i m_l)(b_1 m_1 + b_2 m_2)}}{r b_i \sigma^2}. \tag{A8}$$

(i) Substituting $b_i/m_i = b_l/m_l$ in (A8) yields $\gamma_c < 0$.

(ii) The numerator of the right-hand side in (A8) is negative for high values of r. This indicates that for $\gamma_c > 0$, it has to be the case that $r < r_c$, where r_c yields a zero numerator in (A8), i.e.:

$$r_c = (b_i\sigma^2)^{-1}\left[m_l(b_l m_i - b_i m_l) + \sqrt{m_1 m_2 (b_l m_i - b_i m_l)(b_1 m_1 + b_2 m_2)}\right]. \ \blacksquare$$

Proposition 4

Taking the derivative of the numerator in (12) with respect to γ establishes the claim. ■

Lemma 4

The agent's certainty equivalent is given by:

$$\begin{aligned} CE(z, a_1, a_2) = {} & f + v(m_1 a_1 + m_2 a_2) + v_a m_a a_1 - \tfrac{1}{2}(a_1^2 + a_2^2) - \tfrac{1}{2}\gamma[(s_1 - a_1)^2 + (s_2 - a_2)^2] \\ & - \tfrac{1}{2} r(v^2\sigma^2 + v_a^2\sigma_a^2). \end{aligned} \tag{A9}$$

Differentiating (A9) with respect to a_1 and a_2 and solving the first-order conditions for these variables yields the agent's effort choices $a_1{}^\dagger$ and $a_2{}^\dagger$. Setting (A9) to equal zero, solving for f, and substituting the result in (4) yields the principal's optimization problem:

$$\Pi(z, a_1, a_2) = b_1 a_1 + b_2 a_2 - \tfrac{1}{2}(a_1^2 + a_2^2) - \tfrac{1}{2}\gamma[(s_1 - a_1)^2 + (s_2 - a_2)^2] - \tfrac{1}{2} r(v^2\sigma^2 + v_a^2\sigma_a^2). \tag{A10}$$

Substituting $a_1{}^\dagger$ and $a_2{}^\dagger$ in (A10), differentiating the result with respect to v, v_a, s_1, and s_2, and solving the first-order conditions for these variables yields (14a), (14b), and (14c). Substituting $a_1{}^\dagger$ and $a_2{}^\dagger$ plus (14a), (14b), and (14c) in (A10) yields (14d). ■

Proposition 5

(i) Differentiating Expression (14b) with respect to γ, setting $\partial v^\dagger/\partial\gamma > 0$, and solving the result for γ yields:

$$\gamma' = \frac{-\sigma\left(b_2 m_2 m_a^2 + r\sigma_a^2(b_1 m_1 + b_2 m_2)\right) + m_a\sqrt{m_1 m_2\left((b_1 m_2 - b_2 m_1)(b_1 m_1 + b_2 m_2)\sigma_a^2 - b_1 b_2 m_a^2 \sigma^2\right)}}{r\sigma\sigma_a^2(b_1 m_1 + b_2 m_2)}.$$

Differentiating (14c) with respect to γ, setting $\partial v_a{}^\dagger/\partial\gamma > 0$, and solving the result for γ yields:

$$\gamma'' = \frac{\sigma_a m_a(b_2 m_1 - b_1 m_2) - r\sigma^2\sigma_a b_1 + \sqrt{m_1 m_2\left((b_2 m_1 - b_1 m_2)(b_1 m_1 + b_2 m_2)\sigma_a^2 + b_1 b_2 m_a^2\sigma^2\right)}}{r\sigma^2\sigma_a b_1}.$$

(ii) Substituting (14b) and (14c) in $IR = v^\dagger/v_a{}^\dagger$ yields the incentive ratio. Differentiating $IR^\dagger$ with respect to γ yields:

$$\partial IR^{\dagger}/\partial\gamma = rm_2 \frac{(b_1m_2 - b_2m_1)(b_1m_1 + b_2m_2)\sigma_a^2 - b_1b_2m_a^2\sigma^2}{m_a\left(b_2m_1m_2 - b_1\left(r(1+\gamma)\sigma^2 + m_2^2\right)\right)^2}.$$

Substituting $b_1/m_1 < b_2/m_2$ establishes the claim. ■

Proposition 6

(i) Setting $r = 0$ in (15) and differentiating with respect to γ yields:

$V^{\dagger}(\eta_2|\eta_1) = -\frac{1}{2}(1+\gamma)^{-2}(m_1^2 + m_2^2)^{-1}(m_2^2)^{-1}[m_2(b_1m_2 - b_2m_1)]^2 < 0.$

(ii) Letting r grow to infinity in (15) yields:

$\lim_{r\to\infty} V^{\dagger}(\eta_2|\eta_1) = 0.$

(iii) From (15), $V^{\dagger}(\eta_2|\eta_1) = 0$ for $\gamma = \gamma_{\lambda} \equiv (m_2/m_1 - b_2/b_1)\, m_1m_2 /(r\,\sigma^2) - 1$, and $\gamma_{\lambda} > 0$ for $b_1m_2 - b_2m_1$ sufficiently positive. Moreover, $\partial V^{\dagger}(\eta_2|\eta_1)/\partial\gamma > 0$ if $\gamma = \gamma_{\lambda}$. ■

APPENDIX B

IDENTITY PREFERENCES IN A MULTI-TASK LEN SETTING

This section establishes that the optimal standards chosen by the principal in a setting with quadratic identity- and effort-cost functions are independent of the agent's degree of internalization of the standards (which implies $s_i^{\dagger} = b_i$), irrespective of the number of tasks that the agent is responsible for and irrespective of the characteristics of the information system available for contracting.

B.1 Agent's Preferences, Information System, and Action Choices

Assume the agent controls n activities that influence the payoff to the principal and let the vector $\mathbf{a} \in \Re^n$ represent his choice of effort levels. The personal cost of effort is assumed to be:

$$\kappa(\mathbf{a}) = \tfrac{1}{2}\,\mathbf{a}'\boldsymbol{\Gamma}_{\mathbf{a}}\mathbf{a},$$

where $\boldsymbol{\Gamma}_{\mathbf{a}}$ is a $n \times n$ symmetric, positive-definite matrix of effort cost parameters.

The agent's personal cost of deviating from the standard is assumed to be:

$$\kappa_s(\mathbf{s}, \mathbf{a}) = \tfrac{1}{2}(\mathbf{s} - \mathbf{a})'\boldsymbol{\Gamma}_{\mathrm{s}}(\mathbf{s} - \mathbf{a}),$$

where the symmetric, positive-definite matrix $\boldsymbol{\Gamma}_{\mathrm{s}}$ represents the agent's degree of internalization of the standards.

The information system discloses a $m \times 1$-vector $\mathbf{y}$ of performance measures at date 2, i.e., $\mathbf{y} = \mathbf{M}\,\mathbf{a} + \boldsymbol{\varepsilon}$, where $\mathbf{M}$ is a $m \times n$-matrix of sensitivities of the signals to the agent's effort levels in his n tasks, and $\boldsymbol{\varepsilon}$ is a $m \times 1$-vector of normally distributed noise terms with $\boldsymbol{\varepsilon} \sim \mathrm{N}(\mathbf{0},\boldsymbol{\Sigma})$, and $m \times m$-covariance matrix $\boldsymbol{\Sigma}$.

At date 0, the principal offers the agent a contract, represented by $z = (f,\mathbf{v},\mathbf{s})$, where $\mathbf{s}$ is the standard specified by the principal and $\mathbf{v}$ is the agent's $m \times 1$ vector of incentive rates for each performance measure. Hence, the agent's compensation given performance measures $\mathbf{y}$ and contract z is $w(\mathbf{y},z) = f + \mathbf{v}'\mathbf{y}$. Applying the specifications to the agent's utility provides the following certainty equivalent:

$$CE(z, \mathbf{a}) = f + (\mathbf{M}'\mathbf{v} + \boldsymbol{\Gamma}_{\mathrm{s}}\mathbf{s})'\mathbf{a} - \tfrac{1}{2}\,\mathbf{a}'(\boldsymbol{\Gamma}_{\mathbf{a}} + \boldsymbol{\Gamma}_{\mathrm{s}})\mathbf{a} - \tfrac{1}{2}(r\mathbf{v}'\boldsymbol{\Sigma}\mathbf{v} + \mathbf{s}'\boldsymbol{\Gamma}_{\mathrm{s}}\mathbf{s}). \tag{B1}$$

Differentiating (B1) with respect to **a**, given contract z, provides the agent's action choice:

$$\mathbf{a}^{\dagger}(\mathbf{v},\mathbf{s}) = (\mathbf{\Gamma_a} + \mathbf{\Gamma_s})^{-1}(\mathbf{M}'\mathbf{v} + \mathbf{\Gamma_s s}). \tag{B2}$$

The resulting certainty equivalent (substituting (B2) into (B1)) is:

$$CE^{\dagger}(z) = f + ½(\mathbf{M}'\mathbf{v} + \mathbf{\Gamma_s s})'(\mathbf{\Gamma_a} + \mathbf{\Gamma_s})^{-1}(\mathbf{M}'\mathbf{v} + \mathbf{\Gamma_s s}) - ½(r\mathbf{v}'\mathbf{\Sigma}\mathbf{v} + \mathbf{s}'\mathbf{\Gamma_s s}). \tag{B3}$$

Again, the agent will only participate in the firm if his contract z is such that it provides him with his reservation certainty equivalent, which is scaled to equal zero, i.e.:

$$CE^{\dagger}(z) \geq 0. \tag{B4}$$

B.2 Principal's Preferences and Contract Choice

The expected gross value of the firm is assumed to be a linear function of the effort expended by the agent and is represented by $E[\mathrm{x}|\mathbf{a}] = \mathbf{b}'\mathbf{a}$, where the vector $\mathbf{b} \in \Re^{n}$ represents the agent's productivity with respect to the principal's payoff. The expected net terminal value of the firm is:

$$\Pi = E[x - w|\mathbf{a}]. \tag{B5}$$

To obtain the optimal contract, the principal chooses z to maximize (B5) subject to (B2) and (B4). Choosing f such that $CE^{\dagger}(z) = 0$ and substituting $f^{\dagger}$ and (B2) into (B5) gives the principal's unconstrained decision problem:

$$\begin{aligned}\Pi^{\dagger}(\mathbf{v},\mathbf{s}) = {} & \mathbf{b}'(\mathbf{\Gamma_a} + \mathbf{\Gamma_s})^{-1}\mathbf{M}'\mathbf{v} - ½\,\mathbf{v}'\left(\mathbf{M}(\mathbf{\Gamma_a} + \mathbf{\Gamma_s})^{-1}\mathbf{M}' + r\mathbf{\Sigma}\right)\mathbf{v} + \mathbf{b}'(\mathbf{\Gamma_a} + \mathbf{\Gamma_s})^{-1}\mathbf{\Gamma_s s} \\ & - ½\,\mathbf{s}'\mathbf{\Gamma_a}(\mathbf{\Gamma_a} + \mathbf{\Gamma_s})^{-1}\mathbf{\Gamma_s s}.\end{aligned} \tag{B6}$$

Optimizing (B6) with respect to each incentive rate in **v** and each standard in **s** results in the following characterization of the optimal incentive rates and standards:

$$\mathbf{v}^{\dagger} = \mathbf{Q}^{-1}\mathbf{M}(\mathbf{\Gamma_a} + \mathbf{\Gamma_s})^{-1}\mathbf{b}, \tag{B7a}$$

$$\mathbf{s}^{\dagger} = \mathbf{\Gamma_a^{-1}}\mathbf{b}, \tag{B7b}$$

with:

$$\mathbf{Q} \equiv \mathbf{M}(\mathbf{\Gamma_a} + \mathbf{\Gamma_s})^{-1}\mathbf{M}' + r\mathbf{\Sigma}.$$

Thus the standard is solely influenced by the marginal cost and productivity of the agent's effort choice and is set equal to the first-best effort level.

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Do Financial Statement Users Judge Relevance Based on Properties of Reliability?

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ABSTRACT: Relevance and reliability (now referred to as "representational faithfulness") are qualities of financial information that both the Financial Accounting Standards Board and the International Accounting Standards Board use in setting standards for financial reporting. Despite their importance, very little research has addressed how financial statement users apply these constructs. Via experiments set within the fair value context, we show that users do not view them as independent constructs. Instead, variations in properties that are associated with the *reliability* of a measurement influence users' assessments of the *relevance* of fair value. The relationship between assessed relevance and assessed reliability is unidirectional, in that factors underlying reliability influence judgments of relevance, but factors underlying relevance do not influence judgments of reliability. Our findings are important because inappropriate assessments of relevance can influence firm valuation. The results are particularly meaningful in the context of fair value because such measurements can vary widely in reliability.

Keywords: *relevance; reliability; fair value; valuation.*

JEL Classifications: *M41.*

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I. INTRODUCTION

Relevance and reliability (now referred to as "representational faithfulness") are important qualities of financial reporting that both the Financial Accounting Standards Board (FASB) and the International Accounting Standards Board (IASB) use in setting standards (FASB 2010; IASB 2010).[1] Relevance addresses the pertinence of an economic construct (e.g., fair value, historical cost) to a user's decision. Reliability addresses how well that economic construct, or phenomenon, is depicted or measured (e.g., fair value based on a market transaction versus a model). Standard setters note that although relevance and reliability are both necessary for information to be useful in decision making, information can be relevant without being reliable and can be reliable without being relevant (FASB 2010, ¶QC 17).[2] In this paper, we assert and experimentally test the idea that financial statement users conflate these two constructs when judging the relevance of fair value measurement; that is, we test the idea that users do not view these two constructs as independent.

Investigating how financial statement users think about the constructs of relevance and reliability in the context of fair value measurement is important for several reasons. First, we lack empirical evidence regarding how users assess these qualities. Even the limited research and debates to date suggest that the answer is unclear. Some researchers indicate that users view relevance and reliability as inversely related (e.g., Schipper 2003; Dye and Sridhar 2004), while others assert a positive relationship (McCaslin and Stanga 1983; Duncan and Moores 1988). Second, one potential reason why many financial statement users oppose fair value accounting may originate in their conflating the concepts of relevance and reliability. Indeed, some of the criticisms raised against fair value accounting during the recent financial crisis appear to be due to individuals judging the relevance of fair value based on the reliability of its measurements (American Bankers Association 2009). By providing scholarly evidence on this issue, we provide to standard setters information that will help them make important decisions about fair value measurements in accounting.

In this paper, we report the results of several experiments in which we investigate the hypothesis that financial statement users' assessments of the relevance of an economic construct are influenced by factors (i.e., potential for bias and error) underlying the reliability of its measurement. We examine this idea in the context of fair value measurement of an asset, as relevance and reliability issues are of particular practical concern there.[3] Finally, we also investigate the economic importance of our ideas by determining whether the relationships noted above influence users' judgments about a firm's valuation.

Our hypotheses are grounded in psychology theories about attribute substitution (Kahneman and Frederick 2002). Attribute substitution is a heuristic used by individuals asked to evaluate a difficult-to-assess attribute. The theory predicts that when asked to evaluate a less accessible attribute, individuals often (without awareness) substitute an evaluation of a more accessible attribute of the same entity. The accessibility of an attribute relates to an individual's ability to conceptualize the value of an attribute, with increases in accessibility due, in part, to the frequency with which the attribute is evaluated as part of perception and comprehension (Kahneman and Frederick 2002). We provide evidence that reliability is a basic property of fair value measurement that users understand well. On the other hand, relevance is decision specific and requires a more difficult, case-by-case analysis. Thus, we maintain that in the fair value context, reliability is a more accessible attribute than is relevance. We, therefore, predict that users will substitute the more accessible assessment of measurement reliability when asked to assess the relevance of a fair value.

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[1] The FASB and IASB recently replaced the term "reliability" with "representational faithfulness" to address the possibility that auditors would associate reliability with vouchability in addition to the factors it was intended to incorporate (Schipper 2005). This modification to the conceptual framework does not affect our study because we examine features of reliability (i.e., error and bias) that are unrelated to the change. We use the more familiar term, reliability, herein.

[2] The FASB distinguishes relevance and reliability from a third construct, decision usefulness. They note that "[i]nformation must be both relevant and faithfully represented if it is to be useful. Neither a faithful representation of an irrelevant phenomenon, nor an unfaithful representation of a relevant phenomenon, helps users make good decisions" (FASB 2010, ¶QC 17). Their comments imply that an unreliable measure of a highly relevant phenomenon does not diminish the relevance of the phenomenon to the decision, but reduces the decision usefulness of the information.

[3] Although our paper is set in the context of fair value, it does not address the issue of whether fair value is more or less relevant than historical cost (see Koonce et al. [2011] for a study of that issue). Rather, our study focuses on how the judged relevance of fair value is influenced by variations in the reliability of its measurement.

The results of our experiments support our hypothesis that variations in factors associated with the reliability of an asset's fair value measurement affect financial statement users' assessments of the relevance of a fair value. Specifically, we find that a measurement's source and method of derivation (i.e., a market transaction versus a model) strongly influence users' judgments about relevance. Because attribute substitution is unidirectional, users substitute evaluations of more accessible attributes for evaluations of less accessible attributes. However, the opposite does not occur. Accordingly, we also show that manipulations of factors underlying relevance do not affect users' assessments of reliability. Finally, results support the idea that because of this attribute substitution, factors underlying reliability may be over-weighted in users' valuation judgments; that is, these factors influence valuation not only through their effect on assessed reliability, but also through their influence on assessed relevance. While we cannot establish a normative benchmark for valuation in our experiment, these results suggest that properties underlying reliability have too much emphasis in valuation.

Our study has several important implications. First, our results shed light on one reason that efforts of the FASB and the IASB to extend the use of fair values in financial reports are being met with resistance. We show that acceptance of the concept of fair value measurement may be conditioned primarily on how well the items are measured rather than on separate assessments of the relevance of the economic construct of fair value and the reliability of its measurement. This idea finds anecdotal support when, during the recent financial crisis, observers argued that thin markets and unreliable measurements created fair value accounting outcomes that were not relevant to decision makers (American Bankers Association 2009). We believe our study is important to this debate; it indicates that at least part of the reason users argue against the use of fair value may be that they do not consider reliability and relevance to be independent constructs. The primacy of reliability in users' minds may indicate that standard setters should focus on standards that enhance the reliability of fair value measurements in order to make the biggest gains in acceptance of fair value. Additionally, standard setters may wish to focus on ways to better communicate the importance of relevance of particular constructs separate from their efforts to improve the reliability of the measurements.

Second, users' conflation of the qualities of reliability and relevance may hamper additional attempts to improve the actual reliability of fair value measures or the communication of reliability. Although the FASB has previously issued guidance about the communication of fair value measurement reliability (e.g., SFAS 157), we argue that further efforts to improve those disclosures may be thwarted if users are unable to consider the relevance of the fair value construct *independent of* the reliability of its measurement. By documenting that users do not view these two qualities separately, we hope that our study provides a foundation for greater clarity on what can be improved (i.e., measurement reliability or its disclosure) and what is relatively fixed (i.e., relevance).

Third, our results suggest that financial statement users' conflation of relevance and reliability may influence their valuation judgments. That is, strengths or deficits in reliability due to potential error appear to be over-weighted in valuations. The natural next question is whether users' association of relevance with reliability can be moderated. Our theory and results are consistent with the ideas of attribution substitution theory, which implies that decision makers are *unaware* of the substitution. However, we did not explicitly test this assumption. Psychology researchers have long struggled with determining how individuals unconsciously evaluate their environment, judge other individuals, and undertake actions, all while consciously thinking about something else (Wilson 2002). A better understanding of whether financial statement users are aware of the attribute substitution is important, as the success of attempts to moderate its effects is likely contingent on their awareness. Stated differently, if decision makers are unaware of the attribute substitution, then training in assessing relevance may not be sufficient to correct their behavior (e.g., see Tversky and Kahneman 1974).

In the following section, we develop our theoretical predictions. Sections III and IV describe our two primary experiments, respectively. Section V provides robustness tests and Section VI summarizes and concludes the paper.

II. HYPOTHESES DEVELOPMENT

The Independence of Relevance and Reliability

Relevance and reliability are the two primary qualitative characteristics of accounting information that determine its usefulness in decision making (FASB 1980, 2010). Relevance applies to a construct, a type of information. Reliability, on the other hand, is a property of a specific measurement of that construct. Both relevance and reliability influence the reported measure's decision usefulness (FASB 2010, ¶ QC17).

In its conceptual framework, the FASB notes that information must be both relevant and reliable to be useful, and that neither a reliable depiction of an irrelevant phenomenon nor an unreliable depiction of a relevant phenomenon is decision useful (FASB 2010, ¶ QC17). The FASB's comments imply that all four cells of a 2 (relevance: lower, higher) × 2 (reliability: lower, higher) matrix could be populated, as illustrated in Panel A of Figure 1. The FASB further suggests to preparers that when considering a particular decision, they should follow a sequence of steps, looking first for the most relevant information for that decision and then ascertaining the reliability of a measure of that information. If that information is lacking in reliability (or is just not available), then the decision maker should select the next most relevant measure, then again check its reliability. This process is repeated as necessary (FASB 2010, ¶ QC18; IASB 2010, ¶ QC18). Although many assert that the FASB's conceptual framework is *suggestive* of the independence of relevance and reliability (Maines and Wahlen 2006; Li et al. 2010), the framework does not *explicitly* state that the concepts are independent, and so the FASB's view on this issue is unknown.

Although the FASB's conceptual framework is unclear on the idea that relevance and reliability are independent qualities—that is, that a particular level of one does not imply a particular level in another—viewpoints outside of accounting maintain that the two constructs should be viewed as independent. In marketing, information usefulness is deemed a function of its diagnosticity (i.e., relevance) and, separately, its reliability (Dick et al. 1990). In law, information is useful if it is admissible (i.e., has relevance) and has probative value (i.e., is reliable) (Mueller and Kirkpatrick 1995). In research settings, a construct is useful if it is relevant and its measurement is reliable (Herzog 1997).

The following example, outside of the financial reporting context, helps to illustrate the view that relevance and reliability are distinct and independent constructs. Consider an individual who is spending the day in Chicago. For that individual, the air temperature in Honolulu is not highly relevant to his decision about whether to wear an overcoat when going outside. Nonetheless, the

FIGURE 1
Relevance and Reliability as Independent Constructs

Panel A: Relevance and Reliability for a Decision about What Value to Place on an Asset

	Lower Reliability	**Higher Reliability**
Lower Relevance	Model estimate without comparables, Stand-alone value of asset in use	Comparable sales value, Stand-alone value of asset in use
Higher Relevance	Model estimate without comparables, Stand-alone value of asset held for sale	Comparable sales value, Stand-alone value of asset held for sale

Panel B: Relevance and Reliability for a Decision about Whether to Wear an Overcoat in Chicago

	Lower Reliability	**Higher Reliability**
Lower Relevance	Broken thermometer, Today's Honolulu temperature	Accurate thermometer, Today's Honolulu temperature
Higher Relevance	Broken thermometer, Today's Chicago temperature	Accurate thermometer, Today's Chicago temperature

temperature in Honolulu can be measured by a very unreliable process or instrument, or by a very reliable one. The air temperature in Chicago, on the other hand, is highly relevant to the individual for the decision about whether to wear an overcoat. Likewise, the temperature in Chicago can be measured by a very unreliable process or instrument, or by a very reliable one. Indeed, the construct of "Chicago air temperature" is relevant to the person in Chicago; the reliability of the current measurement of the Chicago temperature is a separate, independent factor that the decision maker must consider.

Do Users View Them as Independent?

While we argue that the constructs of reliability and relevance should be viewed as independent, we also question whether financial statement users judging the relevance of fair value treat them as such. As explained more fully below, prior research indicates that financial statement users have fairly accurate ideas about how to assess reliability, and so we expect that they will be able to do so readily. On the other hand, they are less likely to have a ready framework for assessing relevance, particularly in the fair value context (Koonce et al. 2011). As a result, we posit that they will rely on attribute substitution to judge fair value relevance (Kahneman and Frederick 2002). That is, they are likely to substitute an easier question (i.e., how reliable is this fair value measure?) when asked the relatively difficult question "how relevant is fair value?" We explain our argument in more detail below.

Reliability is a basic property of information, and individuals are well acquainted with its determinants. To support this assertion, we note that numerous studies in accounting and psychology provide evidence that individuals understand that factors underlying reliability, such as the objectivity of the measurement source and the error-proneness of that source, affect the assessed decision usefulness of a reported measure. For example, Hirst (1994) showed that auditors discount the inferential value of information obtained from sources that are low in competence or objectivity relative to that of information obtained from sources higher in these characteristics. Similarly, market participants discount forecasts made by management with low prior accuracy in forecasting earnings relative to those with high prior accuracy (Williams 1996; Hirst et al. 1999). Moreover, market participants' reactions to management forecasts are, in part, a function of the credibility of management (Jennings 1987). Investors glean manager credibility from factors such as their competence, trustworthiness, and situational incentives (Mercer 2004). Additional studies indicate that users of other forms of financial reporting can identify when a source is more or less credible and will utilize the information accordingly. For example, users' assessments of financial analysts' credibility depend, in part, on analysts' prior accuracy, and users' willingness to purchase future reports from these analysts is associated with their assessments of analysts' credibility (Kadous et al. 2009). We thus propose that reliability is relatively easy to assess and, therefore, is likely to be an accessible attribute for most users.

Unlike reliability, the relevance of an item depends on the specific decision to be made and its context (FASB 1980, 2010, ¶ BC3.11). Because each situation is unique, users are less likely to have a ready schema for assessing relevance. That is, rather than relying on well-established relationships (e.g., more competent sources tend to produce more reliable measurements), users have to carefully evaluate their decision task and decision context to assess relevance. There are limited circumstances in which relevance is easy to assess (e.g., judging the relevance of the air temperature in Chicago to the decision to wear a coat). However, based on prior research by Koonce et al. (2011), we propose that in the fair value context, relevance is a less accessible attribute than reliability (also see Joyce et al. [1982] and Jurney [2008] for evidence supporting this idea in financial reporting, more generally).[4]

Because assessing the relevance of fair value will be difficult for financial statement users, we believe that attribute substitution will apply (Kahneman and Frederick 2002). In general, a person who is asked a question about an accessible (i.e., easily retrievable from their knowledge) attribute such as reliability can answer the question directly and easily. In contrast, a person who is asked a difficult question will follow a different process. The individual, being unable to access information about the target attribute, will instead answer an easier question about a more accessible attribute of the same entity or object (Kahneman 2002, 469).

In our fair value setting, we argue that the more accessible attribute is reliability and the less accessible attribute is relevance. Thus, we propose that financial statement users asked to assess the reliability of a fair value measurement of a financial statement item will readily respond to that question, as reliability is an accessible attribute. In contrast, financial statement users will have difficulty responding to a question asking them to assess the relevance of an item's fair value because relevance is less accessible. Following the logic of attribute substitution, users asked to assess relevance will instead answer an easier question about the reliability of the measurement of the financial statement item in question. In other words, they will assess the relevance of a reported economic construct by assessing how well it represents, or is similar to, a reliable measurement.[5] This reasoning implies that financial statement users will not only use factors underlying the reliability of a measurement in assessing its reliability, but they will also rely on those same variables in assessing relevance. This logic leads to our first hypothesis:

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[4] While these studies demonstrate users' relative difficulty in assessing relevance versus reliability, they do not address why this occurs nor do they test our hypothesis that variations in properties associated with reliability influence relevance judgments.

> **H1:** Factors underlying the *reliability* of a fair value measurement influence financial statement users' assessments of the *relevance* of fair value information.

Although our theory predicts that assessments of reliability and relevance will be positively associated, it is important to note that we also expect that the relationship is unidirectional. Manipulations of factors underlying the reliability of a measurement will influence assessments of the reported construct's relevance (and, of course, of measurement reliability). However, we do not expect the opposite relationship to hold—that is, manipulations of factors underlying relevance will not affect users' assessments of reliability. The latter is unlikely to occur because reliability is a relatively accessible attribute and so users will be able to readily answer a question about reliability.

The decision usefulness of information is a function of both its relevance and its reliability (FASB 1980, 2010). Thus, we expect users will rely on their own assessments of the relevance and reliability of information in determining how useful that information is for subsequent decisions, such as valuation. If variations in properties underlying reliability affect assessments of reliability, as is appropriate, but also affect assessments of relevance (H1), and if users incorporate assessments of both relevance and reliability into their valuation judgments, then variations in the properties underlying reliability will influence valuation through two distinct paths—one through users' assessments of the relevance of the item and another through their assessments of the reliability of the item's measurement. We summarize this prediction in the following hypothesis:

> **H2:** Factors underlying the *reliability* of a measurement influence financial statement users' valuation judgments via two effects—one through assessed reliability and a second through assessed relevance.

Two caveats apply. First, Kahneman and Frederick (2002, 53) indicate that attribute substitution usually occurs without decision maker awareness. Indeed, those who substitute one question for another simply fail to notice they are answering a different question (Kahneman 2002, 469). Although we rely on the idea of attribute substitution for our predictions, we do not test the assumption that individuals are unaware of the substitution. Second, although it is difficult to establish a normative benchmark for weights that users should apply to various factors when making valuation judgments, our ideas suggest that users place too much weight on the properties underlying reliability when they make valuation judgments. That is, reliability may have too much emphasis in valuation. This idea is consistent with prior research that indicates that highly accessible values tend to be over-weighted in judgments (Strack and Mussweiler 1997; Epley and Gilovich 2002; Chapman and Johnson 2002; Epley and Gilovich 2006).

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[5] Judgments made in this fashion often are described as following the representativeness heuristic (Tversky and Kahneman 1974).

III. EXPERIMENT 1

Participants, Design, and Variables

We tested our hypotheses in an experiment with 129 M.B.A. students from two *BusinessWeek* top-25 schools.[6] We asked participants to assume the role of a potential investor in a manufacturing firm. All were told that the company had purchased a plot of land for $41 million two years ago. The current value of the land, generated by external consultants who relied on a model, was estimated at $53 million. As explained more fully below, we manipulated two variables relating to properties underlying the reliability and relevance of the land's current value in a 2 × 2 full-factorial, between-participants design. We randomly assigned participants to conditions.

We manipulated the competence of the measurement source (i.e., external consultants) at two levels. Those participants in the high competence (i.e., high reliability) conditions were provided with the following information regarding the consultants:

> Based on your experience and knowledge, you believe the consultants are unbiased, and based on the accuracy of their work you have noted on other valuations, you believe they are highly competent.

We provided participants in the low competence (i.e., low reliability[7]) conditions with the following statement:

> Based on your experience and knowledge, you believe the consultants are unbiased, but based on errors and reasoning flaws you have noted on other valuations, you believe they are not very competent.

Our second manipulation relates to a property we expect financial statement users to associate with relevance—the length of time until management plans to sell the asset (FASB 2006, SFAS 157, ¶20). We manipulated the time to sale at two levels. Approximately half of the participants were told that management intends to sell the plot of land very soon ("sell soon" conditions), which corresponds to higher relevance for the valuation decision we assigned participants. The remaining participants were told that management does not intend to sell the plot of land for at least 20 more years ("no sell" conditions), which corresponds to lower relevance for this decision.

There is debate as to whether time to sale *should* affect relevance judgments in a valuation context (Hague and Willis 1999). While some individuals believe that the relevance of fair values depends on whether something is to be sold soon or sold later, others consider the distinction as unimportant. Our objective is not to resolve this debate, but rather to leverage the finding—as shown in recent research (Koonce et al. 2011)—that, on average, the time to sale is important to investors' relevance judgments in our design. This manipulation allows us to keep constant the measurement basis (i.e., fair value). Other possible ways to manipulate relevance are less desirable

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[6] Participants came from three sources: 24 were full-time M.B.A. students from one institution (average of 4.7 years of work experience, 4.9 finance classes, 3.1 accounting classes, 71 percent had invested in individual stocks previously and 96 percent plan to do so), 55 were full-time M.B.A. students from a second institution (average of 5.5 years of work experience, 2.5 finance classes, 2.1 accounting classes, 65 percent had invested in stocks previously and 91 percent plan to), and 50 were Executive M.B.A. students from the first institution. Time constraints prevented us from collecting individual demographics from the Executive students; however, the class as a whole averaged 12 years of work experience. Source of participants did not have significant effects on any of our dependent measures.

[7] This manipulation represents higher and lower levels of reliability rather than absolute levels of high and low. For ease of exposition, however, we refer to levels of this variable, as well as other analogous variables in the paper, as "high" and "low" rather than "higher" and "lower."

FIGURE 2
Example of Materials Used in Experiment 1
High Competence, Sell Soon Condition

Several years ago, WRSwann Co., a manufacturing company, purchased a plot of land for $41,000,000.

You learn that management intends to sell the plot of land very soon.

The current value of the land is estimated at $53,000,000. This current value number was generated by external consultants.

Original Value	Consultants' Estimate of Current Value
$41,000,000	$53,000,000

The consultants generated this number with a model that they created. The model uses the market values for several comparable plots of land. However, no individual plot is completely comparable to the one owned by WRSwann, so the model incorporates additional factors that could affect its value (i.e., location, size, desirability, population growth). The weights that are placed on these additional factors can vary depending on the various assumptions made by the consultants.

Based on your experience and knowledge, you believe the consultants are unbiased, and, based on the accuracy of their work you have noted on other valuations, you believe they are highly competent.

as they involve changing the underlying economic information that is communicated (e.g., comparing fair value to historical cost). Figure 2 provides an example of the materials.

After presenting participants with the information described above, we asked for their responses to five questions. Our key dependent variables include assessments of the relevance of "the current value information," the reliability of "the number assigned to the land's current value," and how the "situation" affects the valuation of the firm. We worded these relevance and reliability questions carefully to ensure that participants were asked the relevance question at the construct level (i.e., relevance of current value information) and the reliability question at the operational level (i.e., reliability of its measurement). Participants assessed the relevance of the current value information to their evaluation of the firm on a 101-point scale, with 0 labeled "not at all relevant" and 100 labeled "extremely relevant." The responses to this question serve as the main dependent measure for our first hypothesis. Participants assessed the reliability of the number assigned to the land's value on a 101-point response scale ranging from 0, "very unreliable," to 100, "very reliable."

Participants provided assessments of valuation in two stages. They first assessed whether the situation described in the case materials increases, decreases, or neither increases nor decreases the value they would place on the firm. Those who indicated a change in the value also indicated the magnitude of the change on a 101-point scale, ranging from "slightly increases (decreases) how much I value the company" (0) to "greatly increases (decreases) how much I value the company" (100). We created our valuation dependent variable by coding the magnitude of change as a positive amount for increases in value and a negative amount for decreases in value. This variable is zero for participants who indicated neither an increase nor a decrease in valuation due to the reported information. Two additional questions ask for participants' assessments of the amount of bias in the current value measurement and of management's forthcomingness.[8]

Results

Preliminary Tests

We first assess whether our manipulation of competence influenced participants' judgments of the reliability of the current value number. Participants in the high source competence conditions assessed the reliability of the current value number significantly higher (mean of 67.14) than did those in the low source competence conditions (mean of 45.39) ($F_{1,125} = 42.05$, $p < 0.01$). As expected, neither time to sale nor the interaction of time to sale and source competence significantly influenced reliability assessments (both p-values > 0.30). Also as expected, participants' ratings of bias and management forthcomingness did not significantly differ across levels of source competence (both p-values > 0.10).[9]

Tests of Hypotheses

Descriptive (inferential) statistics for participants' assessments of relevance and their valuation judgments are presented in Panel A (Panel B) of Table 1. Turning first to the relevance dependent measure, the analysis of variance (ANOVA) model reveals a significant main effect of the source competence variable ($F_{1,125} = 10.92$; $p < 0.01$), but no other significant effects. Cell means indicate that participants receiving the measurement from a high competence source viewed the current value information as significantly more relevant (mean of 65.66) than did participants receiving the measurement from a low competence source (mean of 51.44). Thus, financial statement users' determination of the relevance of current value information is based on their determination of the reliability of its measurement, supporting H1.[10] This finding is consistent with our theory, as it implies that evaluations of relevance are based on the more accessible reliability attribute. Further support for participants' reliance on attribute substitution is indicated by a positive and highly significant correlation of their assessments of reliability and relevance ($r = 0.48$, $p < 0.01$) (Kahneman and Frederick 2005).

As noted above, there were no significant effects in the model for participants' relevance ratings other than that of source competence. The manipulation of time to sale did not have the expected effect on ratings of relevance. Thus, we have evidence that source competence influences users' assessments of both reliability and relevance and that time to sale does not influence

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[8] An alternative approach for Experiment 1 would have been to ask participants about the relevance of fair value and then provide them with the reliability of a specific fair value measurement. Such an approach would have mimicked the process recommended by standard setters in their conceptual frameworks (FASB 2010; ¶ QC18; IASB 2010 ¶ QC18). We did not follow this approach because financial statement users typically receive measures that already have characteristics of relevance and reliability. That is, they typically receive information about both constructs simultaneously. Only preparers and standard setters are likely to be in a situation to consider them in a sequential fashion. Our materials thus mimic this important contextual feature for users.

[9] An ANOVA model for bias revealed a marginally significant main effect of time to sale ($p = 0.085$) and a significant interaction of competence and time to sale ($p < 0.01$). While participants rated bias as approximately equal in the two higher competence conditions, they rated bias as higher for lower competence sources when management had no plans to sell and lower for lower competence sources when management planned to sell the asset soon.

[10] We also manipulated bias (high versus low) in Experiment 1. Bias is an underlying property of reliability and, thus, allows us another opportunity to test our hypothesis about attribute substitution. Holding competence at a high level, our results of this additional manipulation reveal that participants in the low bias condition rated the relevance of the current value information higher than did those in the high bias condition (65.66 for low bias versus 60.29 for high bias), consistent with our predictions. However, this effect is only marginally significant ($p = 0.10$, one-tailed).

TABLE 1

Relevance Assessments and Valuation Judgments, Experiment 1

Panel A: Mean Judgments (Standard Deviation)

	Assessed Relevance Dependent Variable Source Competence			Valuation Effect Dependent Variable Source Competence		
Time to Sale	**Low**	**High**	**Row Means**	**Low**	**High**	**Row Means**
No Sell	47.47 (24.40) n = 30	66.21 (21.78) n = 33	57.29 (24.74) n = 63	9.21 (33.29) n = 28	34.75 (34.64) n = 32	22.83 (36.09) n = 60
Sell Soon	54.94 (22.92) n = 34	65.09 (29.61) n = 32	59.86 (26.66) n = 66	22.35 (36.25) n = 31	33.63 (32.56) n = 27	27.60 (34.75) n = 58
Column Means	51.44 (23.73) n = 64	65.66 (25.73) n = 65		16.12 (35.20) n = 59	34.24 (33.42) n = 59	

Panel B: Analysis of Variance

	Assessed Relevance				Valuation Effect			
Source	**df**	**MS**	**Statistic**	**Two-Tailed p-value**	**df**	**MS**	**Statistic**	**Two-Tailed p-value**
Time to Sale	1	325	F = 0.53	0.47	1	1060	F = 0.90	0.34
Source Competence	1	6718	F = 10.92	< 0.01	1	9944	F = 8.46	0.01
Time to Sale × Source Competence	1	594	F = 0.97	0.33	1	1942	F = 1.27	0.26
Residual	125	615			114	1176		

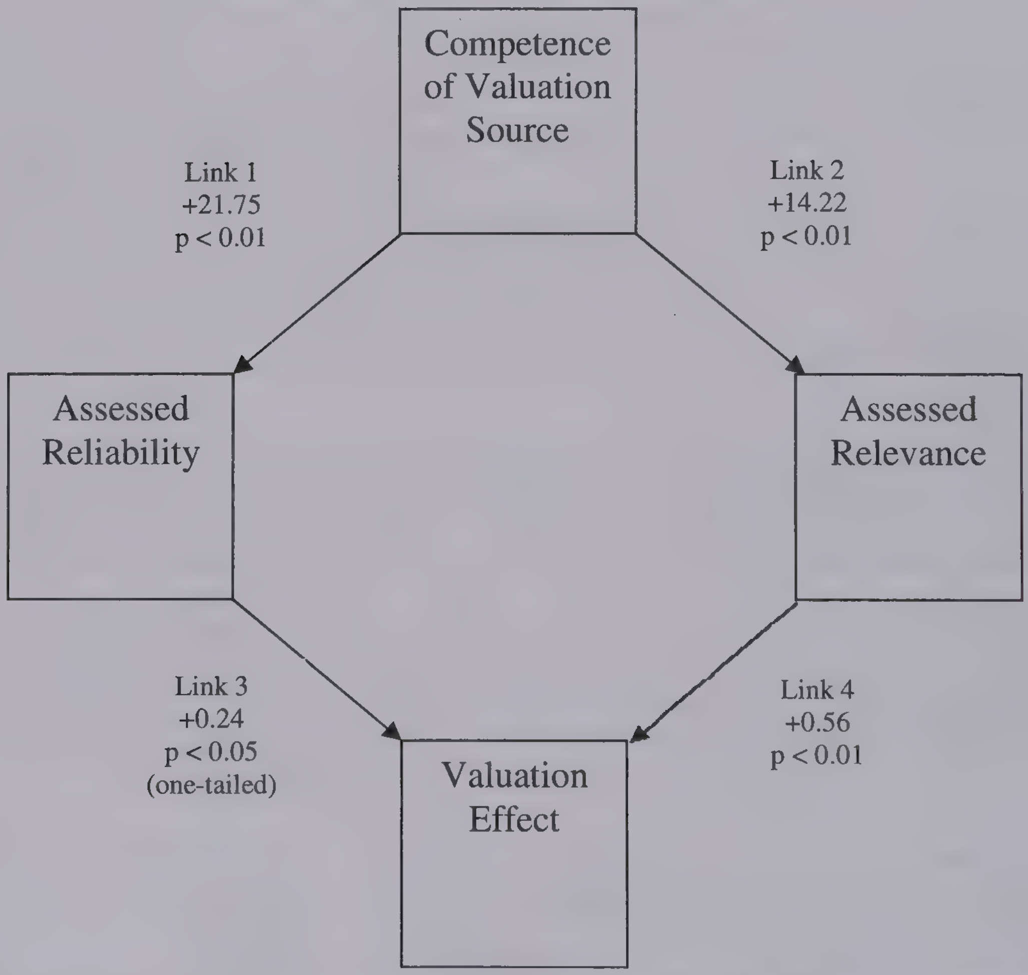
Participants assessed the relevance of the current value information to their overall evaluation of the firm. The competence of the external consultants determining the measurement was varied at two levels, high and low. Management's plans to sell the asset were varied at two levels, sell soon versus no sell. Participants provided relevance assessments on a 101-point scale, with 0 labeled "not at all relevant" and 100 labeled "extremely relevant." Participants also assessed whether the current value information increases, decreases, or neither increases nor decreases the value they place on the firm. Participants who indicated a change in value also indicated the magnitude of the change on a 101-point scale, ranging from "slightly increases (decreases) how much I value the company" (0) to "greatly increases (decreases) how much I value the company" (100). Valuation effect is calculated by coding the magnitude of change as a positive amount for participants who indicated increases in value, a negative amount for those who indicated decreases in value, and 0 for participants who indicated no change in value.

reliability. However, because our manipulation of time to sale did not also affect assessed relevance, we cannot be sure that we have effectively manipulated factors underlying relevance. In sum, Experiment 1 allows us to test our H1 (i.e., manipulations of reliability influence relevance judgments). However, it does not allow us to test the idea that the reliability-relevance relationship is unidirectional (i.e., that manipulation of properties underlying relevance do not result in differences in assessed reliability). We are able to explore the latter issue in Experiment 2.

Table 1 presents participants' valuation judgments. The ANOVA results show a significant main effect of source competence on valuation effects ($F_{1,114} = 8.46$, $p < 0.01$). Consistent with the relevance assessments, participants receiving the measurement from a high competence source indicated more positive valuation effects (mean of 34.24) than did those receiving the measurement from a low competence source (mean of 16.12). Time to sale and the interaction of source competence and time to sale are not significant in the ANOVA.

H2 examines the paths by which properties underlying reliability influence valuation judgments. This hypothesis is important for two reasons. First, although we find support for H1, it is possible that the relationship between manipulated source competence and assessed relevance is not meaningful in the sense of influencing valuation. Second, H2 verifies that users incorporate *both* assessed reliability and assessed relevance in their valuations (i.e., that decision usefulness is a function of both assessments). We test our theory underlying both H1 and H2 using a structural-equations model, as presented in Figure 3. Note that our full hypothesized model would also include a link from manipulated factors influencing relevance (time to sale) to assessed relevance. However, because the manipulation of time to sale did not influence participants' assessments of relevance, we omit this variable from the model.

FIGURE 3
Structural-Equations Model, Experiment 1



This figure summarizes results from Experiment 1. It illustrates that manipulated source competence, a property related to reliability, significantly influences users' assessments of both the reliability of the measurement and the relevance of current value information. It further demonstrates that users take their assessments of both reliability and relevance into account when determining how much weight to put on the reported information in valuing the firm.

Overall, the model fits the data well. The traditional $\chi2$ test shows a good fit ($\chi2 = 0.99$, $p = 0.32$), as do other standard fit measures.[11] Turning to the path coefficients, the relationship between our manipulated source competence variable and assessed reliability (Link 1) is positive and significant ($p < 0.01$), consistent with a successful manipulation of factors underlying reliability. The relationship between competence of the valuation source and assessed relevance (Link 2) is positive and significant ($p < 0.01$), consistent with H1. Finally, both assessed reliability (Link 3) and assessed relevance (Link 4) are, in turn, positively associated with the valuation effect ($p < 0.05$ and $p < 0.01$, respectively). Both of the indirect effects of source competence on valuation (through assessed reliability and through assessed relevance) are significant ($p < 0.01$) by the product of Z-Scores test (Holbert and Stephenson 2003), indicating that both paths are meaningful. These results support H2.

These Experiment 1 results support our theory that variations in source competence cause users to assess differences in reliability, as one would expect. Moreover, our results also support our theory by showing that reliability has an additional effect on relevance judgments. Both assessed reliability and assessed relevance, in turn, affect valuation judgments (i.e., their joint influence is decision useful). These results strongly suggest that properties underlying reliability have too much emphasis in valuation.

IV. EXPERIMENT 2

Overview

Experiment 1 provides evidence supporting our theory that variations in a property underlying reliability (i.e., source competence) influence the assessed relevance of current value information. Additionally, Experiment 1 supports the idea that source competence, as a property of reliability, influences users' valuation judgments by two distinct paths—through both assessed reliability and assessed relevance.

In our second experiment, we test whether our theory holds for a second property related to the reliability of a measurement—whether the measurement arises from a market transaction of a comparable item or from a model. We also attempt a stronger manipulation of factors underlying relevance in Experiment 2, to allow a test of the directionality of the relationship between relevance and reliability. The use of different manipulations of the underlying properties of reliability and relevance is also helpful in demonstrating the generalizability of our theory.

Participants, Design, and Variables

Ninety-nine M.B.A. students from a *BusinessWeek* top-25 school volunteered to participate in Experiment 2 in return for a small fixed payment. The participants had an average of 5.0 years of work experience. Seventy-two percent of participants had previously made investments in common stocks, and 97 percent either had invested or planned to invest in common stocks in the future. Participants had taken an average of 4.7 finance and 3.0 accounting classes. We paid participants \$10 for their participation in this and an unrelated study.[12]

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[11] The Comparative Fit Index, a measure of the proportion of improvement of the fit of our model to the null model, is 1.00, which is above the generally accepted minimum value of 0.95 (Byrne 2001). The Incremental Fit Index (1.00) is above the recommended minimum of 0.95 (Byrne 2001), and the Root Mean Square Error of Approximation (0.00) is below the recommended maximum of 0.01.

[12] Participants completed the unrelated study before participating in Experiment 2. We counterbalanced conditions of the other study with those of our experiments, alleviating the potential for carryover effects. Statistical tests reveal no systematic carryover effects.

As in Experiment 1, we asked participants to assume the role of a potential investor in a fictitious company. All were told that the company had acquired a plot of land for $41 million two years ago that was recently estimated to have a current value of $53 million. Similar to the first experiment, Experiment 2 makes use of a 2 × 2 full-factorial, between-participants design. We manipulated underlying properties of both the reliability of the current value measurement and the relevance of current value information. Specifically, we manipulated the valuation method used in deriving the land's current value as based on the market price of a comparable asset (e.g., Level 2 of SFAS 157 valuation hierarchy) or estimated from a model (e.g., Level 3). We did not use SFAS 157 terminology in our materials. The two levels correspond to high and low reliability and are referred to as "comparable" and "model," respectively.[13]

We manipulated the intended use of the asset as an underlying property of relevance.[14] At one level ("sell," corresponding to high relevance), the land is owned by a real estate development firm that holds the land for development and sale. At the other level ("use," corresponding to low relevance), the land is owned by a cosmetics manufacturing firm that holds it for purposes of building its own factory. Intended use incorporates time to sale (i.e., presumably the real estate developer will sell the land first, making its fair value potentially more timely) and separability of the asset from other operations (i.e., the separate value of land to be developed and sold is likely more relevant for the assigned valuation task than is the fair value of land that is part of productive capacity, because the latter would be valued "in use" rather than separately) (SFAS 157). Figure 4 illustrates the manipulations.

After receiving the information described above, participants made four assessments. The first three response measures are identical to the first three measures captured in Experiment 1 (i.e., relevance of the current value information [i.e., the construct], reliability of the number assigned to the land's current value [i.e., the measurement], and valuation effect of the information). Participants also assessed management trustworthiness using a 101-point scale ranging from "not at all trustworthy" (0) to "extremely trustworthy" (100).

Results

Preliminary Tests

As anticipated, reliability was assessed higher by those in the comparable conditions (mean of 64.06) as compared with those in the model conditions (mean of 42.60) ($F_{1,95} = 31.76$, $p < 0.01$). The manipulation of intended use (i.e., relevance) did not influence reliability judgments (not tabulated), a point we return to in later tests for this second experiment. That is, assessed reliability was not statistically different in the sell and use conditions (means 54.14 and 52.29, respectively, $F_{1,95} = 0.19$, $p = 0.67$). Further, the interaction of intended use and valuation method was not significant ($F_{1,95} = 0.21$, $p = 0.65$).[15]

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[13] Following the argument of Song et al. (2010), this manipulation is a joint manipulation of bias and potential for error. Specifically, those authors contend that investors' downward valuation adjustment for reported Level 3 fair values is likely due to two factors: (1) a downward adjustment to the cash flow effect of the item due to potential bias on the part of management, and (2) an upward adjustment to the discount rate applied to the reported amount due to uncertainty or noise in the fair value estimate.

[14] The issue of whether intended use *should* normatively affect relevance judgments is subject to debate. For the purposes of this study, however, it is not necessary to establish whether individuals should or should not normatively react to intended use, but only that it will influence assessments of relevance and is unrelated to reliability.

[15] If users expected the real estate company to more reliably estimate the value of the land than the cosmetics company, then our intended use manipulation could be viewed as a manipulation of both relevance and reliability. However, the lack of any significant effects of intended use on judged reliability rules out this possibility.

FIGURE 4
Example of Materials Used in Experiment 2

Panel A: Sell, Model Condition

In Year 1, WRSwann Co. purchased a plot of land for $41,000,000. WRSwann Co. is a real estate development firm that routinely purchases, develops, and sells land. The land is ideally suited for development, given its proximity to dense, high-income neighborhoods and transportation.

In Year 3, WRSwann Co. places a current value on the land in the amount of $53,000,000.

This current value number was generated internally by company management. Management based the current value number on an internally developed model (i.e., marked-to-model). There are no comparable plots of land upon which to determine the current value; hence, management used a model to determine the value.

Year 1 Value	Year 3 Value
$41,000,000	$53,000,000

Panel B: Use, Comparable Condition

In Year 1, WRSwann Co. purchased a plot of land for $41,000,000. WRSwann Co. is a cosmetics manufacturing company that plans to build its factory on the land. The land is ideally suited for the factory, given its proximity to an appropriate workforce and an already established distribution center.

In Year 3, WRSwann Co. places a current value on the land in the amount of $53,000,000.

This current value number was generated by an external valuation firm. This external valuation firm based the current value number on a comparable plot of land (i.e., adjacent to the one purchased by WRSwann, same size, and same location desirability). This comparable plot of land was recently sold.

Year 1 Value	Year 3 Value
$41,000,000	$53,000,000

An ANOVA model for participants' ratings of management's trustworthiness (not tabulated) revealed a significant main effect of valuation method ($F_{1,95} = 13.83$, $p < 0.01$). Participants in the comparable conditions rated management trustworthiness significantly higher than did those in the model conditions (58.78 for comparable versus 49.52 for model). Neither intended use ($F_{1,95} = 0.01$, $p = 0.92$) nor the interaction of intended use and valuation method ($F_{1,95} = 1.22$, $p = 0.27$) is significant in the model. Collectively, these results indicate that our manipulation of the valuation method successfully influenced participants' views of the reliability of the asset's reported value.

Tests of Hypotheses

Descriptive (inferential) statistics for participants' assessments of relevance are presented in Panel A (Panel B) of Table 2. The ANOVA reveals a significant main effect of valuation method ($F_{1,95} = 8.44$; $p < 0.01$) such that participants in the comparable conditions judged the relevance of current value information (mean of 68.49) significantly higher than did those in the model conditions (mean of 53.94). Thus, underlying factors (e.g., error and bias) of the reliability of a current value measurement influence financial statement users' assessments of the relevance of current value information, supporting H1. Consistent with the findings from Experiment 1, this supports our theory that users base their evaluations of relevance on the more accessible reliability attribute. Support for participants' reliance on attribute substitution is also indicated by a positive and highly significant correlation of assessed reliability and relevance ($r = 0.48$, $p < 0.01$).

The ANOVA for relevance also reveals a significant main effect of intended use ($F_{1,95} = 4.97$; $p = 0.03$) and an insignificant interaction of valuation method and intended use ($F_{1,95} = 0.19$; $p =$

TABLE 2

Relevance Assessments and Valuation Judgments, Experiment 2

Panel A: Mean Judgments (Standard Deviation)

	Assessed Relevance Dependent Variable Valuation Method			Valuation Effect Dependent Variable Valuation Method		
Intended Use	**Model**	**Comparable**	**Row Means**	**Model**	**Comparable**	**Row Means**
Use	49.48	61.75	55.49	15.00	29.96	22.17
	(24.13)	(25.49)	(25.32)	(37.54)	(29.22)	(34.30)
	n = 25	n = 24	n = 49	n = 25	n = 23	n = 48
Sell	58.40	74.96	66.68	23.24	49.40	36.32
	(24.84)	(24.29)	(25.71)	(41.46)	(32.24)	(39.06)
	n = 25	n = 25	n = 50	n = 25	n = 25	n = 50
Column Means	53.94	68.49		19.12	40.08	
	(24.65)	(25.51)		(39.37)	(32.04)	
	n = 50	n = 49		n = 50	n = 48	

Panel B: Analysis of Variance

	Assessed Relevance				Valuation Effect			
Source	**df**	**MS**	**Statistic**	**Two-Tailed p-value**	**df**	**MS**	**Statistic**	**Two-Tailed p-value**
Intended Use	1	3029	F = 4.97	0.03	1	4688	F = 3.71	0.06
Valuation Method	1	5141	F = 8.44	0.01	1	10341	F = 8.18	0.01
Intended Use × Valuation Method	1	113	F = 0.19	0.67	1	768	F = 0.61	0.44
Residual	95	609			95	1264		

Participants assessed the relevance of the current value information to their overall evaluation of the firm. We manipulated whether the current value was derived from a market transaction for a similar asset (e.g., level 2 of SFAS 157 valuation hierarchy) or from a model (e.g., level 3). We also manipulated the intended use of the asset as either "use" (the land is held for purposes of building the owner's factory) or "sell" (the land is held for development and sale). The dependent measures are described in the notes to Table 1.

0.67). The significant effect of intended use arises from participants in the sell conditions assessing higher relevance for current value information (mean of 66.68) than participants in the use conditions (mean of 55.49). This result indicates that we successfully manipulated participants' views of relevance independent of reliability.

The fact that our manipulation of intended use significantly influenced assessed relevance without affecting assessed reliability provides evidence in support of the unidirectional relationship between relevance and reliability. While factors underlying reliability affect both assessed reliability and assessed relevance, factors underlying relevance influence only assessed relevance. To further test the direct effect of these underlying properties on users' assessments of reliability and relevance as well as the indirect effect of these properties on users' valuation decisions, we employ structural equations analysis.

As shown in Table 2, an ANOVA model for participants' valuation judgments reveals significant main effects of intended use and valuation method. Valuation judgments were more positive when the current value was based on a comparable market transaction (mean of 40.08) versus a model (mean of 19.12) ($F_{1,95} = 8.18$, $p < 0.01$). They also were more positive when the company intended to develop and sell the asset (mean of 36.32) than when they were intending to use the asset in operations (mean of 22.17) ($F_{1,95} = 3.71$, $p = 0.06$). There was no evidence of an interaction of these two variables ($F_{1,95} = 0.61$, $p = 0.44$).

For H2, recall that we predict that factors underlying the reliability of a current value measurement will influence valuation via two paths—through users' assessments of the reliability of the measurement and through their assessments of relevance. Thus, we expect that the valuation method (i.e., comparable versus model) will influence both assessed reliability and assessed relevance and that each will separately influence valuation. We test H2 using a structural-equations model, as presented in Figure 5.

The model is similar to that tested in Experiment 1, except that we include in the model the intended use variable, which significantly influenced participants' relevance judgments. Before exploring the links in the model, we first test the overall fit using various indices. The traditional $\chi 2$ test shows a good fit ($\chi 2 = 1.92$, $p = 0.59$), as do other standard fit measures.[16] Thus, our model describes well the relations in the data.

Figure 5 presents path coefficients and statistical significance. The relationship between valuation method, with model coded as 0 and comparable coded as 1, and assessed reliability (Link 1) is positive and significant ($p < 0.01$), consistent with a successful manipulation of factors underlying reliability. Consistent with H1, the relationship between valuation method and assessed relevance (Link 2) is also positive and significant ($p < 0.01$). Further, both assessed reliability (Link 3, $p < 0.01$) and assessed relevance (Link 4, $p < 0.01$) are significantly associated with valuation, indicating that both influence decision usefulness. Finally, both indirect effects of valuation method on valuation—one through assessed reliability and the other through assessed relevance—are significant ($p < 0.01$, product of Z-scores test), indicating there are two distinct and meaningful paths by which factors underlying reliability affect valuation. These findings support H2.

Further analysis of our results shows that while properties underlying reliability influence relevance judgments, the reverse is not true. That is, properties associated with relevance do not influence reliability judgments. To see this, turn to our manipulation of intended use of the asset (with "use" coded as 0 and "sell" coded as 1). There, we see a significant positive relationship with assessed relevance (Link 6, $p = 0.02$), indicating a successful manipulation of factors underlying

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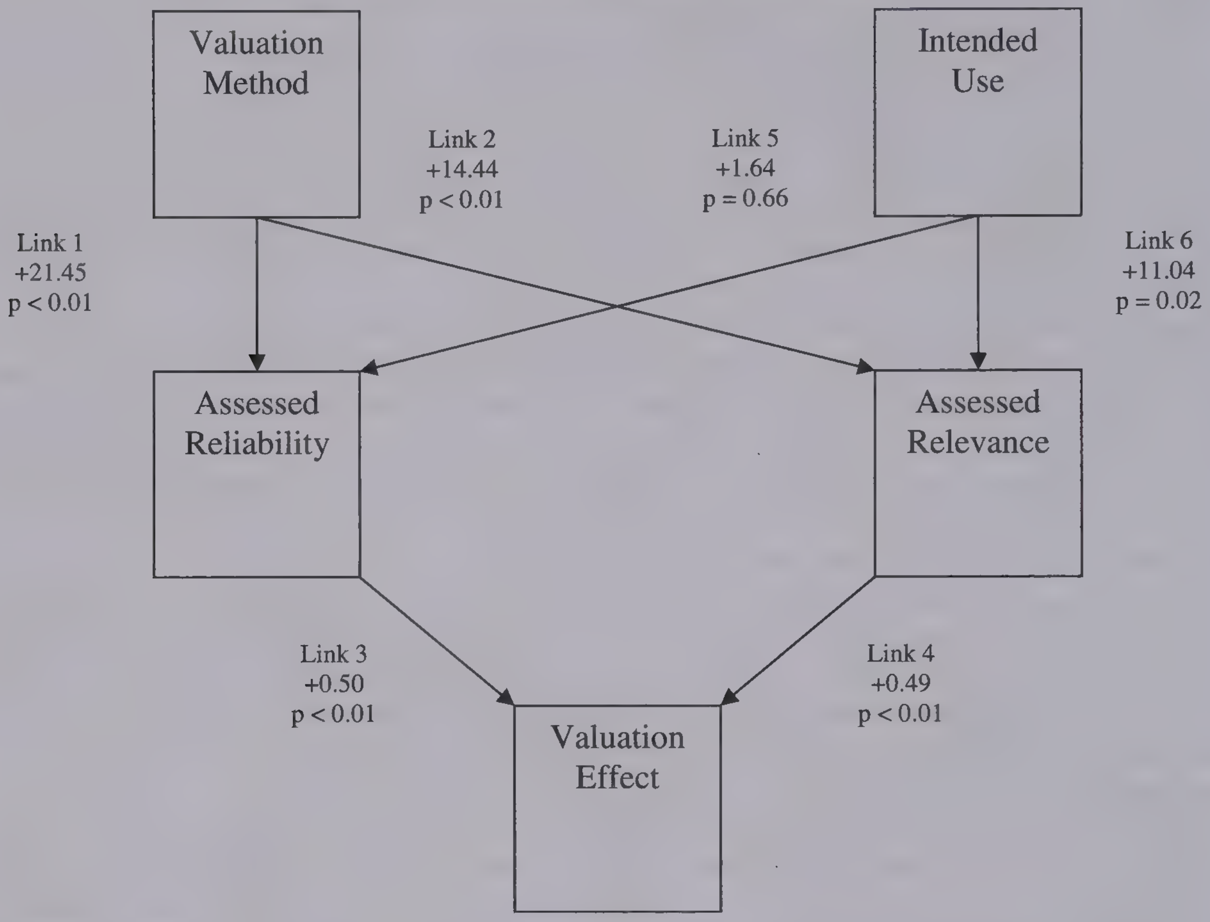
[16] The Comparative Fit Index is 1.00, which is above the generally accepted minimum value of 0.95 (Byrne 2001); the Incremental Fit Index (1.01) is above the recommended minimum of 0.95 (Byrne 2001); and the Root Mean Square Error of Approximation is 0.00, which is below the recommended maximum of 0.01.

FIGURE 5
Structural-Equations Model, Experiment 2



This figure summarizes results from Experiment 2. It illustrates that manipulated valuation method (i.e., comparable transaction versus model), a property related to reliability, significantly influences users' assessments of both the reliability of the measurement and the relevance of current value information. It demonstrates the directionality of this "confusion" between reliability and relevance by showing that the assets' intended use (use as a factory site versus held for sale), a determinant of relevance, significantly influences assessed relevance but does not influence assessed reliability. It further demonstrates that users take their assessments of both reliability and relevance into account when determining how much weight to put on the reported information in valuing the firm.

relevance. Most pertinent to our purposes, the link between the intended use variable and assessed reliability is insignificant (Link 5, $p = 0.66$), indicating that the reliability-relevance relationship is strictly unidirectional, as we posit.

An alternative specification of this model includes a link from assessed reliability to assessed relevance instead of the link from valuation method to assessed relevance. This model is computationally equivalent to the model we test for Experiments 1 and 2, and so fit measures are identical. Both our model and the alternative model are consistent with our theory, as they imply that evaluations of relevance rely on the more accessible reliability attribute.

Ruling Out an Alternative Substitute Attribute

Our theory predicts that users have difficulty assessing relevance, and so will substitute the reliability attribute for relevance. We believe reliability to be the most likely candidate for attribute substitution. Reliability is a basic property of measurement that prior research indicates individuals assess relatively accurately, and thus it is highly accessible. Results of our experiments support this theory. However, an alternative possibility is that users substitute decision usefulness for relevance. That is, they may use the label "relevance" to capture the idea of decision usefulness, which includes the effects of both relevance and reliability (FASB 1980, 2010). We conduct additional analyses to rule out this possibility.

First, recall that the path analysis in Experiment 2 shows that manipulations of properties underlying relevance and reliability affect participants' valuation judgments.[17] If participants used the relevance label to capture decision usefulness, then we would observe the assessed relevance measure capturing *all* effects of factors underlying both relevance and reliability on our valuation dependent measure. However, our analysis does not support this result. Instead, as seen in Figure 5 (Link 3), assessed reliability has a significant effect on valuation above and beyond the impact of assessed relevance on valuation. As a further test of this same idea, we estimated a model identical to that in Figure 5, except that we omitted Link 3. This model also tests the idea that judged relevance is used as a proxy for decision usefulness. The alternative model is a poor fit to the data, and a nested model test shows that our model fits the data significantly better ($\chi_1{}^2 = 8.482$, $p < 0.01$). These results indicate that our participants did not consider relevance as a substitute for decision usefulness.

V. ROBUSTNESS TESTS

We conducted two additional experiments to test the robustness of the findings from our main experiments. First, to determine whether these results hold even if participants are provided with the FASB's definitions of relevance and reliability, we undertook a third experiment with the same design as Experiment 2 with a separate group of 94 participants from a similar population (M.B.A. students with mean work experience of 8.4 years, average of 3.5 finance and 3.1 accounting classes, 81 percent had previously invested in common stocks, and 89 percent planned to do so). Our results are similar to those reported for Experiments 1 and 2. That is, the manipulation of valuation method (i.e., market price of a comparable asset versus model estimate) influenced participants' assessments of relevance ($F_{1,90} = 7.38$; $p < 0.01$) despite the prominent display of FASB's definitions in the materials. Participants assessed relevance as higher in the comparable conditions (i.e., high reliability; mean of 75.60) than in the model conditions (i.e., low reliability; mean of 62.95).

Second, to ensure that participants interpreted the relevance question as referring to the general construct of current value information (versus a specific measurement), we conducted a fourth experiment based on Experiment 2 with a separate group of 47 M.B.A. student participants (mean work experience of 8.4 years, average of 3.8 finance and 3.6 accounting classes, 72 percent had previously invested in common stocks and 91 percent planned to do so). We used the model and comparable conditions for the high relevance condition (i.e., sell) from Experiment 2, with one change. We asked about the relevance of "current value information" rather than "*the* current value information." Again, we replicated our Experiment 2 findings. Assessments of relevance were significantly higher in the comparable condition than in the model condition (68.5 versus 50.1, $F_{1,45} = 6.68$; $p = 0.01$).

VI. CONCLUSIONS

In this study, we experimentally investigate the hypothesis that factors underlying the reliability of a fair value measurement influence financial statement users' assessments of the relevance of fair value. Such an assessment is inconsistent with the qualities of relevance and reliability being independent constructs. We base our tests on attribute substitution theory, which predicts that this relationship arises because financial statement users can access more readily the reliability attribute than the relevance attribute. Thus, when asked to assess relevance, financial statement users substitute an easier question—one about reliability—in forming a response. Our results support our hypothesis, revealing that factors underlying the reliability of a fair value measurement—namely, the competence of the source of the measurement (Experiment 1) and whether the measurement is based on a market transaction for a comparable item or a model calculation (Experiment 2)—strongly influence users' assessments of the relevance of fair value information. Attribute substitution further predicts that this relationship will be unidirectional. That is, when asked to assess reliability, financial statement users will do so rather than substitute the relevance attribute. Our results also support this assertion.

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[17] We did not conduct this test for Experiment 1 as it is less meaningful there, because the manipulation of time to sale (i.e., relevance) showed an insignificant main effect on our dependent measures.

We also demonstrate that factors underlying the reliability of a measurement affect users' valuation judgments through two paths: (1) their influence on assessed reliability, and (2) their influence on assessed relevance. Both of these paths are highly significant in our experiments. While these results are highly suggestive of an over-weighting of factors underlying reliability in valuation, without a normative benchmark we cannot be certain that this is occurring. Future research can draw on our insights to further explore this issue.

Future research also could examine boundary conditions on attribute substitution for relevance and reliability. That is, does what we observed in a fair value context hold in other financial reporting situations? In many financial reporting contexts, users receive a measurement possessing a given level of reliability and relevance, and relevance is typically more difficult to assess than reliability. Thus, we expect the results we observed to obtain generally in these contexts. However, we caution readers that our results may not generalize to other financial reporting contexts in which relevance is made particularly salient, and is therefore more accessible, or in contexts in which reliability is particularly difficult to assess. In such cases, attribution substitution either would not occur or would take a different form than we observe.

Future research could further explore the extent to which financial statement users are aware of the attribution substitution. Recall that attribute substitution theory indicates that individuals are generally unaware of the substitution they are making. Although we did not design our tests to investigate the role of awareness, we nevertheless report results that are consistent with theory, with no obvious alternative explanation for the observed effects. Future research could address the awareness issue by employing, for example, verbal protocol analysis to determine if users are aware of their use of factors underlying reliability when assessing relevance. If this research shows that individuals are aware of the substitution, additional research also could investigate whether participants purposefully use reliability information when judging relevance. We believe such intent is unlikely, since the substitution effect persists when we gave participants definitions of the terms. The fact that this substitution occurs is important regardless of awareness or intentionality, given that it is at odds with a view of relevance and reliability as independent constructs.

This study has important implications for standard setters, firm management, and financial statement users. First and foremost, our findings suggest that financial statement users are likely to make errors in judgments. Because these errors are apparently driven by the difficulties of assessing relevance and the resulting use of a heuristic, training may not be effective in correcting the problem. For example, even individuals with extensive statistical training ignore base rates and rely on representativeness when making intuitive judgments (Tversky and Kahneman 1974, 1130).

The phenomena that we document are also likely to interfere with standard setters' efforts to expand the use of fair value in financial reports. Observers focusing on unreliable fair value measurements have argued that fair values are irrelevant, as a result. We note that this is akin to asserting that the outdoor temperature is not relevant to one's decision whether to wear an overcoat simply because the thermometer at hand is unreliable. A better understanding of the underlying issue—users' inability to assess relevance independently from reliability—indicates that standard setters may wish to focus on addressing how to communicate the relevance of the particular constructs separate from their efforts to improve the reliability of their measurements.

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Investor Sentiment and Stock Market Response to Earnings News

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ABSTRACT: We examine whether market-wide investor sentiment influences the stock price sensitivity to firm-specific earnings news. Using the recently developed measure of investor sentiment by Baker and Wurgler (2006), we find that the stock price sensitivity to good earnings news is higher during high sentiment periods than during periods of low sentiment, whereas the stock price sensitivity to bad earnings news is higher during periods of low sentiment than during periods of high sentiment. This influence of sentiment is especially pronounced for the earnings news of small stocks, young stocks, high volatility stocks, non-dividend-paying stocks, and stocks with extremely high and low market-to-book ratios. Further analysis suggests that the sentiment-driven mispricing of earnings contributes to the general mispricing of stocks due to investor sentiment.

Keywords: *investor sentiment; corporate news; event studies; behavioral finance.*

JEL Classifications: *D14; D21; G24.*

I. INTRODUCTION

Accounting research has long been interested in the valuation implications of corporate earnings. Traditionally, these studies have been couched in an efficient markets framework that postulates that accounting information is efficiently impounded into stock prices by rational agents in well-functioning capital markets (Holthausen and Watts [2001] and Kothari [2001] survey this research). In fact, in many cases, researchers rely on the maintained assumption of the efficient pricing of information and use the change in stock prices around an

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information event as a reliable yardstick to quantify the effect of that event on shareholder value. This view precludes any significant role of investors' behavioral biases in shaping their response to accounting information. Developments in the field of behavioral finance, however, are increasingly spurring accounting researchers to question this view (Lee 2001; Daniel 2004; Hirshliefer and Teoh 2003; Hirshliefer and Teoh 2009). Lee (2001), for instance, challenges the traditional assumption that the price adjustment to new information is always rational and instantaneous, and calls for more work that incorporates the findings of behavioral finance into capital market research in accounting to better understand the process by which stock prices incorporate accounting information.

In this paper, we examine the impact of investors' behavioral biases, measured by market sentiment, on the sensitivity of stock prices to earnings surprises (Earnings Response Coefficients [ERCs]). Market sentiment can be broadly defined as the beliefs about future cash flows or discount rates that are not supported by the prevailing fundamentals (Baker and Wurgler 2006). Prior literature on market sentiment has established that (1) reasonable proxies of investor sentiment can be identified, and (2) sentiment has discernible and regular effects on the stock market (Brown and Cliff 2005; Baker and Wurgler 2006; Lemmon and Portniaguina 2006; Kaplanski and Levy 2010). A key finding of this literature is that when sentiment is high, investors' propensity to speculate is high, so they place excessively optimistic valuations on future expected cash flows associated with risky assets such as stocks, either by overestimating the size of the cash flows or by underestimating the risk, leading them to overvalue stocks. The reverse is true for the low sentiment periods, when investors place excessively pessimistic valuations on future expected cash flows, and undervalue stocks.[1] This contemporaneous misvaluation due to sentiment reverses in the future, thereby creating a negative relation between sentiment and future risk-adjusted returns, especially for more speculative stocks.

We extend this misvaluation argument of Baker and Wurgler (2007) to the setting of earnings announcements. We note that an earnings announcement brings with it information about future earnings and cash flows of the firm (Kasznik and McNichols 2002), and the stock price response to the announcement entails investors' valuation of these *incremental* cash flows. Following the argument of the sentiment literature, investors are likely to place optimistic valuations on the incremental cash flows embedded in earnings announcements during periods of high sentiment and pessimistic valuations during periods of low sentiment. For good earnings news, which generally signifies positive incremental cash flows, an optimistic valuation during high sentiment periods and pessimistic valuation during low sentiment periods implies a greater stock price increase during high sentiment periods than during low sentiment periods. For bad news, on the other hand, which generally signifies negative incremental cash flows, an optimistic valuation during high sentiment periods and a pessimistic valuation during low sentiment periods implies a smaller stock price reduction during high sentiment periods than during low sentiment periods. Therefore, in this paper, we test the hypothesis that the ERC of good (bad) earnings news is higher (lower) when sentiment is high than when sentiment is low, leading to a positive (negative) association between the ERC of good (bad) news and investor sentiment.[2]

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[1] The limits on arbitrage constrain rational traders from forcing the prices back to their fundamental value (Baker and Wurgler 2006, 2007).

[2] While we follow the "top-down" approach of the sentiment literature (see, for example, Baker and Wurgler 2007), and motivate our hypothesis using the broad notion of optimistic and pessimistic valuation by investors during high or low sentiment periods, several specific *individual* biases of investors, which are well-documented in the behavioral finance literature, also produce similar predictions. For instance, Barberis et al. (1998) consider *conservatism* and *representativeness* and show that investors overreact to the new information when it conforms to their prior beliefs and underreact when it contradicts the prior beliefs. Similarly, under *confirmatory bias*, individuals interpret new evidence based on whether the new information conforms or contradicts their prior beliefs (Barberis and Thaler 2003; Hirshliefer 2001).

The primary proxy for investor sentiment in our analysis is the monthly sentiment index developed by Baker and Wurgler (2006). We use the standard event study methodology to compute the three-day abnormal returns around the announcement of earnings as our measure of stock price sensitivity to news. Our results confirm that the stock price sensitivity to good news increases with sentiment, whereas the stock price sensitivity to bad news decreases with sentiment. Consistent with the notion that the incremental cash flows embedded in bad earnings announcements are more uncertain and difficult to value than the incremental cash flows that are embedded in good earnings news, we find that the effect of sentiment on stock price sensitivity is significantly stronger for bad news than for good news. We also validate the prediction in the recent sentiment literature that the effect of sentiment persists over a prolonged period and does not reverse immediately in the short term (Brown and Cliff 2005; Baker and Wurgler 2006). Specifically, we find that the effect of sentiment persists and influences the drift in stock prices in the subsequent 60 days after the earnings announcement.

We also connect the sentiment-related mispricing of earnings to the broader effect of sentiment on prices as delineated by the previous literature. Despite several papers that establish a relation between sentiment and mispricing of stocks (Baker and Wurgler 2006; Lemmon and Portniaguina 2006; Qiu and Welch 2006), the precise channel(s) by which sentiment causes such mispricing is not well understood. Because earnings announcements represent important events that lead to abnormally high trading, especially by small traders,[3] we hypothesize and document that the sentiment-driven misreaction of stock prices to earnings news contributes importantly to the general overpricing (underpricing) of stocks during periods of high (low) sentiment documented in the prior literature. Specifically, in bullish (bearish) times, the market overreacts (underreacts) to good earnings news and underreacts (overreacts) to bad news, and this contributes significantly to the overpricing (underpricing) of stocks during bullish (bearish) times.

The prior literature also argues that the effect of sentiment on the valuation of stocks is not uniform across stocks, and is greater for speculative stocks whose expected cash flows are more uncertain and more difficult to value (Baker and Wurgler 2006; Lemmon and Portniaguina 2006). In our setting, sentiment affects the (mis)pricing of earnings because it induces investors to misvalue the *incremental* cash flows embedded in the earnings announcements. Because the earnings of speculative stocks are often also less persistent (Baginski et al. 1999), it can make the identification and valuation of the associated incremental cash flows harder and more subjective, leading to a greater effect of sentiment in the pricing of the earnings of such stocks. Therefore, we investigate and find that the effect of sentiment on the (mis)pricing of earnings is greater for small, young, volatile, non-dividend-paying, and extremely high- and low-growth stocks than for the corresponding categories of large, mature, stable, high-dividend-paying, and medium-growth stocks, respectively.

An alternative explanation for our results could be that good (bad) news during high (low) sentiment periods has greater inherent information content than good (bad) news during low (high) sentiment periods. The higher ERC of the good (bad) news during high (low) sentiment periods might, therefore, simply reflect the greater information content of the news during such periods. Using the measure of information content of earnings developed by Kasznik and McNichols (2002; hereafter, KM), we find, however, no evidence in support of this information-based story.

We also rule out risk-based explanations for our results for three reasons. First, we compute the abnormal returns after controlling for the known risk measures of size, book-to-market ratio, and momentum. Second, we accumulate the returns over a three-day window and the variation of risk over time is less likely to be an explanation for such a short return accumulation period. Finally, if sentiment captures time-varying discount rates in some way, then the effect for good and bad news should be similar, but we find that the ERC for good news increases, whereas the ERC for bad news decreases with sentiment.

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[3] Lee (1992) and Battalio and Mendenhall (2005), for example, find that while both large and small traders increase their trading around earnings announcements, small traders do so to a greater extent.

Our key finding is that investors' behavioral biases affect how accounting information is impounded into prices. This casts a doubt on the traditional view that the short-term change in stock price is an unbiased and efficient benchmark for assessing the valuation implication of accounting information. As Lee (2001) notes, if the stock price reaction to accounting information contains both rational and irrational components, then the change in stock price around the announcements of accounting information cannot be used by researchers as the *sole* reliable yardstick to assess the value-relevance of the information. The tests of value-relevance need to be bolstered to include measures of *long-term* accounting, product-market, and stock price performance.

Our paper also helps to advance the literature on investor sentiment in two ways. First, we identify mispricing of earnings as an important channel through which sentiment can cause mispricing of stocks. Second, an alternative explanation for the relation between sentiment and stock returns documented in prior work is that sentiment captures time-varying discount rates in some way. If sentiment just reflected time-varying discount rates, then it should have roughly equal effects on both good and bad news in our setting, but we find that this is not the case, further validating the sentiment proxies developed in the prior literature.

Our paper complements two other concurrent papers on the interplay of accounting information and investor sentiment. Seybert and Yang (2012) show that a significant part of the negative stock returns following high sentiment periods is concentrated around management forecast dates. Their study provides insight into how sentiment-related earnings mispricing is later corrected. Hribar and McInnis (2012) examine whether analyst forecast errors of earnings and earnings growth vary with sentiment. They show that these *ex post* forecast errors seem to explain a sizable fraction of the return patterns in Baker and Wurgler (2006). Our paper is different from these studies in that we examine how sentiment affects the initial pricing of earnings information, and how the reversal of this effect contributes to the relation between sentiment and future returns.

Section II describes our data-selection process and research methodology. Section III reports our main results on the effect of sentiment on stock price sensitivity to earnings news. Section IV provides additional analyses, which suggest that sentiment also influences the post-earnings announcement drift, that large earnings surprises affect the relation between sentiment and future returns, and that the pricing effects of sentiment vary cross-sectionally in a way that is consistent with the underlying theory. We provide robustness checks in Section V and conclude the paper in Section VI.

II. DATA AND RESEARCH DESIGN

Data and Sample Selection

Our sample period is 1972 to 2007 for which we could obtain both the Compustat quarterly earnings data and the Baker-Wurgler investor sentiment index. We identify the earnings announcement dates from Compustat and I/B/E/S by following the procedure outlined in DellaVigna and Pollet (2009). For each earnings announcement, we define a three-day event window centered on the date of the event, and calculate the abnormal return for each day as per the method described in Daniel et al. (1997; hereafter, DGTW). This methodology allows for the differences in expected returns that might be related to size, book-to-market ratio, and past returns. Specifically, we calculate the daily abnormal return by subtracting the return on a portfolio that is similar to the stock in terms of size, book-to-market, and past return momentum. We obtain the daily stock returns data from the Center for Research in Security Prices (CRSP). The daily abnormal returns are summed to compute the cumulative abnormal return for each earnings announcement.

Rather than using analyst forecasts, we use seasonally differenced earnings changes as a proxy for unexpected earnings associated with each earnings announcement for three reasons. First, the random walk model of expected earnings allows us to use a longer sample period of 1972–2007, whereas the analyst forecast data are only available starting from 1985. A long sample period gives more power to our tests to explore the role of investor sentiment, which changes slowly over time, and ensures that one or two extreme episodes such as the bull market of the late 1990s do not drive our results. Second, analyst forecasts for quarterly earnings are missing in I/B/E/S for small companies.[4] These, however, are precisely the companies that Baker and Wurgler (2006) argue are more prone to sentiment, and are the most suitable for studying the importance of sentiment in financial markets. Large firms, in contrast, may not be affected by sentiment, or may even be affected in the opposite direction as investors perceive these as safe havens (i.e., blue chip investments) in times of low sentiment (Baker and Wurgler 2007). Finally, Battalio and Mendenhall (2005), among others, show that less-sophisticated small investors rely on time-series forecasts as their expectations of earnings. Since small investors are commonly perceived to be more prone to sentiment (Baker and Wurgler 2007; Kumar and Lee 2006), earnings surprises based on time-series forecasts are more suited to study how sentiment drives investor reaction to earnings news. Nevertheless, in a later section, we report the results for earnings surprises that are computed relative to analyst forecasts.

Following Livnat and Mendenhall (2006), we calculate the unexpected earning, *UE*, which represents the news component associated with the earnings announcement, as follows:

$$UE_{iq} = \frac{Earnings_{iq} - Earnings_{iq-4}}{P_{iq}}. \tag{1}$$

$Earnings_{iq}$ is the primary earnings per share of firm i for quarter q, and P_{iq} is the price per share at the end of quarter q. As in Livnat and Mendenhall (2006), we exclude "special items" in computing the earnings. We take into account stock splits and to mitigate the impact of the erroneous and extreme observations, we trim the distribution of the unexpected earnings at 1 percent and 99 percent. Finally, because prior research shows that the earnings response coefficients are essentially zero for negative earnings (Hayn 1995; Lipe et al. 1998), we delete observations with negative earnings.[5]

In much of our analysis, we split earnings news into good news and bad news because our prediction as to whether earnings are overpriced or underpriced during a certain state of sentiment depends on whether the news is good or bad. We first create two indicator variables, *Up* and *Down*, where *Up* (*Down*) equals 1 if the unexpected earnings is positive (negative), and 0 otherwise. We then multiply *UE* by these indicator variables to yield *UEUp* and *UEDown*, which are our measures of good and bad earnings news, respectively.

To measure investor sentiment, we employ the sentiment index developed by Baker and Wurgler (2006, 2007), which is available on a monthly basis.[6] The index is based on six measures of investor sentiment: closed-end fund discount, NYSE share turnover, number of IPOs, first day returns on IPOs, share of equity issues in total debt and equity issues, and dividend premium (the log difference of the average market-to-book ratios of payers and non-payers). Each of these six measures is first regressed on macroeconomic variables that capture variations in the business cycle, including growth in industrial production index, consumer durables, consumer nondurables, and consumer services. The overall sentiment index is the first principal component of the residuals from the preceding six regressions. The index is standardized to yield a mean of 0 and a standard deviation of 1. Baker and Wurgler (2006, 2007) demonstrate that this sentiment index lines up well with anecdotal accounts of investor exuberance and panic. They also validate the index by providing evidence of the link between the index and the time-series variation in the cross-sectional returns that cannot be explained by rational risk-based models.

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[4] Prior studies have recognized the large-firm bias of I/B/E/S data. For instance, Hong et al. (2000), who examine *annual* earnings forecasts, find that analyst forecasts are missing in some years of their sample for about 82 percent of the stocks that fall in the lowest size quintile. The quarterly earnings forecasts in I/B/E/S are sparser than the annual forecasts. For our sample, we find that in the mid-1980s only 5 to 10 percent of the CRSP stocks in the lowest size quintile have quarterly earnings forecasts available in I/B/E/S.

[5] Nevertheless, in Section V we report the results of our analysis for the subsample of loss firms.

[6] At the time of our study, the sentiment data were available through the end of 2007. We thank Jeffrey Wurgler for making available the data on his website (http://pages.stern.nyu.edu/~jwurgler/).

Regression Specification

We measure stock market sensitivity to earnings news by the elasticity of stock prices to unexpected earnings, or ERC, at announcement dates. Our primary hypothesis is that the ERC of good news is higher during periods of high sentiment, whereas the ERC of bad news is higher during periods of low sentiment. Investors are likely to place optimistic valuations on the incremental cash flows embedded in earnings announcements during periods of high sentiment and pessimistic valuations during periods of low sentiment. For good earnings news, an optimistic (pessimistic) valuation during high (low) sentiment periods implies a positive relation between the ERC and investor sentiment. For bad earnings news, an optimistic (pessimistic) valuation during high (low) sentiment periods implies a negative relation between the ERC and investor sentiment.

To test our hypothesis, we estimate the following regression model:

$$\begin{aligned} RET_{it} = {} & \alpha_0 + \alpha_1 Down_{it} + \beta_0 UEUp_{it} + \beta_1 UEUp_{it} \times Sent_{t-1} + \gamma_0 UEDown_{it} \\ & + \gamma_1 UEDown_{it} \times Sent_{t-1} + \beta_2 NonlUp_{it} + \beta_3 UEUp_{it} \times MktPE_{t-1} + \gamma_2 NonlDown_{it} \\ & + \gamma_3 UEDown_{it} \times MktPE_{t-1} + \varepsilon_{it}. \end{aligned} \tag{2}$$

RET_{it} is the cumulative abnormal return for firm i during the earnings announcement event window from day -1 to $+1$ centered on the earnings announcement date in calendar month t. Note that the cumulative abnormal returns are free of the general effect that sentiment has on similar non-announcing stocks since these returns are computed after subtracting the returns on a portfolio of stocks that are similar in size, book-to-market ratio, and past return momentum. Therefore, the abnormal returns represent the differential impact sentiment has on the announcing stocks relative to that of the non-announcing stocks. Because *UEUp* and *UEDown* depict good and bad news separately, the specification in Equation (2) permits the coefficient for *UE* to be different, conditional on the sign of the earnings surprise. We include the indicator variable *Down* as a stand-alone variable to account for the difference in the intercepts of good and bad earnings news (Bartov et al. 2002). Due to the cross-correlation in the returns of announcing firms, our significance tests for Equation (2) are based on the standard errors that are clustered by calendar month.

We multiply the earnings surprise announced in month t with the sentiment at the end of month t–1 to create the interaction variable, $UEUp \times Sent$. This allows us to test whether the ERC of good earnings news varies with the prevailing investor sentiment. If investors are swayed by the prevailing sentiment in their response to earnings news, we expect the coefficient of this interaction term, β_1, to be *positive*, indicating that the market reacts more to good news when sentiment is high. Similarly, we create the interaction variable, $UEDown \times Sent$, to test whether the ERC of bad earnings news varies with sentiment. We expect the coefficient of this interaction term, γ_1, to be *negative*, indicating that the market reacts less to bad news when sentiment is high.

The model includes control variables for nonlinearity in the ERC that has been documented by prior literature (Freeman and Tse 1992). Since the incidence of large earnings surprises, which cause nonlinearity in the ERC, could potentially vary with sentiment, we account for this possibility in our analysis. Specifically, we use two control variables, *NonlUp*, which is the square of *UEUp*, and *NonlDown*, which is the square of *UEDown* multiplied by −1.

We also control for the relative market price-to-earnings multiple based on the results documented by Conrad et al. (2002). We compute the relative market price-to-earnings ratio (*MktPE*) and add the interaction terms $UEUp \times MktPE$ and $UEDown \times MktPE$ in Equation (2) as additional control variables. We follow Conrad et al. (2002) and compute *MktPE* as the ratio of the market price-to-earnings ratio in the month prior to the month of the earnings announcement to the average market-price-to-earnings ratio in the preceding 12 months. We use all the stocks in the merged CRSP-Compustat database to construct the price-to-earnings ratio of the market for a month. We first compute the price-to-earnings ratio for each stock by dividing the market price of the stock with the most recent annual earnings, and then take the value weighted average[7] of the individual firm ratios to obtain the overall market price-to-earnings ratio.[8]

Controlling for the Information Content of Earnings Surprises

The positive earnings surprises that appear during high sentiment periods could indicate higher growth in future earnings than the positive earnings surprises that appear during low sentiment periods. This could also yield higher ERCs for the positive earnings surprises during high sentiment periods. Correspondingly, if the negative earnings surprises announced in low sentiment periods imply a greater drop in future earnings than the negative surprises announced during high sentiment periods, stock prices should rationally respond more to negative earnings surprises during the low sentiment periods. Investor sentiment would then have nothing to do with the differential response of stock prices to good and bad earnings news across different periods.

To test for this possibility, we draw on the methodology in Kasznik and McNichols (2002) to examine how the estimates of the information content of good and bad earnings news vary with sentiment.[9] Specifically, we estimate the following equation:

$$\begin{aligned} Perf_{iq+1} = {} & \alpha_0 + \alpha_1 E_{iq-3} + \beta_0 UEUp_{iq} + \beta_1 UEUp_{iq} \times Sent + \gamma_0 UEDown_{iq} \\ & + \gamma_1 UEDown_{iq} \times Sent + \varepsilon_{iq+1}. \end{aligned} \tag{3}$$

The subscript q in Equation (3) represents the fiscal quarter whose earnings are announced in month t. We drop the month subscript t from Equation (3) to simplify the notation. $Perf_{iq+1}$ is the earnings for firm i one quarter ahead (E_{iq+1}). E_{iq-3} is the earnings three quarters earlier, and is used in the model as the proxy for expected earnings in quarter q+1, which would be announced later. $UEUp_{iq}$ and $UEDown_{iq}$ represent the earnings news for firm i for quarter q. The coefficients β_0 and γ_0 in Equation (3) represent the estimates of the information content of the good and bad earnings news, respectively, for future earnings when sentiment is zero. If good news has higher information content for future earnings during high sentiment periods compared with low sentiment periods, then β_1 should be positive. Similarly, if bad news has higher information content for future earnings during low sentiment periods than high sentiment periods, then γ_1 should be negative. However, if

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[7] Our results remain robust when we use equal weighting instead.

[8] We note that Conrad et al. (2002) employ the consensus analyst forecast for next year's earnings as the denominator in computing the PE ratios for individual firms. Since analyst forecasts are not available for the earlier period of our sample and we use past earnings as proxy for expected earnings in the rest of our analysis, we therefore divide price by the most recent annual earnings to compute the PE ratio for a firm. Nevertheless, we confirm the robustness of our results for the sub-period 1985–2007, for which analyst forecasts are available, by computing the relative market PE exactly as in Conrad et al. (2002).

[9] We thank the referee for suggesting the Kasznik and McNichols (2002) methodology to test for the informativeness of earnings news.

β_1 and γ_1 are insignificant or of opposite signs, then the differential information content of news across sentiment periods is unlikely to be an alternative explanation for our results.

We also re-estimate Equation (3) after replacing the next-quarter *earnings* as the dependent variable with the next-quarter *cash flows*. This yields an alternative measure of the information content of current-period's earnings news in terms of its implications for future cash flows (CFO_{iq+1}) rather than for future earnings. In this alternate specification, we also include the cash flows three quarters back, CFO_{q-3}, as an additional explanatory variable that depicts the time-series expectation for the future cash flows.

Summary Statistics

Our sample includes 392,212 earnings announcements over the period 1972–2007 for which we could compute the earnings surprises as well as the DGTW risk-adjusted cumulative abnormal returns around the earnings announcement dates. In Table 1, we report the summary statistics of our key variables for the overall sample in Columns (2) through (5). The abnormal announcement and post-announcement returns, $CAR_{(-1\ \text{to}\ +1)}$ and $CAR_{(+2\ \text{to}\ +60)}$, respectively, have both been winsorized at 1 percent and 99 percent of the respective distribution to mitigate the impact of outliers. The mean of the three-day abnormal announcement return is 0.55 percent, which represents the average response to positive, negative, and no-news surprises. The range of these abnormal returns is from −17.89 percent to +21.80 percent. In untabulated analysis, when we estimate the *CAR*s separately for the positive and negative earnings news, the respective *CAR*s are 1.26 percent (t-statistic = 51.75) and −0.89 percent (t-statistic = −30.08), respectively, indicating that stock prices move in the same direction as the sign of the seasonally differenced earnings surprises. The post-announcement return, $CAR_{(+2\ \text{to}\ +60)}$, has a similar mean but a significantly greater standard deviation, which is expected as the post-announcement returns are measured over a significantly longer time period of around 60 trading days. The positive mean of 0.004 for earnings surprise, *UE*, indicates that the earnings news has, on average, been positive. However, when we divide the sample into positive and negative earnings surprises, we note that the mean magnitude of the two sub-groups, $UE[>0]$ and $UE[<0]$, is about the same at 0.012, suggesting that the overall positive mean of *UE* is due to the greater preponderance of positive news in our sample. This is further confirmed in the last two rows of Table 1 where we report that about two-thirds of the earnings announcements in our sample represent positive news, whereas about one-third represent negative news, consistent with our excluding loss firms from our sample. The Baker-Wurgler Sentiment Index ranges from −2.39 to 2.54 during our sample, with a mean close to zero.

We also divide our total sample into three sub-groups based on the estimate of the sentiment at the beginning of the announcement month, and report summary statistics separately for each sub-group in Columns (6) through (11). High (low) sentiment periods are those when sentiment is equal to or greater (less) than 0.5 (−0.5). The medium sentiment periods include the remaining months when sentiment is between −0.5 and 0.5. Differences in the means across high and low sentiment periods are reported in Column (12) and the associated t-statistics are reported in the last column. These are based on the standard errors that are clustered by month.

The average abnormal announcement and post-announcement returns, $CAR_{(-1\ to\ +1)}$ and $CAR_{(+2\ \text{to}\ +60)}$, respectively, are both higher during high sentiment periods. This provides initial support to our hypothesis that the stock price response to earnings news is swayed in the direction of the prevailing sentiment. Furthermore, no monotonic variation in earnings surprise, *UE*, across the high, medium, and low sentiment periods is apparent. However, the mean *UE* seems to be greater during low sentiment than during high sentiment periods. A break-up of the *UE* into positive $UE[>0]$ and negative $UE[<0]$ indicates that the magnitudes of both the positive and negative earnings surprise are larger during low sentiment periods than during high sentiment periods.

TABLE 1

Summary Statistics

	Overall Sample				High Sentiment Periods		Med. Sentiment Periods		Low Sentiment Periods		Difference in Means across High and Low Sentiment Periods	
Variable (1)	Mean (2)	S.D. (3)	Min (4)	Max (5)	Mean (6)	S.D. (7)	Mean (8)	S.D. (9)	Mean (10)	S.D. (11)	Difference (12) = (6) − (10)	t-stat (13)
$RET_{-1,+1}$	0.55%	6.10%	−17.89%	21.80%	0.60%	6.25%	0.56%	6.19%	0.46%	5.52%	0.14%**	2.13
$RET_{+2,+60}$	0.65%	17.22%	−49.66%	54.61%	1.68%	17.84%	0.41%	17.47%	0.19%	15.18%	1.49%***	6.65
UE	0.004	0.021	−0.207	0.171	0.004	0.023	0.004	0.020	0.005	0.025	−0.0015***	−3.43
UE [> 0]	0.012	0.019	0.000	0.171	0.013	0.020	0.011	0.019	0.014	0.021	−0.00198***	−3.03
UE [< 0]	−0.012	0.017	−0.207	0.000	−0.013	0.018	−0.010	0.016	−0.015	0.020	0.00176**	2.25
UEUp	0.008	0.017	0.000	0.171	0.008	0.018	0.007	0.016	0.010	0.019	−0.0017***	−3.95
UEDown	−0.004	0.011	−0.207	0.000	−0.004	0.012	−0.003	0.010	−0.005	0.013	0.0002	0.47
Sent	0.0480	0.831	−2.391	2.541	1.209	0.484	−0.025	0.249	−1.242	0.515	2.451***	27.83
NonlUp	0.0003	0.0018	0.0000	0.0294	0.0004	0.0019	0.0003	0.0017	0.0004	0.0020	−0.0001**	−2.49
NonlDown	−0.0001	0.0010	−0.0428	0.0000	−0.0002	0.0011	−0.0001	0.0009	−0.0002	0.0012	0.0000	1.28
MktPE	0.342	4.184	−16.851	20.120	−0.695	6.960	0.737	2.939	0.198	2.864	−0.893	−0.86
# of total (*UE*) observations	392,212				84,756		245,018		62,438			
Percent of > 0 *UE*	67%				65%		67%		67%			
Percent of < 0 *UE*	31%				33%		30%		30%			

*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively.

This table reports the summary statistics for the various variables in our sample. The overall sample consists of 392,212 quarterly earnings announcements of firms over the period 1972–2007. Columns (6) through (11) report summary statistics for the three sub-periods of high, medium, and low sentiment. The sub-periods are defined based on the value of the Baker-Wurgler Sentiment Index at the beginning of the announcement month. The high (low) sentiment periods are those when sentiment is equal to or greater (less) than 0.5 (−0.5). The medium sentiment periods include those when sentiment is between 0.5 and −0.5. Column (12) reports the difference in the means across high and low sentiment periods. The t-statistics reported in Column (13) are based on standard errors clustered by month.

Variable Definitions:

$RET_{-1,+1}$ = three-day cumulative abnormal return centered on the date of the earnings announcement;

$RET_{+2,+60}$ = cumulative abnormal post-earnings announcement return from day +2 to +60;

UE = seasonally differenced earnings surprise scaled by stock price;

UEUp (*UEDown*) = product of the unexpected earnings, *UE*, and the indicator variable, *Up* (*Down*), which takes the value of 1 if *UE* is positive (negative), and 0 otherwise;

Sent = monthly Baker-Wurgler Sentiment Index prevailing at the beginning of the month;

NonlUp and *NonlDown* = square of *UEUp* and the square of *UEDown*, respectively, multiplied by −1; and

MktPE = difference between the aggregate stock market PE of the lagged month and the average market PE of the prior 12 months.

Because the incidence of the positive news dominates the incidence of the negative news, the net effect is to increase the average *UE* during low sentiment periods. Nevertheless, as we report at the bottom of Table 1, the *incidence* of positive and negative earnings surprises is similar across high, medium, and low sentiment periods, with about two-thirds of the total announcements being positive and much of the remaining one-third being negative across the three sub-periods.

III. MAIN RESULTS

Sentiment and Stock Price Sensitivity to Earnings News

We begin our investigation of the role of sentiment in stock price sensitivity to earnings news by reporting the estimates of Equation (2) in Table 2. The intercept, which captures the stock price response to the incidence of earnings that meet or beat the earnings of the firm in the same quarter of the last year, is positive and statistically significant. The coefficient for the variable *Down*, on the other hand, is negative and statistically significant. This coefficient, when added to the intercept, yields a negative value and captures the stock price response to the incidence of earnings that fall short of the earnings four quarters earlier. Both of these results are expected and consistent with the prior literature. The coefficient for the variable *UEUp*, which captures the ERC of positive earnings news when sentiment is zero, is 1.06, and is statistically significant with the t-statistic of 46.36. This is consistent with a large literature in accounting that documents that earnings surprises elicit significant response from stock prices. The coefficient of *UEDown*, which depicts the ERC of the negative earnings news, is 0.266 and is statistically significant with a t-statistic of 11.38. The positive coefficient indicates that the stock price declines more when the absolute value of the negative earnings news is greater, again consistent with the notion that stock price moves in the direction of the earnings surprises. Further, the evidence of a larger coefficient for *UEUp* than for *UEDown* is also consistent with the extant literature, which finds that positive earnings surprises are considered to be more informative than the corresponding negative surprises, possibly due to the tendency of some managers to take an earnings bath (Conrad et al. 2002).

Our primary focus in Table 2 is on the interaction variables that capture the effect of sentiment on ERC for the positive and negative earnings news. The coefficient of the interaction variable, *UEUp* × *Sent*, is a positive 0.037 with a t-statistics of 3.35, which is consistent with our hypothesis, and indicates that the ERC for the announcement of positive earnings news increases with investor sentiment. The reported coefficient estimate for *UEDown* × *Sent* in Table 2 is –0.069 with a t-statistic of –6.26, which is consistent with stock price sensitivity to negative earnings news decreasing with the sentiment. Taken together, the evidence in Table 2 indicates that the response of stock price to both positive and negative earnings news is more positive when sentiment is more bullish.[10]

To assess the economic significance of these coefficients, we first note that the sentiment variable, *Sent*, is a standardized variable with mean 0 and standard deviation of 1. Thus, in Equation (2), the coefficient for *UEUp* (*UEDown*) when *Sent* is 0 (i.e., *Sent* is at its mean) is given by β_0 (γ_0), after accounting for the effect of the other control variables. When *Sent* is equal to 1, that is, it is one

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[10] We also re-estimate Equation (2) after inserting a stand-alone term, *Sent*, in the model. In untabulated results, we find that the stand-alone *Sent* variable has a coefficient of 0.00067 (t-statistic = 2.38), and the interaction variables, *UEUp* × *Sent* and *UEDown* × *Sent*, have coefficients of 0.023 (t-statistic = 2.29) and –0.053 (t-statistic = –4.61), respectively. We further re-estimate our main model in Equation (2) after replacing the dependent variable of three-day event returns with a longer window return, which captures the abnormal returns beginning two days after the previous earnings announcement to one day after the current announcement. In untabulated results, we find that the results of this sensitivity analysis are similar to those reported in Table 2. The coefficient for *UEUp* × *Sent* is 0.073 with a t-statistic of 1.72, whereas the coefficient for *UEDown* × *Sent* is –0.112 with a t-statistic of 2.56. Thus, the basic patterns in Table 2 seem to hold for the longer return window.

TABLE 2

Investor Sentiment and the Stock Returns Surrounding the Announcements of Earnings News

Variables	Coefficient Estimates (t-statistics)
Intercept	0.003*** (12.29)
Down	−0.010*** (−26.06)
UEUp	1.061*** (46.36)
UEUp × *Sent*	0.037*** (3.35)
UEDown	0.266*** (11.38)
UEDown × *Sent*	−0.069*** (−6.26)
NonlUp	−6.200*** (−31.16)
UEUp × *MktPE*	0.005** (2.15)
NonlDown	−1.924*** (−8.47)
UEDown × *MktPE*	−0.004 (−1.26)
Adjusted R^2	4.54%

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*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively.
This table provides estimates of the following model:

$$RET_{it} = \alpha_0 + \alpha_1 Down_{it} + \beta_0 UEUp_{it} + \beta_1 UEUp_{it} \times Sent_{t-1} + \gamma_0 UEDown_{it} + \gamma_1 UEDown_{it} \times Sent_{t-1} + \beta_2 NonlUp_{it} + \beta_3 UEUp_{it} \times MktPE_{t-1} + \gamma_2 NonlDown_{it} + \gamma_3 UEDown_{it} \times MktPE_{t-1} + \varepsilon_{it}.$$

The model is estimated using OLS and the pooled data of 392,212 quarterly earnings announcements over the period of 1972–2007. The t-statistics are reported in parentheses and are based on heteroscedasticity consistent standard errors clustered by month.

Variable Definitions:
RET_{it} = cumulative abnormal return calculated per the methodology detailed in Daniel et al. (1997) from day −1 to +1 relative to the earnings announcement date;
UEUp (*UEDown*) = product of the unexpected earnings, *UE*, and the indicator variable, *Up* (*Down*), which takes the value of 1 if *UE* is positive (negative), and 0 otherwise;
Sent = monthly Baker-Wurgler Sentiment Index prevailing at the beginning of the month;
NonlUp and *NonlDown* = square of *UEUp* and the square of *UEDown*, respectively, multiplied by −1; and
MktPE = difference between the aggregate stock market PE of the lagged month and the average market PE of the prior 12 months.

standard deviation above its mean, the coefficient of *UEUp* (*UEDown*) becomes $\beta_0 + \beta_1$ $(\gamma_0 + \gamma_1)$. Similarly, when *Sent* is equal to −1, that is, it is one standard deviation below its mean, the coefficient of *UEUp* (*UEDown*) equals $\beta_0 - \beta_1$ $(\gamma_0 - \gamma_1)$. The estimated coefficients we report in Table 2 indicate that β_0 equals 1.061 whereas β_1 is 0.037, implying that the coefficient of *UEUp* is 1.098 [= 1.061 + 0.037] when *Sent* is high (i.e., one standard deviation above mean) and 1.024 [=

1.061 – 0.037] when *Sent* is low (i.e., one standard deviation below mean). These numbers suggest that the stock price sensitivity to positive earnings news decreases by about 7 percent [= 1.024/1.098 – 1] when we move from periods of high sentiment to periods of low sentiment. On the other hand, the reported estimates of γ_0 and γ_1 are 0.266 and –0.069, respectively, implying that the coefficient of *UEDown* is 0.197 [= 0.266 – 0.069] when *Sent* is high and 0.335 [= 0.266 + 0.069] when *Sent* is low. These numbers suggest that the stock price sensitivity to negative earnings news increases by 70 percent [= 0.335/0.197 – 1] when we move from periods of high sentiment to periods of low sentiment. Thus, the effect of sentiment on the mispricing of negative earnings appears to be more pronounced than that for the positive news.

This asymmetry in the effect of sentiment is consistent with the notion that the incremental cash flow embedded in bad earnings news is more uncertain and difficult to value than the incremental cash flow that is embedded in good earnings news. Thus, investors face greater subjectivity in the valuation of bad news, making the stock price reaction to bad news more susceptible to the prevailing sentiment. To provide some empirical support for this conjecture, we first compute the persistence of good and bad earnings news for each firm using the following autoregressive model for seasonally differenced earnings:

$$\begin{aligned} EPS_{iq} - EPS_{iq-4} = {} & \alpha_0 + \alpha_1(EPS_{qt-1} - EPS_{iq-5}) + \alpha_2 DNeg_{q-1} \\ & + \alpha_3(EPS_{iq-1} - EPS_{iq-5}) \times DNeg_{q-1} + \varepsilon_{iq}. \end{aligned} \tag{4}$$

The variable *DNeg* in the model is a dummy variable that equals 1 if the prior quarter's earnings change is negative, and 0 otherwise. The persistence of good news is captured in the estimated coefficient α_1 in the model, whereas the persistence of bad news is represented by the sum of the two coefficients $\alpha_1 + \alpha_3$. We then compare the cross-sectional *variability* of the estimated firm-level persistence measures of good and bad news. Consistent with our conjecture, we find that standard deviation of the persistence of the bad news (1.258) is greater than the standard deviation of the persistence of good news (0.898). This suggests that the identification and the valuation of the incremental cash flows associated with a bad news signal might be a more subjective task than the corresponding valuation of a good news signal. This could allow sentiment to play a more pronounced role in the pricing of bad than good earnings news.

Turning to the control variables, we include the interaction term between relative market *PE* and earnings surprise following Conrad et al. (2002). They find that the stock price sensitivity to good news is largely unaffected by relative market *PE*, but price sensitivity to bad news increases during periods of high relative market *PE*. Our results, in contrast, show that the coefficient of the interaction between relative market *PE* and positive unexpected earnings is positive with a significant t-statistic of 2.15. Moreover, the coefficient on the interaction between relative market *PE* and negative unexpected earnings (*UEDown*) is negative and insignificant. This indicates that relative market *PE* has no impact on the stock price reaction to bad news. Thus, our results suggest that the significance of relative market *PE* for stock price sensitivity documented by the prior literature largely disappears once we incorporate the effect of sentiment.

In sum, our results are consistent with the view that the behavioral biases of investors, summarized in the form of aggregate investor sentiment, affect how stock prices respond to firm-specific news, and provide support for our hypothesis that the stock price response to earnings news contains both a rational as well as a behavioral component.

Sentiment and Informativeness of Current Earnings for Future Earnings

To examine the alternative explanation that the higher ERC for the good (bad) news during high (low) sentiment periods, which we document in Table 2, simply reflects the greater information content of the news during such periods, we follow the methodology outlined in

Section II. We estimate Equation (3), in which future realized earnings or cash flows are regressed on current earnings surprises, and report the results in Table 3. We are specifically interested in the coefficients for $UEUp \times Sent$ and $UEDown \times Sent$, that is, whether β_1 is positive and γ_1 is negative, as this would suggest that the differential information content of earnings news over time, rather than sentiment, could be driving our results in Table 2.

When the dependent variable is the future realized cash flows, the reported estimates of β_1 and γ_1 in Table 3 are statistically indistinguishable from zero, indicating that the informativeness of earnings news for future cash flows does not vary with sentiment. When the dependent variable is future realized earnings, both the interaction variables become statistically significant but with the "wrong" signs. The estimate of β_1 is negative (–0.198) and significant (t-statistic = –2.13), indicating that the impact of current quarter's positive earnings news on earnings one quarter ahead

TABLE 3

Investor Sentiment and the Impact of Current Period Unexpected Earnings on Future Earnings and Cash Flows

Explanatory Variables	**Dep Variable = E_{iq+1}**	**Dep Variable = CFO_{iq+1}**
Intercept	0.082***	0.124***
	(46.72)	(23.40)
E_{q-3}	0.794***	0.525***
	(127.58)	(28.91)
CFO_{q-3}		0.504***
		(63.46)
$UEUp$	2.203***	2.117***
	(25.32)	(9.49)
$UEUp \times Sent$	−0.198**	−0.399
	(−2.13)	(−1.04)
$UEDown$	4.849***	1.306***
	(31.42)	(3.61)
$UEDown \times Sent$	0.605***	−0.629
	(4.41)	(−1.46)
Adjusted R^2	56.90%	33.02%
Sample period	1972–2007	1987–2007
Number of pooled stock-quarter observations	368,756	198,529

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*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively.

This table provides estimates of the following model:

$$Perf_{iq+1} = \alpha_0 + \alpha_1 E_{iq-3} + \beta_0 UEUp_{iq} + \beta_1 UEUp_{iq} \times Sent + \gamma_0 UEDown_{iq} + \gamma_1 UEDown_{iq} \times Sent + \varepsilon_{iq+1}.$$

The dependent variable, $Perf_{iq+1}$, alternately equals E_{iq+1} or CFO_{iq+1}.

The model is estimated using pooled stock-quarter data. The t-statistics are reported in parentheses and are based on heteroscedasticity consistent standard errors clustered by month.

Variable Definitions:

E_{iq+1} = one-quarter-ahead actual earnings reported in Compustat adjusted according to Livnat and Mendenhall (2006);

CFO_{iq+1} = one-quarter-ahead Cash Flow from Operations reported in Compustat;

E_{iq-3} and CFO_{iq-3} = actual earnings and cash flow from operations, respectively, for three quarters back;

$UEUp$ ($UEDown$) = product of the unexpected earnings, UE, and the indicator variable, Up ($Down$), which takes the value of 1 if UE is positive (negative), and 0 otherwise; and

$Sent$ = monthly Baker-Wurgler Sentiment Index prevailing at the beginning of the month of the announcement of quarter q earnings.

decreases with sentiment. For earnings informativeness to account for our main results, this coefficient should be positive and significant rather than negative and significant. Further, the coefficient on γ_1 is positive (0.605) and significant (t-statistic = 4.41), indicating that the impact of the current quarter's negative earnings news on earnings one quarter ahead *increases* with sentiment. To account for our main results in Table 2, this coefficient should be negative and significant.[11] Overall, the results in Table 3 suggest that the time variation in the information content of earnings cannot explain our results in Table 2 because the information content of earnings is either unrelated to sentiment (when measured using future cash flows), or is related in the "wrong" way (when measured using future earnings).[12]

IV. ADDITIONAL ANALYSES

Do the Pricing Effects of Sentiment Reverse Quickly or Contribute to Drift?

Prior studies on investor sentiment (Baker and Wurgler 2006; Brown and Cliff 2005) suggest that its effects on asset prices persist for some time and reverse only over longer horizons of one to three years. To test whether the impact of sentiment on the pricing of earnings news is temporary or continues into the near future, we examine how sentiment influences the commonly documented post-earnings announcement drift (PEAD) in stock prices. We form deciles of earning surprises and examine how the drift associated with each decile varies across periods of extremely high (top 20 percent) and extremely low (bottom 20 percent) investor sentiment. The drift is based on the abnormal stock returns over the period starting from day +2 relative to the current earnings announcement to a day after the announcement of the next quarter's earnings. Table 4 shows that the post-announcement stock returns for every decile of earnings surprises are lower during the low sentiment periods than during the high sentiment periods. Moreover, while the negative abnormal returns associated with the bottom four deciles of the earnings surprises are significantly negative during the low sentiment periods, the corresponding returns are statistically indistinguishable from zero during the high sentiment periods.

In an alternative simple test of the medium-term continuation of the effect of sentiment, we re-estimate Equation (2) after replacing the dependent variable with the cumulative abnormal returns over the period +2 to +60 relative to the earnings announcement date. We find that the results mirror those documented in Table 2. That is, the coefficient of *UEUp* × *Sent* is positive and significant, and that of *UEDown* × *Sent* is negative and significant. Overall, therefore, it appears that, consistent with the view expressed by Baker and Wurgler (2006) and Brown and Cliff (2005),

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[11] In additional analysis, we replace the dependent variable with the earnings two, three, and four quarters ahead and find that the results are very similar, in both economic and statistical significance, to those reported in Table 3.

[12] The results in Table 3 differ when we use earnings versus cash flows as the dependent variable. One possibility is that the accruals component of earnings varies systematically over time with sentiment, and the subsequent reversal of this component causes the informativeness of current earnings for future earnings to vary significantly with sentiment. To consider this possibility, we first note that the finance literature links the overpricing of stocks during periods of high sentiment to greater equity issuance and corporate investment, as managers attempt to exploit the mispricing of their firms (Stein 1996; Baker et al. 2003). Corporate investment, in turn, has strong positive association with accruals (Fairfield et al. 2003; Polk and Sapienza 2009). Thus, periods of high sentiment are also likely to be periods of high accruals. The poor future performance commonly associated with high accruals could, therefore, lead to more mean-reversion in the good earnings news that arrives during high sentiment periods than the good news that arrives during low sentiment periods. Similarly, the accruals-related reversals would make the bad earnings news that arrives during high sentiment periods look more persistent than the bad earnings news that arrives during low sentiment periods. This would show up as the negative (positive) association between sentiment and the informativeness of positive (negative) current earnings news for future earnings in Table 3.

TABLE 4

Investor Sentiment and Post Earnings Announcement Drift

	High Sentiment Periods		Low Sentiment Periods	
***UE* Deciles**	**Returns**	**t-statistics**	**Returns**	**t-statistics**
1	−0.39	−1.24	−1.82***	−6.21
2	0.01	0.04	−0.99***	−3.35
3	−0.19	−0.57	−0.81**	−2.24
4	0.21	0.73	−1.06***	−4.21
5	1.21***	4.02	−0.20	−0.69
6	1.62***	5.04	0.24	0.94
7	2.31***	6.67	1.07***	4.16
8	3.18***	9.69	1.39***	5.76
9	3.31***	9.73	2.17***	7.33
10	4.15***	7.56	3.52***	7.88
Number of months	86		83	

*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively.

This table documents the drift in stock prices in the period +2 to +60 relative to the earnings announcement date. Returns are abnormal returns calculated following the methodology of Daniel et al. (1997). *UE* is the earnings surprise as defined in Equation (1). We first identify months of extremely high (top 20 percent) and extremely low (bottom 20 percent) sentiment, and then examine the drift associated with the earnings announced during the high and low sentiment months. The sorting of earnings surprise (*UE*) into deciles is relative to the breakpoints identified from the previous calendar quarter.

the impact of sentiment on stock price sensitivity to earnings news is not a temporary phenomenon because it extends to several months following the announcement of earnings.

Contribution of Earnings Mispricing to the Sentiment-Return Relationship

So far, our results show that the stock price reaction during the three days surrounding the announcement of both good and bad earnings news is more positive when sentiment is high than when sentiment is low. Further, the drift in stock prices mirrors the results documented for the three-day event window. The upward (downward) stock price drift following the announcement of good (bad) earnings news is greater during periods of high (low) sentiment than during periods of low (high) sentiment. If sentiment-related mispricing of *earnings* reverses over longer horizons and contributes significantly toward the previously documented negative relation between sentiment and future returns,[13] then we should find that the relation is stronger for stocks that experience large earnings surprise than for stocks that experience no or little earnings surprise.

To investigate this possibility, we first sort firms in each calendar year *y* into quartiles based on the average of the absolute values of all the quarterly earnings surprises during the year.[14] We retain only those firms in this analysis for which the earnings news is available for at least three quarters during the year.[15] We label the firms in quartiles 4 and 1, respectively, as the high-news and low-news firms. We then investigate whether the high-news firms have greater sensitivity to sentiment than the low-news firms by running the following regression, where m and y signify month and year:

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[13] Prior research has shown that sentiment-related mispricing of stocks reverses in the future (Baker and Wurgler 2006; Brown and Cliff 2005). During high (low) sentiment years, prices are excessively bid up (down), and so the reversal entails a drop (rise) in future stock prices. Future stock returns are, thus, negatively associated with sentiment.

[14] If the news is good and the sentiment is high in year *y*, then the firm becomes overvalued and stock price falls in the subsequent year. Similarly, if the news is bad and the sentiment is high, then the stock price does not fall as much in the current year and thus also becomes overvalued. Hence, regardless of whether it is good or bad news that arrives during high sentiment periods, the firm becomes overvalued. Hence, we use the absolute value of the news rather than the signed value of the news.

$$RET_{HN,m,y} - RET_{LN,m,y} = \alpha_0 + \alpha_1 Sent_{y-1} + \beta_1 RMRF_{m,y} + \beta_2 SMB_{m,y} + \beta_3 HML_{m,y} + \beta_4 UMD_{m,y} + \varepsilon_{m,y}. \tag{5}$$

The dependent variable is the difference between the equal weighted return of the high-news stocks (labeled with subscript *HN*) and the equal weighted return of the low-news stocks (labeled with subscript *LN*). Similar to Baker and Wurgler (2006), we regress the monthly returns in year y on sentiment in year y–1 in this equation. The coefficient of the lagged sentiment, α_1, captures the *differential* sensitivity of the high-news stocks to sentiment relative to that of the low-news stocks. If the association between sentiment and future returns is stronger (i.e., more negative) for the high-news stocks, as we hypothesize, we should find that the coefficient α_1 is negative and statistically significant. The other four variables on the right-hand side of the model represent the three Fama and French (1993) risk factors and the momentum factor, and are meant to control for the differences in the risk exposures of the high-news and low-news stocks. We obtain the monthly observations of these factors from Wharton Research Data Services (WRDS).

The descriptive statistics in Panel A of Table 5 show that the mean (median) value of the average absolute earnings surprise for the low-news firms is 0.002 (0.002). The corresponding mean (median) for the high-news firms is ten times higher at 0.025 (0.020). This gives us some assurance that there is a reasonable variation in the magnitude of the earnings surprises across the low-news and high-news firms. The last two columns show that the mean (median) earnings surprise of those high-news firms for which all the quarters in the year had positive news is 0.028 (0.022). We label these firms as high-positive-news (*HPN*) firms. This sample makes up around 40 percent of the overall high-news sample. For the remaining high-news firms, which have at least one negative earnings surprise during the year, the mean (median) earnings surprise is 0.024 (0.020). We label these as high-negative-news (*HNN*) firms.

Column (2) of Panel B in Table 5 shows that sentiment (*Sent*) is indeed negatively related to the difference in returns between the high- and low-news stocks (coefficient = –0.178, t-statistic = –1.70). This suggests that the mispricing of earnings surprises due to investor sentiment during the short run is reversed in the longer period, and contributes to the relation between sentiment and future returns. The greater the earnings surprise, the greater the sentiment-related mispricing during the short run and, hence, greater the reversal in the long run. The differential effect of sentiment on future returns for the high-news firms is economically significant. Because sentiment is a standardized variable with mean 0 and standard deviation of 1, the coefficient of –0.178 translates into a differential reversal of 0.356 percent (0.178×2) per month for the high-news firms relative to the low-news firms for a change in sentiment from one standard deviation below the mean to one standard deviation above the mean. In terms of statistical significance, we note that the coefficient of sentiment is significant only at the 10 percent level. This is perhaps not surprising given that long-term return analysis presents thorny statistical issues (Lyon et al. 1999) and because the sign and the magnitude of the earnings surprises during the year might be a noisy proxy for the sign and the magnitude, respectively, of all the firm-specific news that arrives during the year.

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[15] We lose about 40 percent of the sample observations using this filter. Our results, however, are qualitatively similar when we use all firm-years instead of only those with at least three quarters of data.

TABLE 5

The Relation between Sentiment and Future Returns across Firms Sorted on Earnings News

Panel A: Descriptive Statistics of the Sub-Samples Sorted on Earnings Surprises

Statistic **(1)**	**Low-News Firms** **(*LN*)** **(2)**	**High-News Firms** **(*HN*)** **(3)**	**High-News Firms with All Positive Earnings** **(*HPN*)** **(4)**	**High-News Firms with at Least One Negative Earnings** **(*HNN*)** **(5)**
Number of firms per year				
Mean	621	622	245	376
Minimum	301	302	67	137
Maximum	911	912	458	585
Total number of firm-years	22,384	22,394	8,835	13,559
Absolute average earnings surprise per firm-year				
Mean	0.002	0.025	0.028	0.024
Median	0.002	0.020	0.022	0.020
Std. Dev.	0.001	0.016	0.018	0.014

Panel B: Regression Estimates

	Dependent Variable			
Explanatory Variables **(1)**	$Ret_{HN} - Ret_{LN}$ **(2)**	$Ret_{HPN} - Ret_{LN}$ **(3)**	$Ret_{HNN} - Ret_{LN}$ **(4)**	$Ret_{HNN} - Ret_{HPN}$ **(5)**
Intercept	0.180*	0.259**	0.125	−0.132
	(1.65)	(2.45)	(1.01)	(−1.54)
Sent	−0.178*	−0.067	−0.245**	−0.181*
	(−1.70)	(−0.61)	(−2.08)	(−1.81)
RMRF	0.091***	0.103***	0.089***	−0.017
	(3.51)	(3.25)	(3.30)	(−0.59)
SMB	0.576***	0.560***	0.596***	0.045
	(13.28)	(11.40)	(12.03)	(1.09)
HML	0.310***	0.171***	0.406***	0.232***
	(5.88)	(3.00)	(6.80)	(4.47)
UMD	0.018	0.083**	−0.027	−0.104***
	(0.42)	(2.11)	(−0.55)	(−3.64)
Adjusted R^2	52.05%	48.34%	49.91%	22.26%

*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively.

This table examines how the short-term mispricing of earnings news contributes to the broader relation between sentiment and future returns. The sample period is from 1973–2007. For each calendar year, we first sort firms based on the average of the absolute value of all the quarterly earnings surprises during the year, after restricting the sample to those firms for which we have the data of at least three quarterly earnings surprises. The firms in the top and bottom quartiles are labeled as high-news (*HN*) and low-news (*LN*) firms, respectively, and are retained for the analysis.

Panel A reports the summary statistics for these two sets of firms. We also divide the high-news sample further into two sub-groups: the firms for which all quarterly surprises were positive are grouped together as high-positive-news firms (*HPN*), whereas the remaining firms with at least one negative earnings surprise are grouped as high-negative-news firms (*HNN*). The summary statistics of these two subsamples of the high-news firms are also reported in Panel A in the last two columns.

In Panel B, we then examine the how the relation between sentiment and future returns varies across various sub-groups of stocks by estimating variations of the following model:

(continued on next page)

TABLE 5 (continued)

$$RET_{HN,m,y} - RET_{LN,m,y} = \alpha_0 + \alpha_1 Sent_{y-1} + \beta_1 RMRF_{m,y} + \beta_2 SMB_{m,y} + \beta_3 HML_{m,y} + \beta_4 UMD_{m,y} + \varepsilon_{m,y}.$$

The subscripts m and y in this equation stand for calendar month and year, respectively. The dependent variable in the first estimation, reported in Column 2 of Panel B, is the difference in the returns of the equal weighted portfolios of the high-news (*HN*) stocks and the low-news (*LN*) stocks. *Sent* is the annual Baker-Wurgler Sentiment Index. In this model, we regress the monthly returns in year y on the sentiment for calendar year y–1. The coefficient of the lagged sentiment α_1 in the equation captures the *differential* sensitivity of the high-news stocks to sentiment relative to that of the low-news stocks. *RMRF*, *SMB*, *HML*, and *UMD* signify the market, size, value, and momentum factors, respectively. In the last three columns of Panel B, we report the estimates of this model after substituting the dependent variable with the difference between the returns of the high-positive-news and low-news firms (Column 3), the difference between the returns of the high-negative-news and low-news firms (Column 4), and the difference between the returns of the high-negative-news and high-positive-news firms (Column 5). The t-statistics are reported in parentheses and are based on heteroscedasticity consistent standard errors.

Our previous analysis in Table 2 shows that the impact of sentiment on the price sensitivity to earnings surprises (or earnings-mispricing due to sentiment) is higher for bad news than for good news. Therefore, it is likely that the subsequent reversals and the corresponding negative relation between sentiment and future returns is also stronger for stocks that experience negative earnings news than for the stocks that experience positive earnings news. To investigate this possibility, we split the high-news firms into positive high-news and negative high-news firms. The firms whose quarterly earnings surprises during year y–1 are all of positive sign are grouped together as the high-positive-news (*HPN*) firms. The rest of the firms, whose quarterly earnings surprises include at least one negative surprise, form the other group labeled as the high-negative-news (*HNN*) firms. This criterion yields us a somewhat balanced split of the high-news sample and reflects the relative scarcity of the negative earnings surprises.[16]

We then examine the differential relation between sentiment and future returns for the high-negative-news stocks relative to the high-positive-news stocks by modifying the dependent variable in Equation (5) and report the results in the last three columns of Panel B in Table 5. Column (3) reports the estimates when the dependent variable is the difference in the returns of the high-positive-news firms (*HPN*) and low-news firms. Here the coefficient of *Sent* becomes insignificant (coefficient = –0.067, t-statistic = –0.61). On the other hand, in Column (4), we report the estimates of the model when the dependent variable is the difference in the returns of the high-negative-news firms (*HNN*) and the low-news firms. Here the coefficient of *Sent* becomes more pronounced and statistically significant (coefficient = –0.245, t-statistic = –2.08). This suggests that bad earnings news is the primary source of the greater sentiment-future return relation for the high-news stocks compared to the low-news stocks reported in Column 2. Finally, in Column (5), we more directly examine the differential sentiment-driven mispricing of the bad and good earnings news. Here the dependent variable is the difference between the returns of the high-positive-news firms (*HPN*) and high-negative-news firms (*HNN*). The coefficient on sentiment is now negative and significant (coefficient = –0.181, t-statistic = –1.81). This shows that the reversal for the bad news sample is significantly higher than the reversal for the all good news sample. Overall, the results in Table 5 suggest that sentiment-related mispricing of earnings news contributes significantly to the negative relation between sentiment and future returns, and this is especially true for negative earnings news.[17]

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[16] Including only those firms whose quarterly earnings surprises are *all* negative leaves us with very few observations (13 percent of the high-news sample).

Cross-Sectional Variation in the Role of Sentiment

Extant literature argues that broad waves of sentiment will have greater effects on stocks that are harder to value and more difficult to arbitrage (Lemmon and Portniaguina 2006; Qiu and Welch 2006; Baker and Wurgler 2006). Specifically, Baker and Wurgler (2006) suggest that small, young, high volatility, non-dividend-paying, distressed (i.e., low market-to-book), and extreme growth (i.e., high market-to-book) stocks are more likely to be affected by sentiment,[18] while large, mature, stable, high-dividend-paying, and medium-growth companies are likely to be affected less or even negatively. Investors might see these latter firms as safe heavens during times of low sentiment (Baker and Wurgler 2007).

We hypothesize that the effect of sentiment on the ERC would also be more pronounced for small, young, high volatility, non-dividend-paying, distressed (i.e., low market-to-book), and extreme growth (i.e., high market-to-book) stocks. We argue that sentiment influences the ERC because investors value the incremental cash flows embedded in earnings announcements optimistically (pessimistically) during periods of high (low) sentiment. The arguments of Baker and Wurgler (2006) imply that the effect of sentiment on the (mis)valuation of the incremental cash flows is likely to be greater if the cash flows are more uncertain and difficult to assess, which is likely to be the case for earnings of speculative stocks in general.

To test this hypothesis, we classify stocks at the end of each month into groups that are more or less exposed to sentiment based on five individual firm characteristics following the methodology in Baker and Wurgler (2006). The first three characteristics—size, age, and stock return volatility—are computed every month for each stock using the CRSP monthly tape. *Size* is the market capitalization of the stock. *Age* is the number of years since the firm first appeared on CRSP, measured to the nearest month. *Stock return volatility* is the standard deviation of monthly returns over the preceding 12 months. The last two characteristics—dividend payout and market-to-book ratio—are computed once a year at the end of June, and are then assigned to each of 12 subsequent months. *Dividend payout* is the annual dividend divided by the annual earnings in the most recent fiscal year, and is obtained from Compustat. Market-to-book ratio (*MEBE*) is computed by using the book value from Compustat for the most recent fiscal year that ended during calendar year $y-1$. This book value is then matched with the market value of the stock as of the end of June in year y to compute *MEBE* ratio as of June in year y.

We then use each individual firm characteristic to identify one portfolio that is likely to be affected more by sentiment, and a second portfolio that is likely to be affected less, or even negatively, by sentiment. We classify firms that fall in the bottom quintile based on size and age of the overall CRSP sample using NYSE breakpoints as small and young firms, respectively. We classify their counterparts in the top quintile as large and mature firms, respectively. These are the firms that are relatively easier to value and arbitrage. Similarly, for stock return volatility, firms that fall in the top (bottom) quintile are classified as volatile (stable) firms, and are expected to be affected more (less) by sentiment. For classification on dividend payout, all the firms that do not pay dividends are grouped together as non-payers. These are the firms that are more exposed to sentiment. The firms that fall into the top quintile of the remaining dividend-paying firms in the CRSP-Compustat merged dataset using NYSE breakpoints are then classified as high payers. These are the firms that are expected to be less exposed to investor sentiment.

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[17] In additional analysis, we re-estimate Equation (5) for the subsamples of small, young, volatile, and extreme book-to-market stocks (below their median break points). Since the number of stocks each month in the high- and low-news portfolios drops further, we exclude the top and bottom 1 percent of the months identified as influential using the Belsley et al. (1980) methodology. We find that our results in Table 5 continue to hold. For instance, when we estimate the model for small stocks, the coefficients of *Sent* for the four columns in Panel B are, respectively, –0.178 (t-statistic = –1.97), –0.043 (t-statistic = –0.45), –0.259 (t-statistic = –2.49), and –0.177 (t-statistic = –2.30). Estimates for the model for the non-dividend-paying stocks are not reliable because of the prohibitively small number of stocks (e.g., less than 20 in several months) in the portfolios that make up the dependent variables.

[18] They also suggest that loss firms are difficult to value, but since our study focuses only on profitable firms, we do not analyze loss firms.

Baker and Wurgler (2006) predict and find evidence that the effect of sentiment on book-to-market sorted portfolios is roughly U-shaped. Stocks that are more exposed to sentiment reside in the extreme high and low deciles, whereas staid stocks that are less exposed to sentiment are typically found in the middle. Accordingly, we combine the two extreme deciles sorted on market-to-book ratio into one portfolio, which we then expect to be influenced more by sentiment. The stocks in the middle quintile are classified as medium *MEBE* stocks—these are expected to be less affected by sentiment.

To investigate the cross-sectional differences in the impact of sentiment, we estimate Equation (2) separately for the subsamples of stocks classified on the five individual stock characteristics. In doing so, the classification of firms into portfolios at the end of each month is matched to the earnings announcements of the subsequent month. Our expectation is that the interaction variables involving sentiment, which capture the effect of sentiment on the ERC, are more pronounced for the portfolios of firms that Baker and Wurgler (2006) argue are more exposed to sentiment than the firms in their corresponding opposite portfolios.

In addition to studying the above set of firm characteristics separately, we also construct a composite measure based on the five individual characteristics, and study how stocks in the extreme quintiles sorted on this composite measure are affected by sentiment. To do so, we extract the first principle component of the decile ranks of the five individual characteristics.[19] We then sort the stocks each month based on this composite factor. The stocks that fall in the bottom 20 percent are categorized as *More Exposed*, whereas those stocks that are among the top 20 percent are classified as *Less Exposed*. The effect of sentiment on the ERC should be more (less) pronounced for the former (latter) group of stocks.

The results of our cross-sectional analyses are reported in Table 6. Each panel of Table 6 reports the estimates of Equation (2) for two sub-groups of stocks sorted on one of the firm characteristics. In Panel A, we sort stocks on the composite measure, whereas in each of Panels B through F, we classify stocks based on size, age, return volatility, dividend payout, and *MEBE* ratio, respectively. In each panel, the first group of stocks is the one that is expected to be influenced more by sentiment and the latter group is the one expected to be influenced less.

Focusing first on our composite measure in Panel A, it is evident the effect of sentiment is more pronounced for the stocks that we classify as *Exposed* than for the stocks labeled as *Not Exposed*. The coefficient on the interaction variable, $UEUp \times Sent$, is positive and significant, as predicted by our hypothesis, for the *Exposed* stocks (coefficient = 0.038, t-statistic = 2.67). However, for the *Not Exposed* stocks, the estimated coefficient is negative and significant (coefficient = −0.051, t-statistic = −2.54), indicating that for these firms, sentiment operates in the opposite direction compared to the *Exposed* stocks. This result is consistent with the idea posited by Baker and Wurgler (2007) that

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[19] We first adjust the ranks of all characteristics such that low ranks on each characteristic are associated with the stocks that are more affected by sentiment. Specifically, we reverse the ranks for stock return volatility, such that more volatile stocks have lower ranks. Similarly, for *MEBE* we combine the two extreme deciles and assign them the rank of 1.5, combine deciles 2 and 9 and assign them the rank of 3.5, and so on. The middle two deciles then get the rank of 9.5. Finally, the rank for dividend payout equals 0 for the non-payers; the remaining dividend-paying stocks are ranked from 1 through 10.

TABLE 6

Cross-Sectional Variation in the Impact of Sentiment on the Three-Day Stock Price Response to Earnings News

Panel A: Exposed versus Not Exposed Firms

Variables	*Exposed* Stocks	*Not Exposed* Stocks
Intercept	0.006***	0.001***
	(9.71)	(4.92)
Down	−0.016***	−0.006***
	(−15.39)	(−14.54)
UEUp	1.020***	0.661***
	(28.89)	(15.71)
UEUp × *Sent*	0.038***	−0.051***
	(2.67)	(−2.54)
UEDown	0.121*	0.199***
	(1.86)	(4.98)
UEDown × *Sent*	−0.080***	−0.047**
	(−4.27)	(−2.20)
NonlUp	−5.601***	−5.230***
	(−19.25)	(−10.67)
UEUp × *MktPE*	0.001	0.003
	(0.34)	(0.73)
NonlDown	−0.735	−2.035***
	(1.28)	(−4.89)
UEDown × *MktPE*	−0.003	−0.016**
	(−0.56)	(−2.18)
Number of observations	77,080	77,166
Adjusted R^2	4.86%	2.18%

Panel B: Small versus Large Firms

Variables	Small	Large
UEUp	1.162***	0.444***
	(40.85)	(8.17)
UEUp × *Sent*	0.037***	−0.040
	(2.90)	(−1.54)
UEDown	0.208***	0.187**
	(6.45)	(2.26)
UEDown × *Sent*	−0.077***	−0.063*
	(−6.51)	(−1.87)
Number of observations	170,568	44,418
Adjusted R^2	6.66%	1.07%

Panel C: Young versus Mature Firms

Variables	Young	Mature
UEUp	1.067***	0.531***
	(29.95)	(10.88)
UEUp × *Sent*	0.047***	−0.007
	(3.14)	(−0.40)

(continued on next page)

TABLE 6 (continued)

Variables	Young	Mature
UEDown	0.184***	0.212***
	(4.14)	(3.66)
UEDown × *Sent*	−0.078***	−0.058**
	(−4.58)	(−2.51)
Number of observations	117,406	37,711
Adjusted R^2	4.17%	3.04%

Panel D: Volatile versus Stable Firms

Variables	Volatile	Stable
UEUp	1.008***	0.856***
	(28.40)	(21.35)
UEUp × *Sent*	0.029*	0.018
	(1.87)	(0.80)
UEDown	0.175***	0.261***
	(3.54)	(6.33)
UEDown × *Sent*	−0.077***	−0.045**
	(−4.26)	(−2.41)
Number of observations	108,186	71,043
Adjusted R^2	4.08%	4.17%

Panel E: Dividend Non-Payers versus High Dividend Payers

Variables	Dividend Non-Payers	High Dividend Payers
UEUp	1.061***	0.797***
	(35.08)	(17.45)
UEUp × *Sent*	0.052***	−0.076***
	(3.53)	(−2.65)
UEDown	0.167***	0.349***
	(4.33)	(6.48)
UEDown × *Sent*	−0.089***	−0.085***
	(−5.67)	(−3.37)
Number of observations	159,542	46,709
Adjusted R^2	4.21%	3.52%

Panel F: Growth/Value versus Staid Firms

Variables	Growth and Value	Others
UEUp	1.024***	1.070***
	(26.59)	(23.35)
UEUp × *Sent*	0.053***	0.041*
	(3.56)	(1.94)
UEDown	0.055	0.301***
	(0.98)	(5.66)
UEDown × *Sent*	−0.070***	−0.049**
	(−3.85)	(−2.39)
Number of observations	82,689	75,210
Adjusted R^2	4.27%	4.55%

(continued on next page)

TABLE 6 (continued)

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*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively.
We estimate the following regression for various sub-groups of stocks, and report the results in this table:

$$RET_{it} = \alpha_0 + \alpha_1 Down_{it} + \beta_0 UEUp_{it} + \beta_1 UEUp_{it} \times Sent_{t-1} + \gamma_0 UEDown_{it} + \gamma_1 UEDown_{it} \times Sent_{t-1} \\ + \beta_2 NonlUp_{it} + \beta_3 UEUp_{it} \times MktPE_{t-1} + \gamma_2 NonlDown_{it} + \gamma_3 UEDown_{it} \times MktPE_{t-1} + \varepsilon_{it}.$$

The model is estimated using OLS and the pooled data of quarterly earnings announcements over the period of 1972–2007. Cross-sectional characteristics we use to identify hard to value stocks are *size*, *age*, *volatility*, *market-to-book ratio*, and *dividend payment*. The bottom (top) 20 percent of the first principle component of these characteristics is defined as hard-to- (easy-to-) value stocks. Hard-to-value stocks are labeled *Exposed* Stocks and easy-to-value stocks are termed *Not Exposed* Stocks.
Panel A shows the estimates of the above regression for *Exposed* and *Not Exposed* Stocks.
Panels B through F report the estimates for small (large), young (mature), volatile (stable), non-dividend-paying (high-dividend-paying) stocks, and growth/value (others).
The t-statistics are reported in parentheses and are based on heteroscedasticity consistent standard errors and clustered by month.

Variable Definitions:
RET_{it} = cumulative abnormal return calculated per the methodology detailed in Daniel et al. (1997) from day −1 to +1 relative to the earnings announcement date;
UEUp (*UEDown*) = product of the unexpected earnings, *UE*, and the indicator variable, *Up* (*Down*), which takes the value of 1 if *UE* is positive (negative), and 0 otherwise;
Sent = monthly Baker-Wurgler Sentiment Index prevailing at the beginning of the month;
NonlUp and *NonlDown* = square of *UEUp* and the square of *UEDown*, respectively, multiplied by −1; and
MktPE = difference between the aggregate stock market PE of the lagged month and the average market PE of the prior 12 months.

the effect of sentiment might even reverse for some of the stocks that are categorized by sentiment-driven investors as bond-like investments. Specifically, Baker and Wurgler (2007) argue that firms that are large, mature, stable, and pay stable streams of high dividends might be perceived by investors as bond-like investments, and might experience overpricing (underpricing) in periods of low (high) sentiment as investors look for safe havens (high growth) for their investments. The estimated coefficients for *UEDown* × *Sent* are negative for both groups of stocks, indicating that effect of sentiment on the ERC of bad news is pervasive across the whole cross-section of stocks. Even so, the magnitude and the statistical significance of the coefficient is somewhat greater for *Exposed* stocks (coefficient = −0.080, t-statistic = −4.27) than for *Not Exposed* stocks (coefficient = −0.047, t-statistic = −2.20). Overall, the results in Panel A are broadly in line with our prediction that the effect of sentiment on stock price sensitivity to earnings news is more pronounced for harder to value and difficult to arbitrage stocks.

The results in Panels B to F are generally similar to those documented in Panel A above. The ERC for good news increases with sentiment for the small, young, volatile, non-dividend-paying, and the extreme *MEBE* firms, and the effect is statistically significant. In contrast, for large, mature, stable, high-dividend-paying, and moderate growth firms, the ERC for good news either decreases with sentiment or is insignificantly related to sentiment. The ERC for bad news decreases with sentiment for all the firms, but the statistical significance of the relation is greater for small, young, volatile, non-dividend-paying, and extreme market-to-book firms than for the corresponding large, mature, stable, high-dividend-paying, and medium market-to-book ratio firms. The results in Table 6 provide general support for the notion that the effect of sentiment on the stock price sensitivity to news varies cross-sectionally, and is stronger for firms that the prior sentiment literature finds are more affected by sentiment.

V. ROBUSTNESS CHECKS

Monthly Regressions

The stock returns associated with the earnings announcement that are close to each other in time are likely to be cross-sectionally correlated. In the regressions so far, the sample used is a pooled cross-sectional time-series dataset, and the test statistics are corrected for this cross-correlation by computing standard errors that are clustered by month. To check the robustness of our results, in this section, we adopt an alternative empirical strategy to control for the cross-correlations in the data. We first compute the average earnings response coefficients for each calendar month, and then regress the average monthly earnings response coefficients on investor sentiment at the beginning of the month to test whether sentiment influences the ERCs. Specifically, we estimate the following regression each month to compute the average ERC for each month for good as well as for bad news:

$$RET_{it} = \alpha_0 + \alpha_1 Down_{it} + \beta_1 UEUp_{it} + \gamma_1 UEDown_{it} + \delta_1 NonlUp_{it} + \delta_2 NonlDown_{it} + \varepsilon_{it}. \tag{6}$$

The coefficients β_1 and γ_1 are estimates of the ERCs for good news and bad news, respectively, for each calendar month t. We drop months that have fewer than 200 earnings announcements to ensure that the monthly estimates are not noisy.[20] We then use the estimated ERCs as the dependent variables in the second stage regressions of the following form:

$$ERC_t = \alpha_0 + \alpha_1 Sent_{t-1} + \alpha_2 MktPE_{t-1} + \varepsilon_t, \tag{7}$$

where *ERC* is either the ERC for good news or the ERC for bad news, *Sent* is the sentiment at the beginning of the month, and *MktPE* is the relative market *PE* computed at the beginning of the month. In the regression involving the ERC of good news, we expect the coefficient on *Sent* to be positive. Correspondingly, in the regression involving the ERC for bad news, we expect the coefficient on *Sent* to be negative.

The results are shown in Table 7. Consistent with the results documented in Table 2, the monthly ERC for good news is increasing in sentiment (coefficient = 0.056, t-statistic = 1.75), and the ERC for bad news is decreasing in sentiment (coefficient = −0.077, t-statistic = −2.08). These results support our main hypothesis that the prevailing investor sentiment sways the stock price response to earnings surprises in the direction of the sentiment.

Earnings Surprises Defined Relative to Analyst Forecasts

We argued earlier in the paper that the seasonally differenced earnings surprises are a better measure of earnings news to examine how sentiment influences the stock price sensitivity to earnings news. Nevertheless, in this section we examine how stock price sensitivity to news varies with sentiment when the news is defined by comparing the actual earning with the consensus analyst forecast. This alternative earnings news, which we label as forecast error (*FE*), is defined as follows:

$$FE = \frac{\text{Actual Earnings} - \text{Consensus Earnings Forecast}}{\text{Beginning of the Month Stock Price}}. \tag{8}$$

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[20] This removes 11 months from a total of 432 months in our sample period. In an alternative treatment of the noisy estimates for individual months, we drop the months that are identified as "influential" using the Belsley et al. (1980) methodology. Our results remain robust.

TABLE 7

Investor Sentiment and ERC Computed Monthly

Explanatory Variables	Dependent Variable	
	ERC_{Up}	ERC_{Down}
Intercept	1.228***	0.426***
	(40.95)	(9.68)
Sent	0.056*	−0.077**
	(1.75)	(−2.08)
MktPE	0.003	−0.003
	(0.34)	(−0.27)
Number of observations	421	421
Adjusted R^2	1.00%	1.00%

*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively.
This table provides estimates of the following model:

$$ERC_t = \alpha_0 + \alpha_1 Sent_{t-1} + \alpha_2 MktPE_{t-1} + \varepsilon_t.$$

The model is estimated using OLS and on a sample of 421 months over the period of 1972–2007.
The t-statistics are reported in parentheses and are based on heteroscedasticity consistent robust standard errors.

Variable Definitions:
ERC = earnings response coefficient for month *t* calculated from Equation (6);
ERCUp (*ERCDown*) = ERC for positive (negative) unexpected earnings;
Sent = monthly Baker-Wurgler Sentiment Index prevailing at the beginning of the month; and
MktPE = difference between the aggregate stock market PE of the lagged month and the average market PE of the prior 12 months.

Consensus earnings forecast in Equation (8) is the last median forecast available in the I/B/E/S summary file prior to the earnings announcement date. Actual earnings are also extracted from the I/B/E/S summary file. The stock price in the denominator is the share price as at the beginning of the month in which earnings is announced. The actual and forecasted earnings as well as the share price are adjusted for stock splits. For our initial sample of earnings announcements in Table 1, we extract the relevant data from the I/B/E/S summary file for the period 1985 through 2007. Our final sample of analyst forecast errors includes 196,243 observations.

We then replace *UE* with *FE* in Equation (2) to examine how investor sentiment influences the stock price sensitivity to earnings news when the news is defined based on analyst forecast errors. In untabulated results, we find that the coefficient on good news interacted with sentiment is positive and significant (coefficient = 0.227, t-statistic = 2.12), while the coefficient on bad news interacted with sentiment is negative and significant (coefficient = −0.157, t-statistic = −2.93). These results show that the evidence of the influence of sentiment on stock price sensitivity to earnings news continues to hold when we define earnings news by comparing actual earnings to analyst forecasts.

Controlling for Additional Cross-Sectional Determinants of ERC

Prior literature indicates that the ERC could also vary systematically with certain other firm-specific variables, such as size, growth, past earnings volatility, and earnings persistence (DeFond and Park 2001). To examine the robustness of our results to these cross-sectional effects, we interact *UEUp* and *UEDown* with each of *Size*, *MB*, *EVOL*, and *PERS*, and add these as additional controls to our regression in Table 2. *Size* is the decile rank based on the market capitalization of the stock in month *t*–1. *MB* is the decile rank based on the market-to-book ratio. *EVOL* is the standard deviation of previous four quarters' actual earnings divided by the beginning of the month stock price. Finally, *PERS* is the persistence of earnings measured as the coefficient from an autoregressive model, which is seasonally differenced.

In untabulated results, we find that the magnitude of the coefficients and the statistical significance for our key variables, *UEUp* × *Sent* and *UEDown* × *Sent*, remain largely similar to what we report in Table 2 for the parsimonious model. The coefficient on *UEUp* × *Sent* is 0.027 with a t-statistic of 2.28, and the coefficient on *UEDown* × *Sent* is –0.053 with a t-statistic of –4.34. The coefficients associated with the control variables mostly show up with the expected signs.

Loss Firms

We remove the loss firms from our main analysis because the low earnings response coefficients for such firms (Hayn 1995) might make them unsuitable to study the effect of sentiment on ERC. Nevertheless, in this section, we estimate Equation (2) for the subsample of loss firms. In untabulated results, we find that the impact of sentiment on ERC is in the same direction as that of the profitable firms but is not statistically significant. The coefficient on *UEUp* × *Sent* is 0.013 with a t-statistic of 0.79, whereas the coefficient on *UEDown* × *Sent* is –0.011 with a t-statistic of –1.37. The explanatory power of the model for loss firms, however, is far smaller than that for profitable firms, mirroring the findings of Hayn (1995).

VI. CONCLUSION

Most prior studies on stock price response to earnings follow the efficient market paradigm, and examine cross-sectional differences in the stock price response to earnings. This study takes a time-series approach and helps answer the call for more work that incorporates the findings of behavioral finance in capital market research in accounting (Lee 2001). Using a recent proxy of investor sentiment constructed by Baker and Wurgler (2006, 2007), we examine how the stock price reaction to earnings news varies with investor sentiment. Our results show that the stock price response to good earnings news is higher during periods of high sentiment, whereas stock price reaction to bad earnings news is higher during periods of low sentiment. This is consistent with the notion that as sentiment increases, investors place increasingly optimistic valuations on the incremental cash flows embedded in earnings announcements. The relation between sentiment and stock price response is more pronounced for stocks that have more subjective valuations, that is, small stocks, young stocks, non-dividend-paying stocks, volatile stocks, and growth and distressed stocks.

Our evidence that stock price reactions to earnings news contain both rational and irrational components has implications for the event study literature. Many of these studies attempt to quantify the valuation implications of accounting information. Our results imply that value and price could diverge in the short run because of the impact of behavioral biases on how accounting information is impounded into prices. Hence, relying solely on the short-term market reaction as a measure of the valuation impact of accounting information may be insufficient. Researchers need to look beyond the immediate stock price response and consider additional information signals, such as the longer term accounting and stock price performance, to fully understand the implications of an information event on the fundamental value of the firm.

We also connect the sentiment-related (mis)pricing of earnings to the broader effect sentiment has on prices and future returns. Despite several papers that establish a relation between sentiment and mispricing of stocks (Baker and Wurgler 2006; Lemmon and Portniaguina 2006; Qiu and Welch 2006), the precise channel(s) by which sentiment causes such mispricing is not well understood. Our evidence adds to the sentiment literature by identifying misreaction to earnings news as one possible channel through which sentiment causes mispricing of stocks.

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Do Unverifiable Disclosures Matter? Evidence from Peer-to-Peer Lending

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ABSTRACT: The role of disclosure in attenuating market inefficiencies has been the subject of extensive research. While costless, voluntary, and unverifiable disclosures are unlikely to be credible sources of information, prior research demonstrates that individuals' decisions can be influenced by uninformative content. I use a unique dataset from a peer-to-peer lending website, Prosper.com, to demonstrate an economically large effect of voluntary, unverifiable disclosures in reducing the cost of debt. My results show an additional unverifiable disclosure is associated with a 1.27 percentage point reduction in interest rate and an 8 percent increase in bidding activity.

Keywords: *voluntary disclosure; cost of debt; asymmetric information; microfinance; behavioral economics; Prosper.com.*

Data Availability: *All data are publicly available from sources identified in the paper.*

I. INTRODUCTION

The role of disclosure in attenuating inefficiencies in financial markets has been the subject of extensive research.[1] Generally, these studies focus on mandated and audited financial reports. The lack of good measures for voluntary disclosures limits empirical work in this area. In the absence of any cost, it is unclear that voluntary disclosures could be seen as a credible source of information and thus should not influence investment decisions. However, prior research in psychology and behavioral economics demonstrates that people incorporate objectively uninformative content into their decisions in certain circumstances (Nisbett et al. 1981; Gilbert

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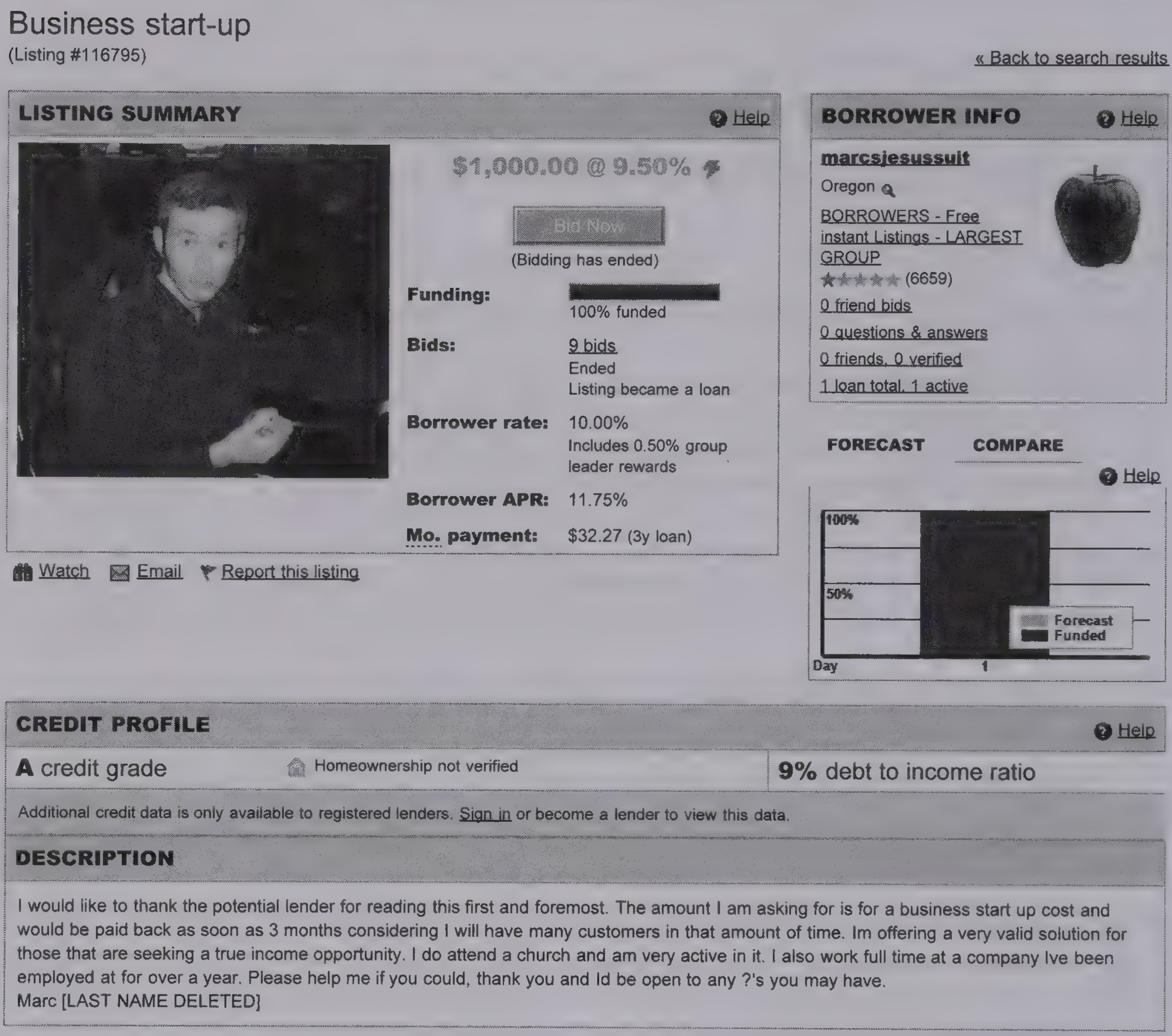
[1] See Healy and Palepu (2001) for a review of this literature.

FIGURE 1
Example Loan Listing on Prosper.com



Business start-up
(Listing #116795)

LISTING SUMMARY

$1,000.00 @ 9.50%

(Bidding has ended)

Funding: 100% funded

Bids: 9 bids
Ended
Listing became a loan

Borrower rate: 10.00%
Includes 0.50% group leader rewards

Borrower APR: 11.75%

Mo. payment: $32.27 (3y loan)

BORROWER INFO

marcsjesussuit
Oregon
BORROWERS - Free instant Listings - LARGEST GROUP
★★★★★ (6659)
0 friend bids
0 questions & answers
0 friends, 0 verified
1 loan total, 1 active

CREDIT PROFILE

A credit grade | Homeownership not verified | **9%** debt to income ratio

Additional credit data is only available to registered lenders. Sign in or become a lender to view this data.

DESCRIPTION

I would like to thank the potential lender for reading this first and foremost. The amount I am asking for is for a business start up cost and would be paid back as soon as 3 months considering I will have many customers in that amount of time. Im offering a very valid solution for those that are seeking a true income opportunity. I do attend a church and am very active in it. I also work full time at a company Ive been employed at for over a year. Please help me if you could, thank you and Id be open to any ?'s you may have.
Marc [LAST NAME DELETED]

et al. 1993; DellaVigna and Gentzkow 2009). Therefore, the influence of voluntary, unverifiable disclosures on investing decisions is an interesting empirical question.

In this study, I use a unique dataset of unsecured personal loans, made to individuals, originated through Prosper.com[2] to investigate the relationship between voluntary, unverifiable disclosures and the cost of debt. Prosper.com is an online, peer-to-peer lending marketplace that provides a service for lending that is similar to eBay.com's service for consumer products. On the Prosper website, potential borrowers apply for a loan by creating a loan listing. The loan listing states the amount of money they wish to borrow and the maximum interest rate they are willing to pay. Figures 1 and 2 give examples of loan listings. Potential lenders[3] bid on portions of loans by setting the minimum interest rate that they are willing to receive and the amount of the loan they wish to fund.

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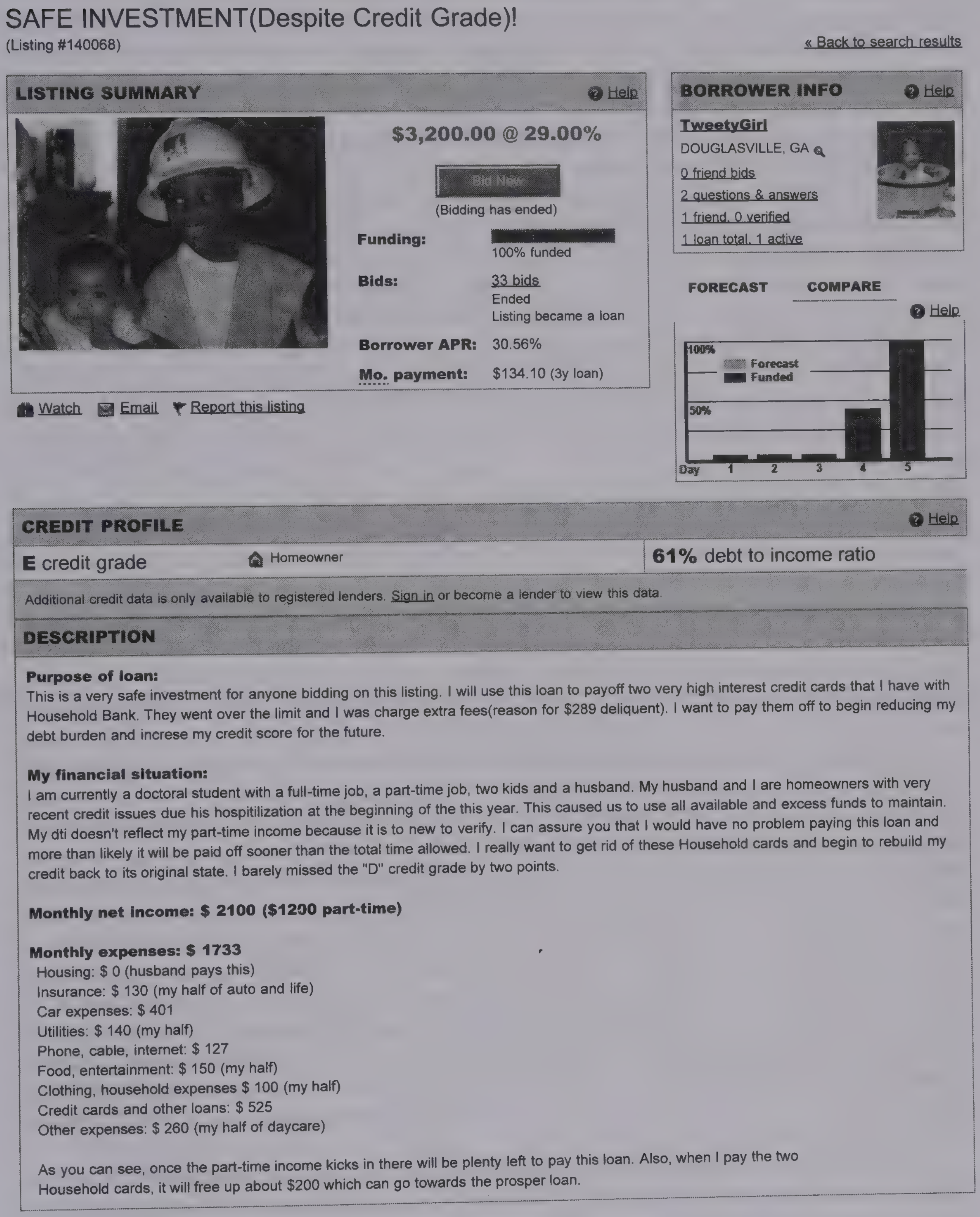
[2] All data used in this study are publicly available at http://www.prosper.com/tools/

[3] Lenders, as they are referred to here, are actually purchasing loans, making them technically loan buyers. However, for simplicity I refer to them as lenders throughout the paper.

FIGURE 2
Example Loan Listing on Prosper.com



SAFE INVESTMENT(Despite Credit Grade)!

(Listing #140068)

LISTING SUMMARY

$3,200.00 @ 29.00%

(Bidding has ended)

Funding:	100% funded
Bids:	33 bids Ended Listing became a loan
Borrower APR:	30.56%
Mo. payment:	$134.10 (3y loan)

BORROWER INFO

TweetyGirl
DOUGLASVILLE, GA
0 friend bids
2 questions & answers
1 friend, 0 verified
1 loan total, 1 active

CREDIT PROFILE

E credit grade — Homeowner — **61%** debt to income ratio

Additional credit data is only available to registered lenders. Sign in or become a lender to view this data.

DESCRIPTION

Purpose of loan:
This is a very safe investment for anyone bidding on this listing. I will use this loan to payoff two very high interest credit cards that I have with Household Bank. They went over the limit and I was charge extra fees(reason for $289 deliquent). I want to pay them off to begin reducing my debt burden and increse my credit score for the future.

My financial situation:
I am currently a doctoral student with a full-time job, a part-time job, two kids and a husband. My husband and I are homeowners with very recent credit issues due his hospitilization at the beginning of the this year. This caused us to use all available and excess funds to maintain. My dti doesn't reflect my part-time income because it is to new to verify. I can assure you that I would have no problem paying this loan and more than likely it will be paid off sooner than the total time allowed. I really want to get rid of these Household cards and begin to rebuild my credit back to its original state. I barely missed the "D" credit grade by two points.

Monthly net income: $ 2100 ($1200 part-time)

Monthly expenses: $ 1733
Housing: $ 0 (husband pays this)
Insurance: $ 130 (my half of auto and life)
Car expenses: $ 401
Utilities: $ 140 (my half)
Phone, cable, internet: $ 127
Food, entertainment: $ 150 (my half)
Clothing, household expenses $ 100 (my half)
Credit cards and other loans: $ 525
Other expenses: $ 260 (my half of daycare)

As you can see, once the part-time income kicks in there will be plenty left to pay this loan. Also, when I pay the two Household cards, it will free up about $200 which can go towards the prosper loan.

While Prosper verifies borrowers' identities and provides certain information obtained from their credit reports, a borrower's loan listing may also include voluntary and unverified personal information, such as intended use of proceeds, interest rates on other debts, explanations for poor credit ratings, or a picture. Using the natural variation in the amount of these voluntary, unverified disclosures in the loan listings, I identify the effect these disclosures have on the number of bids a loan listing receives and the final interest rate on a loan. Data are available for both successful loan listings that resulted in a loan and unsuccessful ones that did not receive enough bids to fund the listing.

My results show that these voluntary disclosures made in loan listings do affect the loans' interest rates. More unverifiable disclosures are associated with a lower interest rate on a loan. Additionally, more unverifiable disclosures increase the bidding activity on a loan listing. These effects are economically large: an additional disclosure is associated with a 1.27 percentage point reduction in interest rate and an 8 percent increase in bidding activity. Given that these essentially costless disclosures reduce interest costs for borrowers, it may seem surprising that not all borrowers make as many voluntary disclosures as possible, regardless of their truthfulness. At least two explanations may account for this behavior. First, the borrowers on Prosper may not fully understand the effectiveness of disclosures in reducing their interest costs. If the market had persisted,[4] then it is possible that learning would have occurred and the amount of disclosures made would have increased. Second, borrowers may be reluctant to use misleading disclosures to reduce their interest rates. This second argument is consistent with work demonstrating an aversion to lying, even when truth-telling is costly (Evans et al. 2001; Gneezy 2005).

My results contribute to a literature that demonstrates the role of presentation and persuasion in decision making by showing that unverifiable disclosures influence investors' decisions in a natural setting with large economic consequences. These findings may interest regulators as these nontraditional financial markets grow in prevalence. Since Prosper's inception in 2006, several other peer-to-peer lending sites have been established, such as Lending Club and Loanio. These online markets have already garnered the attention of the U.S. Securities and Exchange Commission, which imposed a cease-and-desist order against Prosper in 2008, determining that the website must register under the Securities Act of 1933.[5]

This study's findings also have implications for banking and microfinance. Prior research demonstrates the importance of relationships between borrowers and lenders when information asymmetries are severe (Petersen and Rajan 1994; Berger and Udell 1995). In the absence of a relationship, many microfinance institutions rely on group lending and liability to overcome adverse selection issues. While groups of affiliated lenders exist on Prosper, these groups do not have joint liability for the loans of their members. In the absence of group liability, methods developed by microfinance institutions to manage incentives include offering very small, but successively larger loans, and threatening to cut off access to future credit in the case of default (Armendariz de Aghion and Morduch 2010). However, in the context of Prosper, competition among lenders, and other lending websites, is likely to undermine the success of such efforts. Also, repeated borrowing is observed to only a limited degree. Thus, the Prosper marketplace offers an opportunity to study adverse selection problems in lending when traditional remedies are not available. Results indicate participants in the Prosper marketplace appear to be willing to rely on unverifiable disclosures to overcome informational asymmetries.

Finally, this study's setting offers several advantages over studies of voluntary disclosures by publicly traded firms. First, the direction of causality is clearer in the current study than in those using publicly traded firms. In this paper's setting, the disclosures must precede any investment decision. In studies using publicly traded firms, the timing of events (that is, the disclosure and change in firm performance or value) is often less clear. Further, disclosure measures in studies of publicly traded firms are often incomplete, as firms can make disclosures through many alternative avenues, such as annual reports, management forecasts, or conference calls. In this study, all information available to potential lenders is contained in the loan listing, reducing the potential for incomplete measures. Last, most disclosures made by public firms, such as management forecasts, are verifiable in at least an *ex post* sense, giving them some level of credibility. Truly unverifiable disclosures by public firms compose such a small portion of the total information surrounding the firm that it is difficult to identify the effect of these disclosures. A natural solution to this problem is to examine disclosure effects in less developed markets (Verrecchia 2001). My paper contributes to the accounting literature by using such a setting to understand the impact of unverifiable disclosures on participants in a real world market. As Verrecchia (2001, 174) notes, "[A]s the theory of disclosure matures, it seems reasonable to inquire whether the empirical literature can provide additional insights into the economic consequences of disclosure."

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[4] Loans on Prosper are no longer priced through the bidding process analyzed in this study.

[5] Prosper has since registered and is again operational. All data in this study precede this action.

The remainder of the paper is organized as follows. Section II outlines prior research related to disclosure and develops hypotheses. Section III describes the data source. Section IV discusses the research design. Section V details the main results, while Section VI presents alternative models to test for robustness. Section VII examines the association of unverifiable disclosures and loan performance. Section VIII concludes.

II. RELATED RESEARCH AND HYPOTHESIS DEVELOPMENT

Several contemporaneous working papers also examine the setting of the Prosper marketplace. Some of these studies examine the social aspects of Prosper, such as whether membership in a group of affiliated borrowers affects the success of loan listings (Freedman and Jin 2008; Lin et al. 2009).[6] Other studies examine aspects of the pictures of borrowers included in loan listings to investigate the effect of demographic characteristics such as gender, age, or race (Herzenstein et al. 2008; Pope and Sydnor 2011). Duarte et al. (2010) examine even more subjective aspects of borrower pictures, including trustworthiness and attractiveness. Herzenstein et al. (2011) examine claims of similarly subjective characteristics, such as trustworthiness or piety, in listings. Klafft (2008) offers an overview of how credit information and borrower-provided pictures are related to listing success, while Iyer et al. (2010) investigate how closely lenders can infer actual credit scores. Several of these studies attempt to control for information in the narrative description of the loan listings. Proxies used include word count (Freedman and Jin 2008; Lin et al. 2009), indicator variables for "general" or "specific" personal information (Herzenstein et al. 2008), or the residual of a regression of interest rate on credit variables (Iyer et al. 2010). Note this last measure is only available for successful listings that result in loans.

The present study differs from these works in that I code for the presence of specific disclosures in the loan listings. The unverifiable disclosure measure that I develop allows me to investigate specifically which elements of the loan listing description lenders respond to. I discuss this disclosure measure in detail in Section IV. I am aware of no other study that codes for the presence of particular disclosures in the loan listings.

Empirical studies of disclosure typically focus on firms listed on major exchanges, where voluntary disclosures represent a small fraction of the available information relative to mandatory, regulated filings. This research generally finds that costs of capital are decreasing in the amount of disclosure (Botosan 1997; Sengupta 1998). Sivakumar and Waymire (1994) offer some insight into the effects of disclosures in an unregulated environment by investigating stock market reactions to voluntary disclosures for companies listed on the New York Stock Exchange during 1905–1910. Reporting requirements were minimal during this time. Annual earnings disclosures were mandatory; however, there were no accounting standards or audit requirements. Nonetheless, significant changes in price and trading volume are observed in association with discretionary disclosures during this period, suggesting that the disclosures were seen as at least partially credible. In a similar vein, Price (2000) investigates the association between earnings-related disclosures by franchisers and the fees they charge to franchisees. Disclosure of earnings by franchisers is optional. If disclosed, then the earnings claim is not subject to verification. The study indicates that franchisers who disclose earnings charge their franchisees higher fees. The willingness of franchisees to accept these fees suggests that they perceive these earnings disclosures to be credible. In the context of initial public offerings (IPOs), Leone et al. (2007) demonstrate that firms that volunteer more detailed intended-use-of-proceeds disclosures enjoy less IPO underpricing. Lang and Lundholm (2000) find firms increase voluntary disclosures before seasoned equity offerings to "hype" their stock. Upon the announcement of the offering, the market penalizes these firms, as it suspects their ulterior motive for the increase in disclosure. However, the reduction in stock price is incomplete, suggesting firms can use "hype" to reduce their cost of equity capital.

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[6] Groups on Prosper do not have joint liability for the loans of their members, unlike some other micro-lending settings.

In summary, empirical research on unverifiable disclosures generally indicates market participants perceive such disclosures as being at least partially credible. However, voluntary disclosures made by firms, such as management forecasts, are often verifiable in an *ex post* sense. It is not clear that these results would generalize to a setting in which there is no opportunity for *ex post* settling up. While shareholders may have the opportunity to sue for misleading information, lenders on Prosper have no recourse against borrowers who may have used misleading information in their loan listings. Borrowers are never individually identified to lenders, and Prosper pursues all collection efforts on the lender's behalf. Thus, this setting offers a unique opportunity to further our understanding of how investors use unverifiable disclosures in decisions that have large economic consequences.

Analytical research on disclosures has typically focused on models where disclosures must be made truthfully. Early work in bargaining typically assumes signals are costless and verifiable (Grossman 1981; Milgrom 1981). However, since the seminal paper of Crawford and Sobel (1982), several analytical papers have explored scenarios in which the assumption that disclosures must be made truthfully is relaxed. In these so-called "cheap-talk" games, disclosures that are made may be false. Gigler (1994) shows that even when disclosures are unverifiable, the tension between proprietary costs and the wish to signal favorable information can make disclosures credible. That is, the cost associated with making the disclosure lends it credibility. However, in the setting of Prosper, one would not expect there to be proprietary costs associated with disclosing personal information for the purpose of obtaining a personal loan. Stocken (2000) considers a model in which a manager can make unverifiable disclosures to a potential investor. In this model the manager gives an unverifiable signal to an investor concerning the potential payoff of a project. In a single period game, no informative disclosures occur in equilibrium. In a repeated game, however, the manager will most likely disclose, and do so truthfully, as benefits exist to developing a reputation for telling the truth. While repeated interaction could produce credible disclosures on Prosper, the incidence of repeat borrowing is relatively low. During my sample period, 7 percent of borrowers obtained more than one loan through Prosper; however, less than 1 percent obtained more than two loans. Further, Stocken's (2000) result depends on both the number of interactions and the manager's discount factor being sufficiently high. A reading of the loan listings on Prosper indicates that many borrowers are financially distressed and already have large debts. This revealed preference for consumption over savings implies a low discount factor.

Models from the cheap-talk literature recognize that unverifiable disclosures are only relevant when agents' incentives are at least partially aligned (Farrell and Rabin 1996). However, evidence from both experiments and empirical studies demonstrates that people fail to fully account for conflicts of interest (Cain et al. 2005; Malmendier and Shanthikumar 2007). The tendency of people to rely on false or irrelevant information in decision making has been well established in the psychology literature. People behave as if their first reaction is to believe any information they are presented with (Gilbert 1991; Gilbert et al. 1993) and have difficulty ignoring irrelevant information in making decisions (Nisbett et al. 1981; Kahneman et al. 1982). Experimental research in marketing has produced results consistent with these findings. Carpenter et al. (1994) show that consumers value differentiating, but irrelevant, product attributes. Mandel and Johnson (2002) find changing only the background colors and images on a website influences product choices. Also, in psychology, Scharlemann et al. (2001) demonstrate subjects in a one-shot "trust" game exhibit more (potentially costly) cooperative behavior when their counterpart is represented by a picture of a smiling face. Overall, this stream of research suggests that lenders on Prosper would be influenced by the presence of unverifiable information in loan listings, especially given its prominent display in the listings (see Figures 1 and 2).

In related work in behavioral economics, Mullainathan et al. (2008) develop a model, in the spirit of the cheap-talk literature, that highlights how uninformative material can influence choice. Motivated by the psychology and marketing literature, they argue that rather than fully updating on new information, people think "coarsely" in that they partition potential outcomes into categories. The authors then demonstrate how this mental simplification strategy can result in useless information influencing choice, either through the misapplication of information within a partition, or by influencing the partition choice. Their model is consistent with results observed in experimental settings (for example, see Krueger and Clement 1994). A field experiment by Bertrand et al. (2010) also examines the effect of uninformative content on consumer choice. In loan offer letters sent to potential borrowers, the authors find that either including a picture of a smiling female, offering only one example of loans terms (versus several examples), or not suggesting a use for the loan increases offer acceptance by the same amount as a two-percentage point decrease in the interest rate of the loan. This result demonstrates that uninformative material can significantly affect behavior.

In sum, while traditional work in disclosure argues that unverifiable disclosures should be irrelevant, behavioral studies demonstrate that objectively uninformative content affects individual behavior. In the context of Prosper, if lenders are influenced by the voluntary, unverifiable disclosures made by borrowers in their loan listings, then I expect to see interest rates decreasing in the amount of disclosure. This leads to the following hypothesis:

H1: Interest rates are decreasing in the amount of voluntary, unverifiable disclosures in a loan listing.

If unverifiable disclosures in a loan listing are seen as convincing, then they should not only result in lower interest rates, but also greater bidding activity as well (Beaver 1968; Bamber 1986; Kim and Verrecchia 1991). That is, if these disclosures increase perceptions of quality regarding the loan listing, then I expect a listing with more disclosures to generate more bids. This prediction is consistent with work in auction theory that allows for entry of bidders (rather than assuming a fixed number of bidders). In this literature, it is costly to submit a bid. This cost is often interpreted as the cost of preparing the bid and processing information to learn what the object in question is worth. The number of entrants in an auction is decreasing in this cost (Samuelson 1985; McAfee and McMillan 1987; Levin and Smith 1994). If the unverifiable disclosures in a loan listing are perceived to be credible, then they should decrease the effort of learning the value of the loan, thus increasing the amount of bidding. This leads to my second hypothesis:

H2: The number of bids on a loan listing is increasing in the amount of voluntary, unverifiable disclosures in a loan listing.

Finally, I expect the relations in H1 and H2 to be stronger for loan listings by borrowers with relatively poor credit. Borrowers with good credit are likely able to obtain sufficient bids to fund their loan and a favorable rate by virtue of verifiable information alone (for example, their credit reports). Potential borrowers with poor credit will likely rely more heavily on the descriptive portion of the loan listing (that is, the section where they tell their personal story, which is unverifiable) in an effort to differentiate themselves from other competing borrowers with poor credit. Additionally, unverifiable disclosures may be more important for potential borrowers with poor credit quality because credit scores are based in part on the length of credit history. Therefore, a low credit score may reflect a dearth of verifiable information about a potential borrower. These predictions are formalized in the following hypotheses:

H3a: The relation between interest rates and the amount of voluntary, unverifiable disclosures in a loan listing is stronger for listings with relatively poor credit quality.

H3b: The relation between the number of bids and the amount of voluntary, unverifiable disclosures in a loan listing is stronger for listings with relatively poor credit quality.

Note that my measure of voluntary, unverifiable disclosure is a count of the number of specific items disclosed in the loan listing. While the specific content of what is revealed in a given disclosure is also likely to be important in determining its effect, I code only for the presence of a disclosure to increase objectivity. An indicator variable for the presence of a disclosure is a noisy proxy for its content. So to the extent the specific content of disclosures is important, my measure simply offers less powerful tests, biasing against finding a result. In additional analysis, I also investigate which types of disclosures have the greatest effect on lenders' decisions.

III. DATA

As outlined in Section I, the dataset of personal loan listings is collected from Prosper.com. All loans made through Prosper are three-year unsecured loans. Potential borrowers submit personal information to Prosper, enabling Prosper to verify their identity and obtain their credit reports. Prosper assigns a credit grade to each potential borrower based on his or her credit score.[7] Prosper also calculates a debt-to-income ratio (DTI) for each potential borrower by dividing the borrower's nonhousing debt payments (taken from their credit report) by his or her income. Income is self-reported by the borrower. Employment status,[8] length of employment, and occupation (chosen from a list) are also self-reported by potential borrowers.

After Prosper has obtained their credit information, potential borrowers can make loan listings by selecting the size of the loan they wish to obtain (maximum loan size is $25,000) and the maximum interest rate they are willing to pay (the maximum rate allowed is 35 percent). This information, along with their credit grade,[9] DTI ratio, employment status, length of employment, occupation, and stated income become part of the loan listing. Additionally, potential borrowers are encouraged to include in their loan listing a picture and additional personal information as to why they are good loan candidates in a free-form text field. This additional information often includes items such as the purpose of the loan (intended use of proceeds), current interest rates on other debts, explanations for poor credit grades, and monthly budgets. The picture and additional personal information are not verified by Prosper. Examples of loan listings are given in Figures 1 and 2.[10]

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[7] AA = 760 and up, A = 720–759, B = 680–719, C = 640–679, D = 600–639, E = 560–599, HR (high risk) = 520–559. Persons with scores below 520 are not allowed to create a loan listing.

[8] Full-time, part-time, self-employed, retired, or not employed.

[9] Lenders have the ability to view a variety of credit information in addition to the credit grade, including number of accounts currently delinquent, amount delinquent, delinquencies in last seven years, public records in last 12 months, public records in last ten years, inquiries in last six months, first credit line, current credit lines, open credit lines, total credit lines, revolving credit balance, and bankcard utilization.

Once the borrower has made the loan listing, lenders may place bids on the listing. Lenders bid on loans by stating the amount of the loan they wish to fund (minimum of $50) and the minimum interest rate they are willing to receive (their reservation rate). Importantly, many different lenders may bid on and fund a loan listing. If, when the loan listing closes, the amount of money bid by lenders meets or exceeds the amount of the loan listing, then the listing becomes a loan. Winning bidders fund the loan in the amount of their bid and receive a note also equal to the amount of their winning bid. In the case that the total amount of bids exceeds the amount of the loan, the bidders with the lowest reservation rates are given priority in funding the loan. In this way, competition among lenders can lower the interest rate on a loan below the borrower's maximum acceptable rate.

Prosper makes money on this process by charging a closing fee to borrowers and a servicing fee to lenders. Penalties for default on Prosper loans are similar to those on other consumer debt. Delinquencies are reported to credit-reporting agencies, so borrowers' credit reports are adversely affected. Prosper also pursues collection efforts (through collections agencies) on behalf of the lenders. In no case can individual lenders pursue collection efforts themselves. Borrowers and lenders are identified to each other on the website only by screen names. Personal identities are not revealed.

The population of loan listings used in this study is all loan listings created on Prosper between February 12, 2007, and October 31, 2008. This period was selected because Prosper's informational policies remained constant over this time. In total, this includes 246,841 loan listings, 21,450 of which were successfully funded and 225,391 of which were not funded. The analysis that follows is based on a stratified random sample of 500 funded listings and 500 unfunded listings (1,000 listings in total).[11] I use a random sample rather than the entire population of listings because each loan listing must be read and hand-coded in developing the disclosure measure. I stratify over-funded and unfunded listings to ensure sufficient variation in each type of listing.

IV. RESEARCH DESIGN

Disclosure Measures

To evaluate the effect that voluntary, unverifiable disclosures have on the interest rate at which a loan listing funds, I must first develop a measure of such disclosures. To do this I grade each loan listing for the presence of specific voluntary and unverified information. One point is awarded for each item disclosed. Items scored are the purpose of the loan, income amount, income source, education, amount of other debt, interest rate on other debt, explanation for poor credit grade, listing of monthly expenses, and a picture of a person (presumably the borrower). All information I code is from the description section of the loan listing, which is not subject to verification.

The sum of all points awarded to a loan listing is its disclosure score (*DSCORE*). A summary of the definitions of the components of *DSCORE* is given in Panel A of Table 1, along with

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[10] Some features of Prosper.com have changed since the time period examined in this study. All features described here relate to the operation of Prosper prior to November 2008. Many of the details of Prosper's operations during this time period are still available in the firm's SEC filings.

[11] Weights are not used in the following analysis; however, all inferences are robust to using sampling weights.

TABLE 1

Disclosure Score (*DSCORE*) Components

Panel A: Variable Definitions

Variable	Definition
Purpose	1 if purpose of loan given, 0 otherwise
Income	1 if income given, 0 otherwise
Inc Source	1 if income source given, 0 otherwise
Education	1 if education level given, 0 otherwise
Other Debt	1 if amount of other debt given, 0 otherwise
Other Debt Rate	1 if interest rate on other debt given, 0 otherwise
Poor Credit Expl	1 if explanation for poor credit grade given, 0 otherwise
Expenses	1 if monthly expenses listed, 0 otherwise
Picture	1 if picture of person included in listing, 0 otherwise

Panel B: Summary Statistics

	Unfunded			Funded			Difference in Means	
Variables	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Diff.	Std. Err.
Purpose	500	0.87	0.34	500	0.93	0.25	−0.07***	(0.019)
Income	500	0.79	0.41	500	0.74	0.44	0.05+	(0.027)
Inc Source	500	0.30	0.46	500	0.39	0.49	−0.09**	(0.030)
Education	500	0.07	0.26	500	0.10	0.30	−0.02	(0.018)
Other Debt	500	0.12	0.32	500	0.17	0.37	−0.05*	(0.022)
Other Debt Rate	500	0.05	0.21	500	0.11	0.31	−0.06***	(0.017)
Poor Credit Expl	500	0.21	0.41	500	0.21	0.41	0.00	(0.026)
Expenses	500	0.80	0.40	500	0.71	0.45	0.09**	(0.027)
Picture	500	0.50	0.50	500	0.59	0.49	−0.09**	(0.031)

Panel C: *DSCORE* Frequencies

	Frequency		
DSCORE	Unfunded	Funded	Total
0	18	8	26
1	25	38	63
2	57	56	113
3	123	90	213
4	130	113	243
5	86	108	194
6	41	56	97
7	16	23	39
8	4	6	10
9	0	2	2
Total	500	500	1,000

+, *, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively (for two-tailed tests).
DSCORE for each loan listing in the sample equals the sum of the nine indicator variables listed in Panel A.

summary statistics in Panel B. Below I describe each of the components of the disclosure score in some detail.

Purpose: A point is awarded if the borrower states the intended use for the proceeds of the loan. If the purpose of the loan is debt consolidation, then I create an additional indicator variable, *DEBT*. Debt consolidation is the most commonly stated purpose for borrowing, and I expect loans with this purpose to be perceived as relatively less risky than other loans, because a borrower's financial situation is likely to improve if he or she can consolidate debt. Therefore, I include *DEBT* as a separate covariate outside of *DSCORE* in several of my model specifications.

Income Amount: Income amount refers to a disclosure of a specific monthly or yearly income number. This may include other income, such as that coming from a spouse or self-employment, which is not used in calculating the borrower's debt-to-income ratio (DTI). I include DTI as a separate control in the analysis. Because the borrower's self-reported income only indicates a $25,000 interval in which his or her income falls, a disclosure of a specific income value will provide incremental information to the lender. Thus a point is awarded for this disclosure.

Income Source: This disclosure specifies the source of the borrower's income. This may refer to additional sources of income beyond the mandatory job title given in the loan listing. It may also provide a more detailed description of the borrower's position. A point is not awarded for this disclosure if the borrower simply indicates the industry in which he or she is employed.

Education: A point is awarded if the borrower states in the listing the level of education he or she has achieved. However, statements such as "I need a loan to pay off student loans" or "pay for my last semester of tuition" do not earn points, as it is not clear if the borrower has completed or will complete the educational program.

Amount of other Debt: A point is awarded if the borrower reports outstanding balances of other debt.

Interest Rate on other Debt: A point is awarded if the listing reports the interest rate on at least one of the borrower's other debts. A specific interest rate must be stated. For example, simply stating that the borrower wants to use the loan to consolidate other high-interest debt does not earn a point without explicitly stating at least one rate.

Explanation of Poor Credit: A point is awarded if the borrower explains why his or her credit grade is low. The statement must be specific with respect to the reason the credit grade is low. For example, the statement "I need the loan because I have fallen behind on my mortgage payments" would not earn a point by itself, even though this is likely at least partially the source of the poor credit grade. Stating "I was injured in a car accident and have been unable to work, thus have trouble keeping up with bills" would earn a point.

Monthly Expenses: A point is awarded if the borrower lists their monthly expenses, for example, a budget.

Picture: A point is awarded if the listing includes a picture of a person, independent of who the picture represents.

Figures 1 and 2 offer examples of these disclosures. The loan listings in Figures 1 and 2 earn a point for the *Picture* disclosure. Likewise, both listings earn a point for stating the purpose of the loan, which is "for a business start up" in Figure 1 and "to payoff [sic] two very high interest credit cards" in Figure 2. The loan listing in Figure 2 also includes *Explanation of Poor Credit* as "credit issues due his hospitilization [sic]," *Income,* and *Monthly Expenses*. In particular, note that the listing in Figure 2 discloses income that would not be used in calculating the borrower's DTI.

Panel B of Table 1 reports summary statistics for the components of the disclosure score (*DSCORE*) in the sample. As noted previously, each element of the score is an indicator variable taking a value of 1 if the disclosure is present in the loan listing, and 0 otherwise. Most elements of the disclosure score display significant variation, although *Purpose, Education,* and *Other Debt Rate* have relatively low variation.

Panel B of Table 1 also reports that, for the nine disclosure components, disclosure frequency is greater for funded loans in six components, and five of these differences are statistically significant. Panel C of Table 1 reports the frequency of different levels of *DSCORE*. The median number of disclosures across all loan listings is 4. Successfully funded loans have a higher frequency than unfunded loans in each *DSCORE* category above 4. This preliminary evidence suggests lenders consider these unverifiable disclosures in the bidding process.

Control Variables

The variables used to control for other likely sources of variation in the rate at which a loan listing is funded and the number of bids it receives are auto-funding, maximum interest rate, loan amount, credit grade, homeownership, DTI, two-year Treasury bond yield, group membership, and prior loan performance. Each of these variables is described next.

Auto-Funding (AUTOF): A listing with automatic funding is closed as soon as the requested amount is met by bidders, so that additional bidders cannot bid down the rate once the loan is fully funded. This is an option for borrowers who want to fund a loan quickly. I expect auto-funded loans to have higher interest rates, as there is less opportunity for competition among bidders. This variable is an indicator variable taking value of 1 if the loan uses auto-funding, and 0 otherwise.

Maximum Interest Rate (MAXR): The borrower sets the maximum interest rate above which bids for funding the loan will not be accepted. It is not clear what effect this variable will have on the final rate of a loan. While *MAXR* places a cap on how high the rate can be, higher values could attract more bids, pushing the final interest rate lower.

Loan Amount (LAMT): Loan amount is the natural logarithm of the principal amount of the loan requested. I expect larger loans to have higher interest rates because larger payments should increase the risk of default. Larger loans should also attract more bids, because, holding bid size equal, it takes more bids to fund the loan.

Credit Grade (CRDG): The credit grade assigned to the borrower by Prosper is based on the borrower's credit score (see footnote 7). Grades range from AA (best) to HR or High Risk (worst). For the purposes of analysis, grades are assigned an integer value ranging from 1 to 7, with 1 reflecting the best credit grade (AA). I expect interest rates to be increasing with the credit grade.[12]

Homeownership (HOME): This variable indicates whether the borrower is a homeowner, as indicated by an active mortgage loan on the borrower's credit report. The effect of homeownership on the funded interest rate is not clear. Homeowners may be perceived as being more financially stable and may have greater access to other funding by using their house as collateral. Alternatively, the greater expense and inflexibility associated with a mortgage payment may increase risk. This variable is an indicator variable taking value of 1 if homeownership has been verified, and 0 otherwise.

Debt-to-Income Ratio (DTI): The debt-to-income ratio is the borrower's nonhousing debt payments, including both principal and interest components as taken from their credit report, divided by self-reported income. I expect interest rates to be increasing in DTI.

Bond Yield (BOND): This variable is the two-year Treasury bond yield on the date the listing was posted.[13] Listings fund quickly, usually within seven days. Even though all Prosper loans have three-year maturities, I use the two-year bond as there are no prepayment penalties, so I expect some loans to be paid back early. I expect interest rates charged on loans to increase with bond yields, as the opportunity cost of lending is greater. Additionally, bond yields are generally increasing with expected inflation, so higher bond yields are also a proxy for inflation risk, which should increase the funded interest rate.

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[12] Conclusions from the analysis are unchanged if I use an array of indicator variables for credit score in place of the index variable *CRDG*.

[13] Taken from http://www.federalreserve.gov/releases/h15/data.htm

Group Membership (GROUP): Groups on Prosper are collections of people with a common interest, including professional, ethnic, religious, geographic, or athletic. The concept is that a group whose members consistently make on-time loan payments will develop a reputation, allowing the group's members to attract more bids and lower interest rates to their loan listings. Likewise, the social pressure of maintaining the group's reputation could reduce the likelihood of default for borrowers in the group. However, groups do not guarantee the loans of their members and members are in no way jointly liable for the loans of other group members. Several contemporaneous working papers show membership in a group of affiliated members on Prosper may affect the success of loan listings (Freedman and Jin 2008; Lin et al. 2009). Therefore, I include a control variable indicating group membership.

Prior Loan Performance (PR_G and PR_B): During my sample period, about 7 percent of borrowers had obtained loans previously through Prosper. Because prior loans are visible to lenders, I include control variables indicating the presence and performance of a prior loan. I expect that having either a loan on which all payments are current or having a fully paid prior loan (*PR_G*) to decrease the interest rate on a listing, while having a loan in default or with late payments (*PR_B*) should increase the interest rate on a listing.

Panel A of Table 2 reports summary statistics for all regression variables. *DSCORE* has a median of 4 of a possible of 9 for both funded and unfunded listings. The mean of *DSCORE* is slightly higher for funded loans at 3.95, compared to 3.70 for unfunded listings. Panel B of Table 2 gives the correlations between all regression variables.[14] For funded loans, the Pearson correlation of 0.203 between *RATE* and *DSCORE* is statistically significant, reflecting a positive relation between *RATE* and *DSCORE* before controlling for any other variables. Although this positive association is contrary to the hypothesis that the interest rate charged will be decreasing in disclosures, *DSCORE* is also strongly correlated with *CRDG*, the borrower's credit score, which may explain this result, as *CRDG* has a strong positive correlation with *RATE*. Indeed, the partial correlation between *RATE* and *DSCORE*, holding *CRDG* constant, is significantly negative at −0.084. The positive correlation between *DSCORE* and *CRDG* is consistent with the argument in Section II that the descriptive portion of the loan listing is relatively more important for borrowers with relatively poor credit.

Model

To test the effect of unverifiable disclosures on the interest rate at which a loan is funded (H1), I use a Tobit regression of the interest rate at which a loan listing is funded on the disclosure score and control variables. If lenders perceive the unverifiable disclosures in the loan listings as credible, then I expect to see the interest rate decreasing in the disclosure score. For unfunded loans, the interest rate limit is set at 35 percent, the maximum allowable rate. This assumes all loans would be funded at some interest rate if rates were allowed to exceed 35 percent. As noted in Section III, the sample has 500 funded loans (uncensored) and 500 unfunded loans (right-censored at $RATE \geq 0.35$). As previously described, the control variables used are auto-funding, maximum interest rate, loan amount, credit grade, homeownership, DTI, two-year Treasury bond yield, group membership, and prior loan performance. That is:

$$RATE_i = f(\beta_0, \beta_1 AUTOF_i, \beta_2 MAXR_i, \beta_3 LAMT_i, \beta_4 CRDG_i, \beta_5 HOME_i, \beta_6 DTI_i, \beta_7 BOND_i,$$

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[14] All Variance Inflation Factors (VIFs) associated with the independent variables used in subsequent models are less than 2, indicating multicollinearity is unlikely to pose a problem (Menard 1995; Kennedy 2008).

TABLE 2

Regression Variables

Panel A: Summary Statistics

Variable[a]	Obs.	Mean	Std. Dev.	Median	Min.	Max.
Unfunded Listings						
RATE	NA	NA	NA	NA	NA	NA
BIDS	500	0.54	1.52	0.00	0.00	13.00
AUTOF	500	0.26	0.44	0.00	0.00	1.00
MAXR	500	0.21	0.09	0.20	0.04	0.35
LAMT	500	8.67	0.78	8.61	6.91	10.13
CRDG	500	5.58	1.61	6.00	1.00	7.00
HOME	500	0.30	0.46	0.00	0.00	1.00
DTI	500	0.69	1.44	0.32	0.02	10.01
BOND	500	3.40	1.14	3.17	1.43	5.08
GROUP	500	0.19	0.39	0.00	0.00	1.00
PR_G	500	0.03	0.17	0.00	0.00	1.00
PR_B	500	0.02	0.14	0.00	0.00	1.00
DEBT	500	0.45	0.50	0.00	0.00	1.00
DEFAULT	NA	NA	NA	NA	NA	NA
DSCORE	500	3.70	1.60	4.00	0.00	8.00
Funded Listings						
RATE	500	0.18	0.08	0.16	0.04	0.35
BIDS	500	22.09	21.64	16.00	0.00	134.00
AUTOF	500	0.21	0.41	0.00	0.00	1.00
MAXR	500	0.21	0.08	0.20	0.04	0.35
LAMT	500	8.44	0.90	8.52	6.91	10.13
CRDG	500	3.66	1.79	4.00	1.00	7.00
HOME	500	0.46	0.50	0.00	0.00	1.00
DTI	500	0.37	0.95	0.23	0.01	10.01
BOND	500	3.25	1.14	2.89	1.35	5.08
GROUP	500	0.29	0.46	0.00	0.00	1.00
PR_G	500	0.10	0.30	0.00	0.00	1.00

(continued on next page)

TABLE 2 (continued)

Variable[a]	Obs.	Mean	Std. Dev.	Median	Min.	Max.
PR_B	500	0.02	0.15	0.00	0.00	1.00
DEBT	500	0.45	0.50	0.00	0.00	1.00
DEFAULT	500	0.34	0.47	0.00	0.00	1.00
DSCORE	500	3.95	1.72	4.00	0.00	9.00

Panel B: Correlations[b]

	RATE	*BIDS*	*AUTOF*	*MAXR*	*LAMT*	*CRDG*	*HOME*	*DTI*	*BOND*	*GROUP*	*PR_G*	*PR_B*	*DEBT*	*DEFAULT*	*DSCORE*
Unfunded Loans															
RATE	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA
BIDS	NA	1.000	−0.039	**0.114**	**0.165**	**−0.448**	**0.099**	−0.042	−0.004	0.059	0.040	−0.043	−0.061	NA	0.026
AUTOF	NA	−0.032	1.000	**−0.103**	0.027	**0.124**	−0.054	−0.054	**0.341**	0.017	−0.016	−0.018	0.004	NA	0.032
MAXR	NA	**0.086**	**−0.113**	1.000	**−0.102**	**0.154**	−0.030	0.039	**−0.267**	0.056	0.024	0.025	**0.077**	NA	**0.088**
LAMT	NA	**0.173**	0.029	**−0.108**	1.000	**−0.403**	**0.116**	**0.264**	**0.131**	**−0.108**	**−0.081**	−0.051	−0.040	NA	0.006
CRDG	NA	**−0.401**	**0.124**	**0.167**	**−0.411**	1.000	**−0.221**	**−0.174**	**0.122**	0.056	−0.017	0.000	0.064	NA	0.072
HOME	NA	0.048	−0.054	−0.032	**0.117**	**−0.219**	1.000	0.066	−0.036	**−0.075**	−0.005	0.062	**−0.101**	NA	**−0.075**
DTI	NA	−0.032	−0.007	−0.035	**0.078**	−0.062	**0.088**	1.000	−0.034	−0.039	**−0.106**	−0.045	0.044	NA	0.002
BOND	NA	−0.027	**0.358**	**−0.320**	**0.125**	**0.124**	−0.050	**0.114**	1.000	**0.205**	−0.055	**−0.105**	−0.060	NA	0.011
GROUP	NA	**0.079**	0.017	0.050	**−0.118**	0.059	**−0.075**	0.027	**0.223**	1.000	**0.164**	**0.221**	**0.090**	NA	**0.179**
PR_G	NA	0.036	−0.016	0.020	**−0.079**	−0.008	−0.005	−0.053	−0.059	**0.164**	1.000	−0.024	0.066	NA	−0.032
PR_B	NA	−0.041	−0.018	0.023	−0.057	0.002	0.062	−0.038	**−0.110**	**0.221**	−0.024	1.000	0.043	NA	0.034
DEBT	NA	−0.057	0.004	**0.080**	−0.050	0.071	**−0.101**	−0.058	−0.058	**0.090**	0.066	0.043	1.000	NA	**0.249**
DEFAULT	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA
DSCORE	NA	0.051	0.040	**0.100**	0.015	0.061	**−0.078**	−0.070	0.002	**0.166**	−0.021	0.027	**0.273**	NA	1.000
Funded Loans															
RATE	1.000	**−0.349**	**0.383**	**0.890**	**−0.164**	**0.738**	**−0.196**	**0.290**	−0.048	**0.095**	−0.037	**0.175**	0.062	**0.349**	**0.237**
BIDS	**−0.324**	1.000	**−0.462**	**−0.175**	**0.740**	**−0.534**	**0.281**	0.038	−0.018	−0.033	0.012	−0.054	0.031	0.021	−0.020
AUTOF	**0.393**	**−0.324**	1.000	**0.183**	**−0.135**	**0.251**	**−0.086**	0.019	**0.210**	0.012	−0.021	**0.124**	0.023	**0.132**	−0.003
MAXR	**0.882**	**−0.162**	**0.176**	1.000	**−0.158**	**0.682**	**−0.148**	**0.286**	**−0.118**	**0.089**	−0.035	**0.160**	0.051	**0.331**	**0.276**
LAMT	**−0.197**	**0.737**	**−0.121**	**−0.163**	1.000	**−0.462**	**0.285**	**0.173**	0.045	0.010	0.012	0.023	**0.097**	**0.107**	−0.039
CRDG	**0.724**	**−0.495**	**0.251**	**0.690**	**−0.440**	1.000	**−0.330**	**0.092**	0.066	**0.199**	0.006	0.064	**0.105**	**0.215**	**0.356**

(continued on next page)

TABLE 2 (continued)

	RATE	*BIDS*	*AUTOF*	*MAXR*	*LAMT*	*CRDG*	*HOME*	*DTI*	*BOND*	*GROUP*	*PR_G*	*PR_B*	*DEBT*	*DEFAULT*	*DSCORE*
HOME	**−0.192**	**0.290**	**−0.086**	**−0.152**	**0.273**	**−0.329**	1.000	0.072	0.014	−0.028	**0.084**	−0.003	0.022	0.060	**−0.173**
DTI	0.052	0.040	0.002	0.072	**0.099**	−0.001	0.041	1.000	−0.067	**0.115**	−0.027	**0.084**	**0.187**	**0.202**	0.044
BOND	**−0.084**	−0.044	**0.219**	**−0.150**	0.056	0.070	0.005	**0.098**	1.000	**0.367**	**−0.082**	**−0.121**	0.029	0.061	**0.157**
GROUP	**0.082**	−0.068	0.012	**0.078**	0.022	**0.193**	−0.028	**0.077**	**0.389**	1.000	**0.097**	**0.083**	0.064	0.052	**0.253**
PR_G	−0.038	−0.025	−0.021	−0.030	0.011	0.014	**0.084**	0.031	**−0.093**	**0.097**	1.000	−0.049	−0.030	**−0.106**	0.023
PR_B	**0.203**	−0.071	**0.124**	**0.170**	0.016	**0.074**	−0.003	0.010	**−0.125**	**0.083**	−0.049	1.000	0.055	**0.182**	−0.026
DEBT	0.053	−0.011	0.023	0.044	**0.101**	**0.115**	0.022	0.005	0.028	0.064	−0.030	0.055	1.000	−0.002	**0.185**
DEFAULT	**0.331**	0.048	**0.132**	**0.320**	**0.114**	**0.222**	0.060	**0.100**	0.070	0.052	**−0.106**	**0.182**	−0.002	1.000	−0.059
DSCORE	**0.203**	−0.037	−0.011	**0.259**	−0.012	**0.355**	**−0.158**	−0.023	**0.163**	**0.247**	0.025	−0.027	**0.195**	−0.058	1.000

Bold indicates significance at the 10 percent level or better.

[a] *RATE* is the interest rate at which the loan funded. *BIDS* is the number of bids on the loan divided by the number of days the loan listing was open, rounded to the nearest integer. *AUTOF* is an indicator variable for auto-funding. *MAXR* is the maximum interest rate that the borrower will accept for his loan listing. *LAMT* is the natural log of loan principal amount. *CRDG* is borrower's credit score. *HOME* is an indicator variable for homeownership. *DTI* is borrower's debt-to-income ratio. *BOND* is the two-year Treasury bond yield on loan listing date. *GROUP* is an indicator variable for membership in a Prosper Group. *PR_G* is an indicator variable for having a prior Prosper loan that was paid off. *PR_B* is an indicator variable for having a prior Prosper loan that defaulted. *DEBT* is an indicator variable for the borrower claiming that the purpose of the loan is debt consolidation/refinancing. *DEFAULT* is an indicator variable for the loan resulting from the listing eventually defaulting. *DSCORE* is the borrower's disclosure score (see Table 1).

[b] Pearson correlations are below the diagonal, Spearman correlations are above.

$$\beta_8 GROUP_i, \beta_9 PR\_G_i, \beta_{10} PR\_B_i, \beta_{11} DSCORE_i) + \varepsilon_i \qquad (1)$$

where:

$RATE$ = interest rate at which the loan was funded;
$AUTOF$ = indicator variable, taking value of 1 if loan used auto-funding, 0 otherwise;
$MAXR$ = maximum interest rate the borrower will accept for his or her loan listing;
$LAMT$ = natural logarithm of principal amount of the loan;
$CRDG$ = borrower's credit score; takes integer values 1 through 7, with 1 being best score (or AA, see footnote 7);
$HOME$ = indicator variable, taking value of 1 if borrower is homeowner, 0 otherwise;
DTI = borrower's debt-to-income ratio;
$BOND$ = yield on two-year Treasury bond on date loan listing was posted;
$GROUP$ = indicator variable, taking value of 1 if borrower is a member of a Prosper group, 0 otherwise;
PR_G = indicator variable, taking value of 1 if the borrower has a prior loan that has been paid off or is current at the time of the listing, 0 otherwise;
PR_B = indicator variable, taking value of 1 if the borrower has a prior loan that defaulted or has late payments at the time of the listing, 0 otherwise; and
$DSCORE$ = borrower's disclosure score (see Table 1).

The coefficient of primary interest is β_{11}. Alternatively, I estimate this model using each level of disclosure, rather than the index variable *DSCORE*.

I use a Poisson regression to test the effect of unverifiable disclosures on the number of bids a loan listing receives (H2). For this model, the dependent variable is the number of bids per day a listing receives (*BIDS*). Formally, I define *BIDS* as the number of bids on a loan divided by the number of days the loan listing was open, rounded to the nearest integer. If the lenders perceive the additional information provided in the unverifiable disclosures represented in *DSCORE* as credible, I expect to see *BIDS* increasing in *DSCORE*, as the lender's assessment of the loan listing is improved. Note that *BIDS* is not censored for unfunded loans. I use a Poisson regression due to the count nature of the *BIDS* variable.[15] Thus, the second model is:

$$BIDS_i = g(\beta_0, \beta_1 AUTOF_i, \beta_2 MAXR_i, \beta_3 LAMT_i, \beta_4 CRDG_i, \beta_5 HOME_i, \beta_6 DTI_i, \beta_7 BOND_i, \\ \beta_8 GROUP_i, \beta_9 PR\_G_i, \beta_{10} PR\_B_i, \beta_{11} DSCORE_i) + \varepsilon_i \qquad (2)$$

Control variables are as defined in Model (1). Again, the coefficient of primary interest is β_{11}. I also estimate this model using each level of disclosure, rather than the index variable *DSCORE*.

To test H3a and H3b, I define a new variable, *POOR*, as an indicator variable taking a value of 1 if *CRDG* is greater than or equal to 5.[16] Thus, *POOR* is an indicator for loan listings where the borrower has relatively poor credit. I then re-estimate Models (1) and (2) adding *POOR* and the interaction of *POOR* with all other independent variables. The coefficient of primary interest is that on the interaction *DSCORE***POOR*.

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[15] Results are robust to using a zero-inflated Poisson or negative binomial regression.

[16] I partition around $CRDG = 5$, as 5 is the median value of this variable. Defining *POOR* to indicate $CRDG \geq 5$ creates the most equal partitions. However, inferences from tests are unchanged if I define *POOR* to indicate $CRDG \geq 4$. Inferences are again unchanged if I define *POOR* to indicate $CRDG \geq 6$ in the Poisson models; however, the interaction term $DSCORE * POOR$ becomes statistically insignificant in the Tobit models under this definition.

TABLE 3

Tobit Regression Results

Variable[a]	(1)	(2)	(3)
Constant	−0.4206***	−0.4075***	−0.4122***
	(0.0499)	(0.0494)	(0.0495)
AUTOF	0.0206+	0.0180+	0.0183+
	(0.0106)	(0.0105)	(0.0105)
MAXR	−0.2258***	−0.1857**	−0.1914**
	(0.0626)	(0.0623)	(0.0624)
LAMT	0.0611***	0.0629***	0.0639***
	(0.0053)	(0.0053)	(0.0053)
CRDG	0.0689***	0.0704***	0.0710***
	(0.0032)	(0.0032)	(0.0032)
HOME	−0.0062	−0.0095	−0.0090
	(0.0087)	(0.0087)	(0.0087)
DTI	0.0176***	0.0165***	0.0164***
	(0.0041)	(0.0041)	(0.0041)
BOND	−0.0135**	−0.0118**	−0.0122**
	(0.0044)	(0.0043)	(0.0043)
GROUP	−0.0419***	−0.0330**	−0.0324**
	(0.0102)	(0.0103)	(0.0102)
PR_G	−0.0531***	−0.0518***	−0.0524***
	(0.0154)	(0.0152)	(0.0151)
PR_B	0.0373	0.0308	0.0326
	(0.0287)	(0.0284)	(0.0283)
DEBT			−0.0168*
			(0.0083)
DSCORE		−0.0127***	−0.0116***
		(0.0026)	(0.0026)
Model Chi-Squared	702.26***	726.60***	730.68***
Number of Obs.	1,000	1,000	1,000

+, *, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively (for two-tailed tests).

Tobit regression of interest rate at which loan was funded on disclosure score and control variables. Interest rate limit for unfunded loans is set at 35 percent, the maximum allowable rate. The sample has 500 funded loans (uncensored) and 500 unfunded loans (right-censored at $r \geq 0.35$). Standard errors in parentheses.

Dependent variable is *RATE*.

[a] *RATE* is the interest rate at which the loan funded. *AUTOF* is an indicator variable for auto-funding. *MAXR* is the maximum interest rate that the borrower will accept for his loan listing. *LAMT* is the natural log of loan principal amount. *CRDG* is borrower's credit score. *HOME* is an indicator variable for homeownership. *DTI* is borrower's debt-to-income ratio. *BOND* is the two-year Treasury bond yield on loan listing date. *GROUP* is an indicator variable for membership in a Prosper Group. *PR_G* is an indicator variable for having a prior Prosper loan that was paid off. *PR_B* is an indicator variable for having a prior Prosper loan that defaulted. *DEBT* is an indicator variable for the borrower claiming that the purpose of the loan is debt consolidation/refinancing. *DSCORE* is the borrower's disclosure score (see Table 1).

V. RESULTS

Table 3 presents the results for Model (1). The sign on the *DSCORE* coefficient is negative and statistically significant, consistent with H1. This suggests that the voluntary, unverifiable disclosures in a loan listing do affect the loan's interest rate. The coefficient on *DSCORE* of

−0.0127 indicates each additional disclosure is associated with a reduction in the loan's interest rate of 1.27 percentage points. Thus, all else equal, a loan listing with a *DSCORE* at the 75th percentile (*DSCORE* of 5) could expect to obtain an interest rate 2.54 percentage points lower than a loan listing with a *DSCORE* at the 25th percentile (*DSCORE* of 3).

The signs on the coefficients of the control variables are largely as expected. All but *HOME* and *PR_B* are statistically significant. The lack of significance on *PR_B* is surprising. While the positive and significant coefficient on *PR_G* indicates that lenders give lower rates to borrowers with good payment histories on prior loans, the lack of significance on *PR_B* suggests lenders do not punish borrowers for late payments or defaults on prior loans. Lenders may choose to ignore this negative information in the listing, or it may be subsumed by the other credit information in *CRDG*. Alternatively, lenders may believe that all borrowers on Prosper are likely to have poor payment histories, so they do not update their beliefs after observing late payments. As expected, the coefficient on *DEBT* is negative, indicating borrowers who state the purpose of their loan is debt consolidation receive a significantly lower rate than other borrowers. This is again consistent with unverifiable disclosures in the loan listings affecting lender behavior. The negative and significant coefficient on *BOND* is also surprising. This indicates that lower two-year bond yields are associated with higher loan rates. This may reflect lenders' heightened concern regarding the creditworthiness of borrowers who seek financing through Prosper when bank loan rates are low.

In untabled analysis, I estimate Model (1) using levels of the *DSCORE* variable in the regression, rather than the index variable. Every level of *DSCORE* is negatively associated with interest rates, consistent with the result from Table 3 that interest rates are decreasing with disclosure. While the increase in the magnitude of coefficients as the number of disclosures increases is not monotonic, the general trend is that more disclosures are associated with more negative coefficients. F-tests reveal the change in interest rate for an additional disclosure is relatively constant over different levels of disclosure. However, the decrease in rate for an increase from four to five disclosures is significantly greater than the change in rate for an increase from three to four disclosures. The impact of disclosures on rate is not statistically significant until five disclosures are present in the listing.

I also estimate several variations of Model (1) to demonstrate that the results are robust to alternative ways of controlling for the credit quality of potential borrowers. First, I replace the variable *CRDG*, which takes values of 1 to 7, with each value corresponding to a specific credit grade, with an array of dummy variables for each category of credit grade. Using *CRDG* to control for credit worthiness could be problematic because it equalizes the effect of credit grade on interest rate across every credit category. This could introduce a bias in *DSCORE* if interest rates are increasing in credit grade but at a decreasing rate, as *DSCORE* is also associated with credit grades. However, results are largely unchanged when using dummy variables to control for credit grades in a more general way, with the coefficient on *DSCORE* being −0.0123 and remaining significant at the 0.1 percent level. Second, I add all additional employment and credit report data that are available to lenders, as outlined in footnotes 8 and 9, as controls in the analysis. If these additional credit data are negatively associated with interest rates and positively associated with disclosures, then omitting it would introduce a negative bias to the *DSCORE* coefficient. Again, however, the results are robust to the inclusion of these additional controls, with the coefficient on *DSCORE* being −0.0120 and statistically significant at the 0.1 percent level.

Table 4 presents the results for regression Model (2). The coefficient on *DSCORE* is positive and statistically significant, indicating that more voluntary disclosures increase the bidding activity on a loan, consistent with H2. Again, this suggests that lenders consider the voluntary, unverifiable disclosures in the loan listings in their investment decisions. The coefficient on *DSCORE* of 0.08 in this regression indicates, all else equal, that an additional disclosure in the loan listing is associated with an 8 percent increase in the number of bids on the listing. The coefficient on *DEBT* is insignificantly

TABLE 4

Poisson Regression Results

Variable[a]	(1)	(2)	(3)
Constant	−0.54	−0.58	−0.57
	(0.380)	(0.376)	(0.381)
AUTOF	−0.71***	−0.69***	−0.68***
	(0.121)	(0.122)	(0.122)
MAXR	5.32***	5.06***	5.12***
	(0.544)	(0.550)	(0.551)
LAMT	0.40***	0.38***	0.38***
	(0.042)	(0.043)	(0.044)
CRDG	−0.51***	−0.52***	−0.53***
	(0.025)	(0.025)	(0.025)
HOME	0.21**	0.23**	0.22**
	(0.079)	(0.079)	(0.079)
DTI	−0.18+	−0.18+	−0.18+
	(0.101)	(0.103)	(0.104)
BOND	0.08*	0.07	0.07+
	(0.040)	(0.041)	(0.041)
GROUP	0.23**	0.17+	0.17+
	(0.087)	(0.090)	(0.090)
PR_G	0.22*	0.19+	0.19+
	(0.102)	(0.100)	(0.102)
PR_B	−0.21	−0.16	−0.17
	(0.270)	(0.264)	(0.259)
DEBT			0.07
			(0.071)
DSCORE		0.08***	0.08***
		(0.022)	(0.022)
Model Chi-Squared	1022.63***	1085.00***	1102.28***
Number of Obs.	1,000	1,000	1,000

+, *, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively (for two-tailed tests).

Poisson regression of number of bids per day listing was open on disclosure score and control variables. Robust standard errors in parentheses.

Dependent variable is *BIDS*.

[a] *BIDS* is the number of bids on the loan divided by the number of days the loan listing was open, rounded to the nearest integer. *AUTOF* is an indicator variable for auto-funding. *MAXR* is the maximum interest rate that the borrower will accept for his loan listing. *LAMT* is the natural log of loan principal amount. *CRDG* is borrower's credit score. *HOME* is an indicator variable for homeownership. *DTI* is borrower's debt-to-income ratio. *BOND* is the two-year Treasury bond yield on loan listing date. *GROUP* is an indicator variable for membership in a Prosper Group. *PR_G* is an indicator variable for having a prior Prosper loan that was paid off. *PR_B* is an indicator variable for having a prior Prosper loan that defaulted. *DEBT* is an indicator variable for the borrower claiming that the purpose of the loan is debt consolidation/refinancing. *DSCORE* is the borrower's disclosure score (see Table 1).

different from zero, indicating that, while borrowers stating loan consolidation as the purpose of their loan may receive lower rates, they do not generate more bidding activity on their listings.

As with Model (1), in untabulated results I estimate Model (2) with categories of *DSCORE* rather than the index variable. More extreme disclosure categories lack significance, perhaps due to limited power in these categories (see Table 1, Panel C). Disclosures in the listings do not affect the bidding on a listing until five disclosures are present. While the effect of an additional disclosure is

TABLE 5

Interacted Tobit Regression Results

Variable[a]	(1)		(2)		(3)	
Constant	−0.3452***	(0.0626)	−0.3417***	(0.0617)	−0.3526***	(0.0617)
AUTOF	0.0398*	(0.0167)	0.0382*	(0.0164)	0.0350*	(0.0164)
MAXR	−0.0735	(0.0960)	−0.0611	(0.0947)	−0.0883	(0.0950)
LAMT	0.0532***	(0.0067)	0.0543***	(0.0067)	0.0562***	(0.0067)
CRDG	0.0506***	(0.0059)	0.0521***	(0.0059)	0.0542***	(0.0059)
HOME	−0.0164	(0.0111)	−0.0175	(0.0109)	−0.0150	(0.0109)
DTI	0.0205***	(0.0052)	0.0200***	(0.0051)	0.0200***	(0.0051)
BOND	−0.0092	(0.0061)	−0.0084	(0.0060)	−0.0088	(0.0060)
GROUP	−0.0330*	(0.0154)	−0.0286+	(0.0153)	−0.0285+	(0.0153)
PR_G	−0.0445*	(0.0201)	−0.0441*	(0.0198)	−0.0456*	(0.0197)
PR_B	0.0488	(0.0513)	0.0396	(0.0507)	0.0487	(0.0507)
DEBT					−0.0281*	(0.0112)
DSCORE			−0.0063+	(0.0034)	−0.0048	(0.0034)
POOR	−0.3308**	(0.1211)	−0.2910*	(0.1208)	−0.2751*	(0.1204)
*AUTOF * POOR*	−0.0296	(0.0218)	−0.0329	(0.0216)	−0.0301	(0.0216)
*MAXR * POOR*	−0.2000	(0.1310)	−0.1453	(0.1302)	−0.1164	(0.1300)
*LAMT * POOR*	0.0259*	(0.0115)	0.0268*	(0.0115)	0.0244*	(0.0115)
*CRDG * POOR*	0.0464***	(0.0102)	0.0445***	(0.0102)	0.0419***	(0.0102)
*HOME * POOR*	0.0304	(0.0185)	0.0242	(0.0183)	0.0219	(0.0183)
*DTI * POOR*	−0.0121	(0.0082)	−0.0147+	(0.0083)	−0.0147+	(0.0082)
*BOND * POOR*	−0.0152+	(0.0090)	−0.0125	(0.0089)	−0.0119	(0.0089)
*GROUP * POOR*	−0.0160	(0.0207)	−0.0076	(0.0208)	−0.0078	(0.0207)
*PR_G * POOR*	−0.0089	(0.0317)	−0.0074	(0.0313)	−0.0054	(0.0312)
*PR_B * POOR*	−0.0193	(0.0623)	−0.0123	(0.0617)	−0.0216	(0.0616)
*DEBT * POOR*					0.0321+	(0.0170)
*DSCORE * POOR*			−0.0139**	(0.0053)	−0.0157**	(0.0054)
Model Chi-Squared	733.81***		761.81***		768.23***	
Number. of Obs.	1,000		1,000		1,000	

+, *, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively (for two-tailed tests).

Tobit regression of interest rate at which loan was funded on disclosure score and control variables. Interest rate limit for unfunded loans set at 35 percent, the maximum allowable rate. The sample has 500 funded loans (uncensored) and 500 unfunded loans (right-censored at $r \geq 0.35$). Standard errors in parentheses.

Dependent variable is *RATE*.

[a] *RATE* is the interest rate at which the loan funded. *AUTOF* is an indicator variable for auto-funding. *MAXR* is the maximum interest rate that the borrower will accept for his loan listing. *LAMT* is the natural log of loan principal amount. *CRDG* is borrower's credit score. *HOME* is an indicator variable for homeownership. *DTI* is borrower's debt-to-income ratio. *BOND* is the two-year Treasurv bond yield on loan listing date. *GROUP* is an indicator variable for membership in a Prosper Group. *PR_G* is an indicator variable for having a prior Prosper loan that was paid off. *PR_B* is an indicator variable for having a prior Prosper loan that defaulted. *DEBT* is an indicator variable for the borrower claiming that the purpose of the loan is debt consolidation/refinancing. *DSCORE* is the borrower's disclosure score (see Table 1). *POOR* is an indicator variable for *CRDG* ≥ 5.

similar across most levels of disclosure, the effect of an increase from both three to four disclosures and four to five disclosures is significantly different from the effect of the previous increase.

Finally, I also estimate Model (2) including additional credit worthiness controls. As with Model (1), all inferences remain unchanged in the presence of these additional controls. The coefficient on *DSCORE* is 0.07 when controlling for credit grade with an array of dummy variables

TABLE 6

Interacted Poisson Regression Results

Variable[a]	(1)		(2)		(3)	
Constant	−1.3921**	(0.4288)	−1.4097***	(0.4234)	−1.4036**	(0.4277)
AUTOF	−0.7476***	(0.1554)	−0.7348***	(0.1569)	−0.7306***	(0.1569)
MAXR	3.9034***	(0.6779)	3.7520***	(0.6789)	3.7853***	(0.6825)
LAMT	0.4895***	(0.0483)	0.4807***	(0.0485)	0.4796***	(0.0492)
CRDG	−0.3556***	(0.0426)	−0.3631***	(0.0420)	−0.3658***	(0.0425)
HOME	0.2617**	(0.0859)	0.2706**	(0.0858)	0.2667**	(0.0860)
DTI	−0.1684	(0.1124)	−0.1667	(0.1129)	−0.1670	(0.1133)
BOND	0.0537	(0.0456)	0.0431	(0.0465)	0.0431	(0.0463)
GROUP	0.1319	(0.1029)	0.0995	(0.1038)	0.0996	(0.1036)
PR_G	0.1291	(0.0980)	0.1109	(0.0933)	0.1109	(0.0939)
PR_B	−0.3153	(0.3718)	−0.2442	(0.3806)	−0.2499	(0.3755)
DEBT					0.0274	(0.0779)
DSCORE			0.0491*	(0.0225)	0.0477*	(0.0223)
POOR	5.9371***	(1.2856)	5.4611***	(1.2675)	5.5955***	(1.2566)
AUTOF * *POOR*	0.0126	(0.2147)	0.0808	(0.2206)	0.0632	(0.2226)
MAXR * *POOR*	3.1874*	(1.4573)	2.7812+	(1.4294)	2.7575+	(1.4269)
LAMT * *POOR*	−0.5096***	(0.1226)	−0.5340***	(0.1196)	−0.5514***	(0.1164)
CRDG * *POOR*	−0.7047***	(0.1101)	−0.6885***	(0.1039)	−0.6940***	(0.1045)
HOME * *POOR*	−0.3425+	(0.1971)	−0.2190	(0.1957)	−0.1934	(0.1996)
DTI * *POOR*	0.0006	(0.2082)	0.0616	(0.2032)	0.0586	(0.2084)
BOND * *POOR*	0.2722**	(0.1022)	0.2251*	(0.1010)	0.2301*	(0.1010)
GROUP * *POOR*	0.3949*	(0.1817)	0.2933	(0.1827)	0.2924	(0.1827)
PR_G * *POOR*	0.4491+	(0.2348)	0.4246+	(0.2399)	0.4399+	(0.2360)
PR_B * *POOR*	0.2946	(0.5194)	0.1620	(0.4933)	0.1536	(0.4851)
DEBT * *POOR*					0.1132	(0.1743)
DSCORE * *POOR*			0.1821**	(0.0596)	0.1757**	(0.0609)
Model Chi-Squared	1083.08***		1151.07***		1163.56***	
Number of Obs.	1,000		1,000		1,000	

+, *, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively (for two-tailed tests).

Poisson regression of number of bids per day listing was open on disclosure score and control variables. Robust standard errors in parentheses.

Dependent variable is *BIDS*.

[a] *BIDS* is the number of bids on the loan divided by the number of days the loan listing was open, rounded to the nearest integer. *AUTOF* is an indicator variable for auto-funding. *MAXR* is the maximum interest rate that the borrower will accept for his loan listing. *LAMT* is the natural log of loan principal amount. *CRDG* is borrower's credit score. *HOME* is an indicator variable for homeownership. *DTI* is borrower's debt-to-income ratio. *BOND* is the two-year Treasury bond yield on loan listing date. *GROUP* is an indicator variable for membership in a Prosper Group. *PR_G* is an indicator variable for having a prior Prosper loan that was paid off. *PR_B* is an indicator variable for having a prior Prosper loan that defaulted. *DEBT* is an indicator variable for the borrower claiming that the purpose of the loan is debt consolidation/refinancing. *DSCORE* is the borrower's disclosure score (see Table 1). *POOR* is an indicator variable for $CRDG \geq 5$.

and 0.08 when including all additional creditworthiness controls, outlined in footnotes 8 and 9. Both are statistically significant at the 0.1 percent level. In addition, all results from subsequent analysis in this paper are robust to including these additional control variables. For parsimony, I only discuss results using *CRDG* as the credit quality control in subsequent analysis.

The results from the models testing H3a and H3b are presented in Table 5 and Table 6, respectively. In Table 5 the coefficient on the interaction *DSCORE* $*$ *POOR* of –0.0139 is statistically significant, indicating that an additional disclosure reduces a listing's interest rate by 1.39 percentage points more for borrowers with relatively poor credit. This is consistent with H3a. Table 6, consistent with H3b, shows that the coefficient on the interaction *DSCORE*$*$*POOR* is 0.1821, which is statistically significant. This indicates an additional disclosure increases bidding activity by 18.21 percent more for borrowers with relatively poor credit. Collectively, these results indicate that unverifiable disclosures are relatively more important for borrowers with relatively poor credit.

VI. ALTERNATIVE SPECIFICATIONS

In this section, I test the robustness of my results. First, I use a two-step model, rather than a Tobit model, to analyze separately whether a loan is funded and, if it is funded, which interest rate it receives. Next, I allow for the possibility that certain components of my disclosure measure may be correlated with information that is verified elsewhere in the loan listing. I separate from my disclosure measure those components that in limited instances may be verified in other sections of the loan listing to ensure I am measuring only the effect of truly unverifiable items in influencing lender behavior.

Table 7 shows the results of the two-step model. First, I estimate a probit model, regressing an indicator for a listing successfully funding on *DSCORE* and control variables. The results show that, for the average listing, an additional unverifiable disclosure increases the likelihood of funding by 3.28 percent. Next, for the subsample of successfully funded loans, I regress the actual interest rate on *DSCORE* and control variables. This shows that, given a loan is funded, an additional disclosure decreases the interest rate by 0.22 percentage points. Finally, I estimate a Heckman two-step model, where the inverse-Mills ratio from the funding equation is added to the rate regression to control for selection effects. I omit *GROUP* from the rate regression in the Heckman model to avoid identification only through nonlinearities. I use this as the identifying variable because group membership, while having a large effect on whether the loan receives enough bids to fund (as seen in the selection model), may have little direct impact on the rate of the loan.[17] Results from the Heckman model show that an additional disclosure decreases the interest rate on a loan by 0.31 percentage points.

Table 8 shows the results of Models (1) and (2) with the *DSCORE* disclosure index replaced with an array of indicator variables for each specific disclosure included in the index. Many individual disclosures do not have a statistically significant effect when considered in isolation. *Purpose*, *Other Debt Rate*, and *Poor Credit Expl* appear the most important in determining interest rates, while *Purpose, Other Debt*, and *Poor Credit Expl* have the greatest effect on bidding activity.

In limited instances Prosper may verify other borrower-provided information in the loan listing, such as income, employment, or occupation. While the information I code in developing *DSCORE* comes only from the descriptive portion of the loan listing, which is not subject to verification, this information is likely to be consistent with the information provided by the borrower elsewhere in the listing. Thus, these disclosures could be considered at least partially verified. Therefore, to ensure that lenders' responses to *DSCORE* are reflecting reliance on unverifiable information, I

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[17] The fact that I am able to control for all information in a loan listing available to potential lenders mitigates the concern of selection bias. Identifying an exogenous instrument is difficult in this setting, as only information potentially relevant in the lending decision is likely to be included in the loan listing. Note results from the Heckman model are robust to not excluding a variable in the second stage, which may be a preferred specification when reasonable exclusion restrictions do not exist (Li and Prabhala 2007).

TABLE 7

Two-Step and Heckman Selection Models

Variable[b]	Model: Probit *FUNDED*[a]	Model: OLS *RATE*[a]	Model: Heckman *RATE*[a]
Constant		−0.0237	−0.0532*
		(0.0148)	(0.0255)
AUTOF	0.0495+	0.0430***	0.0417***
	(0.0299)	(0.0030)	(0.0037)
MAXR	1.4486***	0.6793***	0.6365***
	(0.1691)	(0.0346)	(0.0389)
LAMT	−0.1915***	0.0039*	0.0083*
	(0.0150)	(0.0018)	(0.0035)
CRDG	−0.1734***	0.0096***	0.0140***
	(0.0052)	(0.0015)	(0.0032)
HOME	0.0344	−0.0020	−0.0027
	(0.0254)	(0.0030)	(0.0030)
DTI	−0.0372***	−0.0000	0.0013
	(0.0106)	(0.0013)	(0.0017)
BOND	0.0335**	−0.0026+	−0.0036*
	(0.0128)	(0.0013)	(0.0016)
GROUP	0.0971**	0.0011	
	(0.0304)	(0.0034)	
PR_G	0.1682***	−0.0042	−0.0080
	(0.0504)	(0.0044)	(0.0054)
PR_B	−0.0140	0.0173	0.0172+
	(0.0760)	(0.0106)	(0.0096)
DSCORE	0.0328***	−0.0022**	−0.0031**
	(0.0075)	(0.0008)	(0.0011)
MILLS			−0.0143
			(0.0097)
Model Chi-Squared	549.93***		2580.61***
Adj. R^2		0.85	
Number of Obs.	1,000	500	1,000

+, *, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively (for two-tailed tests).

Average marginal effects reported for probit regression. Standard errors in parentheses.

[a] Dependent variables: *FUNDED* is an indicator for the loan listing resulting in a loan. *RATE* is the interest rate at which the loan funded.

[b] *AUTOF* is an indicator variable for auto-funding. *MAXR* is the maximum interest rate that the borrower will accept for his loan listing. *LAMT* is the natural log of loan principal amount. *CRDG* is borrower's credit score. *HOME* is an indicator variable for homeownership. *DTI* is borrower's debt-to-income ratio. *BOND* is the two-year Treasury bond yield on loan listing date. *GROUP* is an indicator variable for membership in a Prosper Group. *PR_G* is an indicator variable for having a prior Prosper loan that was paid off. *PR_B* is an indicator variable for having a prior Prosper loan that defaulted. *DSCORE* is the borrower's disclosure score (see Table 1). *MILLS* is the inverse Mills ratio.

partition *DSCORE* into two measures. *D_VER* includes disclosures that may be verified in other parts of the listing in limited instances (income, income source, other debt, and interest rate on other debt), while *D_UNV* includes disclosures that are most likely to be unverifiable in all cases (purpose, education, poor credit explanation, expenses, and picture). Table 8 shows that in both determining the interest rate and the number of bids on a loan, *D_UNV* is greater than *D_VER* in

TABLE 8

DSCORE Component Analysis

	Tobit Regression Dependent Variable is *RATE*			**Poisson Regression Dependent Variable is *BIDS***		
Variable[a]	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Constant	−0.4075***	−0.4021***	−0.4069***	−0.5840	−0.8989*	−0.6792+
	(0.0494)	(0.0510)	(0.0497)	(0.3762)	(0.4037)	(0.3749)
AUTOF	0.0180+	0.0181+	0.0180+	−0.6908***	−0.6786***	−0.6928***
	(0.0105)	(0.0105)	(0.0105)	(0.1219)	(0.1210)	(0.1227)
MAXR	−0.1857**	−0.1946**	−0.1857**	5.0611***	5.3902***	5.1061***
	(0.0623)	(0.0630)	(0.0623)	(0.5502)	(0.5411)	(0.5476)
LAMT	0.0629***	0.0628***	0.0629***	0.3813***	0.3722***	0.3846***
	(0.0053)	(0.0053)	(0.0053)	(0.0430)	(0.0420)	(0.0427)
CRDG	0.0704***	0.0701***	0.0704***	−0.5221***	−0.5310***	−0.5280***
	(0.0032)	(0.0033)	(0.0032)	(0.0255)	(0.0267)	(0.0261)
HOME	−0.0095	−0.0081	−0.0096	0.2269**	0.2247**	0.2283**
	(0.0087)	(0.0087)	(0.0087)	(0.0792)	(0.0780)	(0.0780)
DTI	0.0165***	0.0165***	0.0165***	−0.1775+	−0.1754+	−0.1755+
	(0.0041)	(0.0041)	(0.0041)	(0.1029)	(0.1011)	(0.1002)
BOND	−0.0118**	−0.0113**	−0.0118**	0.0665	0.0769+	0.0723+
	(0.0043)	(0.0044)	(0.0043)	(0.0409)	(0.0395)	(0.0410)
GROUP	−0.0330**	−0.0313**	−0.0331**	0.1683+	0.1437	0.1744+
	(0.0103)	(0.0105)	(0.0103)	(0.0896)	(0.0888)	(0.0900)
PR_G	−0.0518***	−0.0513***	−0.0519***	0.1911+	0.2024*	0.1926+
	(0.0152)	(0.0153)	(0.0152)	(0.1002)	(0.1008)	(0.0998)
PR_B	0.0308	0.0362	0.0308	−0.1599	−0.1628	−0.1646
	(0.0284)	(0.0285)	(0.0284)	(0.2643)	(0.2560)	(0.2652)
DSCORE	−0.0127***			0.0800***		
	(0.0026)			(0.0222)		
Purpose		−0.0274+			0.5281**	
		(0.0152)			(0.1884)	
Income		0.0068			−0.1325	
		(0.0143)			(0.1292)	
Inc Source		−0.0102			0.0464	
		(0.0090)			(0.0773)	
Education		−0.0025			0.1229	
		(0.0140)			(0.1099)	
Other Debt		−0.0185			0.2065*	
		(0.0124)			(0.0892)	
Other Debt Rate		−0.0258+			−0.1357	
		(0.0146)			(0.1237)	
Poor Credit Expl		−0.0227*			0.2173*	
		(0.0107)			(0.0945)	
Expenses		−0.0185			0.2062	
		(0.0141)			(0.1316)	
Picture		−0.0099			0.0020	
		(0.0085)			(0.0692)	

(continued on next page)

TABLE 8 (continued)

	Tobit Regression Dependent Variable is *RATE*			Poisson Regression Dependent Variable is *BIDS*		
Variable[a]	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
D_VER			−0.0122*			0.0243
			(0.0050)			(0.0415)
D_UNV			−0.0130**			0.1329**
			(0.0045)			(0.0428)
Model Chi-Squared	726.60***	732.12***	726.61***	1085.00***	1263.39***	1079.23***
Number of Obs.	1,000	1,000	1,000	1,000	1,000	1,000

+, *, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively (for two-tailed tests).

[a] *RATE* is the interest rate at which the loan funded. *BIDS* is the number of bids on the loan divided by the number of days the loan listing was open, rounded to the nearest integer. *AUTOF* is an indicator variable for auto-funding. *MAXR* is the maximum interest rate the borrower will accept for his loan listing. *LAMT* is the natural log of loan principal amount. *CRDG* is borrower's credit score. *HOME* is an indicator variable for homeownership. *DTI* is borrower's debt-to-income ratio. *BOND* is the two-year Treasury bond yield on loan listing date. *GROUP* is an indicator variable for membership in a Prosper Group. *PR_G* is an indicator variable for having a prior Prosper loan that was paid off. *PR_B* is an indicator variable for having a prior Prosper loan that defaulted. *DSCORE* is the borrower's disclosure score (see Table 1). *Purpose–Picture* are individual components of *DSCORE* (see Table 1). *D_VER* is a subset of *DSCORE* (the sum of *Income*, *Inc Source*, *Other Debt*, and *Other Debt Rate*). *D_UNV* is another subset of *DSCORE* (the sum of *Purpose*, *Education*, *Poor Credit Expl*, *Expenses*, and *Picture*).

both magnitude and statistical significance. This offers additional evidence that borrowers respond to truly unverifiable information in loan listings.

VII. LOAN OUTCOME ANALYSIS

Given that unverifiable disclosures in loan listings significantly influence the decisions of lenders, a natural extension is to examine how these disclosures relate to loan performance. To accomplish this, I perform a probit regression of an indicator for eventual loan default on *DSCORE* and control variables. Table 9 shows the results of this model, with average marginal effects reported. This analysis is limited to listings that resulted in loans. The size of the loan strongly predicts future default, as does having a prior loan with poor performance. Of all control variables, only having a prior loan with current payments is significantly associated with a lower risk of future default. Interestingly, rate does not predict default. *DSCORE,* however, has a strong negative association with future defaults. An additional disclosure is associated with a 5.37 percent less chance of future default. Partitioning *DSCORE* into two components, as in Table 8, demonstrates that it is the most unverifiable of the disclosures that are driving this result. Thus, lenders' reliance on unverifiable disclosures in the loan listings appears to be validated in an *ex post* sense.

VIII. CONCLUSION

This study analyzes the impact of voluntary, unverifiable disclosures with a unique dataset of peer-to-peer loans. This setting allows for more powerful tests of unverifiable disclosures than are possible with publicly traded firms. The results indicate that such disclosures influence lenders by both decreasing the interest rate charged and increasing the bidding activity on a loan. Additional analysis shows the effect of these disclosures is stronger for borrowers with relatively poor credit. Further, unverifiable disclosures are negatively associated with future loan defaults.

TABLE 9

Loan Outcome Analysis

Variable[a]	**(1)**	**(2)**	**(3)**	**(4)**
AUTOF	0.0108	0.0175	−0.0076	−0.0137
	(0.0558)	(0.0481)	(0.0547)	(0.0552)
MAXR	0.7458	1.4099***	1.0146+	1.0009+
	(0.5441)	(0.3341)	(0.5322)	(0.5330)
LAMT	0.1104***	0.1270***	0.1247***	0.1243***
	(0.0262)	(0.0254)	(0.0256)	(0.0256)
CRDG	0.0420*	0.0642***	0.0588**	0.0594**
	(0.0192)	(0.0181)	(0.0190)	(0.0190)
HOME	0.0869*	0.0647	0.0663	0.0650
	(0.0411)	(0.0407)	(0.0407)	(0.0407)
DTI	0.0281	0.0209	0.0212	0.0215
	(0.0228)	(0.0221)	(0.0222)	(0.0222)
BOND	0.0378+	0.0477*	0.0490*	0.0477*
	(0.0206)	(0.0201)	(0.0201)	(0.0202)
GROUP	−0.0574	−0.0302	−0.0307	−0.0370
	(0.0488)	(0.0484)	(0.0483)	(0.0487)
PR_G	−0.1610*	−0.1554*	−0.1513*	−0.1519*
	(0.0727)	(0.0712)	(0.0710)	(0.0709)
PR_B	0.3932*	0.3756*	0.3667*	0.3656*
	(0.1607)	(0.1584)	(0.1584)	(0.1574)
RATE	0.8101		0.5668	0.6057
	(0.5986)		(0.5860)	(0.5883)
DSCORE		−0.0537***	−0.0525***	
		(0.0122)	(0.0123)	
D_VER				−0.0356
				(0.0232)
D_UNV				−0.0674**
				(0.0212)
Model Chi-Squared	106.72***	123.07***	124.01***	124.74***
Number of Obs.	500	500	500	500

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*, **, *** $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively (for two-tailed tests).
+ $p < 0.10$ (for two-tailed tests).
Probit regression of an indicator for default on disclosure score and control variables. Average marginal effects reported.
Standard errors in parentheses.
Dependent variable is *DEFAULT*.

[a] *DEFAULT* in an indicator variable for the loan resulting from the successful listing eventually defaulting. *AUTOF* is an indicator variable for auto-funding. *MAXR* is the maximum interest rate that the borrower will accept for his loan listing. *LAMT* is the natural log of loan principal amount. *CRDG* is borrower's credit score. *HOME* is an indicator variable for homeownership. *DTI* is borrower's debt-to-income ratio. *BOND* is the two-year Treasury bond yield on loan listing date. *GROUP* is an indicator variable for membership in a Prosper Group. *PR_G* is an indicator variable for having a prior Prosper loan that was paid off. *PR_B* is an indicator variable for having a prior Prosper loan that defaulted. *RATE* is the interest rate at which the loan funded. *DSCORE* is borrower's disclosure score (see Table 1). *D_VER* is a subset of *DSCORE* (the sum of *Income*, *Inc Source*, *Other Debt*, and *Other Debt Rate*). *D_UNV* is another subset of *DSCORE* (the sum of *Purpose*, *Education*, *Poor Credit Expl*, *Expenses*, and *Picture*).

These results can be interpreted as being consistent with a desire of borrowers to develop a reputation for truth telling in their disclosures so that they can continue to benefit from using these disclosures in future loan listings (Stocken 2000). However, this interpretation requires the possibility of repeated borrowing, which is observed only to a limited degree in the data. The results are also consistent with work in psychology and behavioral economics that shows objectively uninformative material can influence individual choice. Developing an appreciation for how individuals value and react to these types of disclosures is important in understanding and predicting the behavior of market participants.

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Accrual Quality, Realized Returns, and Expected Returns: The Importance of Controlling for Cash Flow Shocks

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ABSTRACT: This paper develops a simple methodology based on the earnings response coefficient framework that allows decomposing realized returns into cash flow shocks and returns excluding cash flow shocks. I find that stocks with poor (good) accrual quality were on average subject to relatively lower (higher) cash flow shocks over the past 37 years. These lower (higher) cash flow shocks offset the higher (lower) expected returns of poor (good) accrual quality firms. After excluding cash flow shocks, future realized returns are negatively associated with accrual quality. The premiums pertaining to accrual quality are both statistically and economically significant in standard asset-pricing tests when cash flow shocks are excluded by firm-specific return decomposition. Overall, this paper provides evidence on the existence of a priced accrual quality risk factor, and underscores the importance of controlling for cash flow shocks in asset-pricing tests that use realized returns.

Keywords: *accounting quality; asset pricing; cash flow shocks; cost of equity.*

I. INTRODUCTION

There is an ongoing debate as to whether the quality of accounting information influences firms' cost of equity capital. Much of the debate centers around the existence of a priced risk factor related to the quality of accruals. In an influential set of papers, Francis et al. (2004, 2005; hereafter, FLOS) argue that accrual quality affects the cost of equity.[1] FLOS base their

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[1] Throughout the paper, I use "cost of equity" and "expected return" interchangeably.

conclusions in part on a significant positive correlation between the returns on *AQ Factor* (the accrual quality factor) and contemporaneous stock returns.[2] However, using conventional two-stage asset-pricing tests (e.g., Fama and MacBeth 1973; hereafter, Fama-MacBeth), Core et al. (2008; hereafter, CGV) show that the loadings on *AQ Factor*, although positive on average, do not explain the cross-sectional variation in returns. In addition, CGV document that accrual quality, as a characteristic, does not predict future (one-year-ahead) realized returns. Taken together, the findings in CGV are inconsistent with accrual quality being priced by the market. Resolving the debate on the existence of a priced risk factor related to accrual quality is important given the large number of studies that implicitly or explicitly rely on the existence of *AQ Factor*.[3]

In this paper, I argue that CGV are unable to find an association between accrual quality and future realized returns because their inverse measure of accrual quality—the residual accrual volatility measure proposed by Dechow and Dichev (2002; hereafter, the *DD Measure*)—is negatively correlated with future cash flow shocks; that is, firms with low (high) accrual quality experience more negative (positive) cash flow shocks in the future.[4] Due to this correlation, the higher (lower) expected returns associated with poor (good) accrual quality firms are systematically offset by negative (positive) cash flow shocks, thereby leading to no association between future realized returns and accrual quality.

Prior research provides evidence that indirectly supports the conjectures above. Both FLOS and CGV use *DD Measure* to measure accrual quality. The *DD Measure* is estimated as the standard deviation of the residuals from a regression of working capital accruals on past, current, and future operating cash flows, such that *higher* levels of *DD Measure* represent *lower* accrual quality. The *DD Measure* is correlated with several characteristics that are likely to be associated with future cash flow news. For example, high *DD Measure* (i.e., low accrual quality) stocks that have higher sales growth in the past (Doyle et al. 2007), are likely to have higher bankruptcy risk due to a high frequency of losses (Dechow and Dichev 2002), and have more volatile earnings and sales (Dechow and Dichev 2002). Each of these characteristics has been linked to systematic future underperformance in the stock market (Lakonishok et al. 1994; Dichev 1998; Mohanram 2005), possibly as a result of adverse cash flow news.

To detect a premium on accrual quality, I perform asset-pricing tests that use a novel measure of expected returns—realized returns exclusive of cash flow shocks. I rely on the earnings response coefficient framework to estimate the cash flow shock portion of realized returns. A cash flow shock is a return arising from a revision in the expectation of the entire stream of future cash flows, which can be measured as the return associated with an earnings surprise. I estimate cash flow shocks as fitted values from regressions of realized returns on earnings surprises. These regressions vary along two dimensions: the assumption of a linear versus nonlinear relation between returns and earnings surprises, and the choice of a cross-sectional versus time-series research design. The linear cross-sectional (firm-specific time-series) return decomposition is expected to be the least (the most) powerful at identifying returns associated with earnings surprises (e.g., Lipe et al. 1998).

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[2] The conclusions in FLOS (2004, 2005) are based on multiple tests that, in addition to *AQ Factor*, employ earnings-to-price ratios, betas, and implied cost-of-equity estimates to proxy for cost of equity. The results based on these alternative proxies are generally consistent with low accrual quality being penalized with higher cost of equity.

[3] For example, studies that use the loadings on *AQ Factor* to proxy for accounting quality include Ecker et al. (2006), Chen et al. (2007), Kravet and Shevlin (2010), Krishnan et al. (2008), Kim and Venkatachalam (2011), Ecker (2009), Bhattacharya et al. (2009), and Chang et al. (2009).

[4] In the finance literature (e.g., Campbell 1991), the term "cash flow shock" describes the return resulting from a revision in expectation of future benefits accruing to equity holders. The earnings response coefficient studies in the accounting literature (e.g., Kormendi and Lipe 1987) use earnings surprises to proxy for such revisions.

I start my empirical analyses by replicating CGV's findings for a sample of 92,145 firm-year observations with returns from April 1971 to March 2008.[5] Unlike CGV, I lag *DD Measure* by one year to prevent the look-ahead bias in asset-pricing tests.[6] First, I confirm CGV's "non-result" for my sample period. In particular, *DD Measure* does not predict future stock returns either at the individual stock level or at the portfolio level. Similarly, stocks with higher loadings on *AQ Factor*, the accrual quality factor based on *DD Measure*, do not earn significantly higher returns at the portfolio level.

Next, I decompose total realized returns into cash flow shocks and returns that exclude cash flow shocks. The results of the decomposition confirm that stocks with poor accrual quality (high *DD Measure*) experience significantly more negative cash flow shocks in the near future. The result is robust to the choice of a time-series or cross-sectional research design and to different nonlinearity assumptions.

Finally, I perform asset-pricing tests using returns that exclude cash flow shocks. As in CGV, the tests use either *DD Measure* or the loading on *AQ Factor* (*AQ Factor* beta) and are performed either at individual stock or portfolio level. The results are mixed. *DD Measure* as a characteristic is, by itself, a significant predictor of future returns excluding cash flow shocks. However, the premium on *DD Measure* (i.e., the regression coefficient on the decile rank of the *DD Measure*) is reduced after controlling for the three Fama-French characteristics of beta, market value of equity, and book-to-market. It remains statistically significant only in the test that relies on the firm-specific time-series return decomposition, the most powerful research design. The premium from the latter test is also economically significant, implying a 3.24 percent difference in annualized returns between stocks in the highest and the lowest *DD Measure* deciles.

The premium on *AQ Factor* (i.e., the regression coefficient on the *AQ Factor* beta) is significant after controlling for the three Fama-French factor loadings when cash flow shocks are excluded using nonlinear cross-sectional or firm-specific time-series return decomposition methods. The statistically significant *AQ Factor* premium estimates range from 3.6 percent to 4.68 percent on the annualized basis.

In summary, I provide evidence suggesting that accrual quality, as measured using the Dechow-Dichev model, is associated with future realized returns after controlling for cash flow shocks. However, the results should be interpreted with caution because they are sensitive to the return decomposition method.

I perform two additional analyses aimed at strengthening the conclusions of the paper. First, I directly estimate unconditional expected returns for each firm from the firm-specific time-series returns-earnings surprise regressions. When the resulting expected returns are used in asset-pricing tests in place of returns excluding cash flow shocks, the premiums on accrual quality remain statistically and economically significant in all characteristic- and factor-loading-based tests. Second, I address the proposed return decomposition's inability to capture the discount rate shocks, i.e., the portion of returns due to changes in the expectation of future discount rates. If such shocks are negatively related to accrual quality, then they represent a correlated omitted variable that biases asset-pricing tests toward finding an accrual quality premium. However, additional tests show that the correlation between accrual quality and proxies for future discount rate shocks is positive, suggesting that discount rate shocks are unlikely to be responsible for the positive and significant premiums on accrual quality.

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[5] This sample has five additional recent years not included in the CGV and FLOS (2005) samples. Including more years following the recent accounting scandals (e.g., Enron) should lend more power to tests that explore the asset-pricing implications of accounting quality.

[6] The look-ahead bias stems from using one-year-ahead cash flow to construct *DD Measure*. The tests are robust to using a non-lagged *DD Measure*, as discussed in Section V.

This paper contributes to the literature in two ways. First, it sheds light on the recent debate as to whether realized returns reflect the risk premium associated with poor accrual quality (FLOS 2005; CGV 2007). Second, it underscores the importance of controlling for cash flow shocks in asset-pricing tests that use realized returns to proxy for cost of equity, and develops a simple methodology that excludes cash flow shocks from the realized returns.

The conclusions of the paper are subject to the following caveats. First, the proposed return decomposition methodology assumes non-stochastic discount rates and relies solely on earnings surprises to capture news about future payoffs to shareholders. Second, the empirical tests in the paper seek to provide evidence on the existence of a premium related to accrual quality. The paper does not suggest a complete multi-factor asset-pricing model that fully explains the cross-section of expected returns. Such an objective would require asset-pricing tests to satisfy additional restrictions (Lewellen et al. 2010).

The rest of the paper proceeds as follows. Section II reviews the related literature, develops the paper's hypotheses, and describes the methodology used to control for cash flow shocks. Section III describes the sample selection procedures and the data. Section IV describes the main empirical analyses and results, Section V reports additional analyses and robustness checks, Section VI discusses methodology limitations, and Section VII presents my conclusions.

II. MOTIVATION

Prior Literature

Analytical Literature

The theoretical foundation for the link between accrual quality and the cost of equity comes from analytical models in the estimation risk and market microstructure literatures.[7] The estimation risk literature (e.g., Klein and Bawa 1976; Barry and Brown 1985; Coles et al. 1995) suggests that securities with less information about past return realizations should earn higher expected returns. Further, Lambert et al. (2007, 2009) suggest that firms with more precise information about future cash flows have lower conditional covariances with the market and, as a consequence, lower conditional betas and lower expected returns. The estimation risk literature suggests the following two-step link: (1) firms with higher information quality have lower forward-looking betas; and (2) lower forward-looking betas lead to lower cost of equity. Note that the traditionally estimated CAPM betas assume no differences in estimation risk across securities and therefore are an imperfect proxy for forward-looking betas.

The market microstructure literature (Amihud and Mendelson 1986) argues that a stock's expected return increases with the magnitude of the bid-ask spread to compensate for higher transaction costs. Higher information quality may decrease the firm's cost of equity by lowering the adverse selection component of the bid-ask spread.[8]

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[7] In this section, I discuss only models that relate the quality of information to the cost of equity under conditions that are directly applicable to asset-pricing tests in the presence of diversification opportunities with multiple firms and multiple traders in the market. A number of models explore the effects of information asymmetry on liquidity but do not have clear implications for expected returns and are directly applicable only to the setting of equity issues, such as IPOs and SEOs, e.g., Diamond and Verrecchia (1991).

[8] In addition, Easley and O'Hara (2004) model the effect of information asymmetry on the cost of equity. In their model, poorly informed investors demand compensation for their informational disadvantage *vis-à-vis* more informed investors. However, two recent papers dispute the conclusions of Easley and O'Hara (2004). Hughes et al. (2007) argue that after controlling for factor betas, there is no cross-sectional relation between expected returns and the degree of information asymmetry, although information asymmetry affects factor premiums. Lambert et al. (2009) show that after controlling for the average precision of information, the degree of information asymmetry is not related to expected returns.

Overall, while analytical models suggest different mechanisms through which information quality affects cost of equity, they imply similar empirical predictions regarding the association between accrual quality and cost of equity. If low accrual quality increases estimation risk or information asymmetry, then low accrual quality stocks should earn higher returns.

The above two streams of research can also be used to derive a risk factor related to accrual quality. For example, Kogan and Wang (2003) show that a model with estimation risk or parameter uncertainty can be recast in a multi-factor asset-pricing framework with a separate factor related to estimation risk, while Easley et al. (2010) argue that there is a separate risk factor related to information asymmetry.

Empirical Literature

Most empirical studies use the Dechow and Dichev (2002) measure of residual accrual volatility (*DD Measure*) to infer the association between accrual quality and cost of equity.[9] The *DD Measure* is estimated as the standard deviation of residuals from a regression of working capital accruals on past, current, and future operating cash flows. Higher *DD Measures* indicate lower accrual quality.

Several studies document a higher cost of equity for firms with low accrual quality (high *DD Measures*) using alternative cost of equity measures, including (1) implied cost of equity (FLOS 2004; CGV 2007); (2) CAPM betas (FLOS 2004; Liu and Wysocki 2007); and (3) industry-adjusted earnings-to-price ratios (FLOS 2004; Liu and Wysocki 2007).[10]

In contrast, the evidence concerning the relation between cost of equity and accrual quality based on realized stock returns is mixed. FLOS (2004, 2005) form a factor-mimicking portfolio *AQ Factor* based on *DD Measure* and document that the return on *AQ Factor* is positively correlated with realized returns on individual stocks. Nichols (2006) documents significant abnormal daily returns on *AQ Factor*. In contrast, Aboody et al. (2005) find that the hedge return earned by taking a long (short) position in the stocks with high (low) *AQ Factor* loadings is not statistically significant. Further, CGV point out that while FLOS (2004, 2005) show that the average loading on *AQ Factor* is positive, to conclude that *AQ Factor* is a priced risk factor also requires establishing that stocks with higher loadings on *AQ Factor* have higher average realized returns. CGV perform standard two-stage asset-pricing tests (Fama and MacBeth 1973) and find that the loadings on *AQ Factor* do not explain the cross-sectional variation in returns. The premium on the *AQ Factor* is neither economically nor statistically significant. They also document that the *DD Measure* characteristic does not predict future monthly realized returns. CGV thus conclude that the significant returns on *AQ Factor* found by Nichols (2006) result from the daily portfolio re-balancing implicitly imposed by using equally weighted daily portfolio returns.

Resolving the debate on the existence of a priced risk factor related to accrual quality is important because a large number of studies implicitly or explicitly rely on its existence, including the following studies that use the loadings on *AQ Factor* to proxy for accounting quality: Ecker et al. (2006), Chen et al. (2007), Kravet and Shevlin (2010), Krishnan et al. (2008), Kim and Venkatachalam (2011), Ecker (2009), Bhattacharya et al. (2009), and Chang et al. (2009).

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[9] Empirically, the association between accounting quality and the cost of equity has been tested using a variety of definitions for accounting quality, including the overall quality of disclosures (e.g., Botosan 1997; Botosan and Plumlee 2002; Berger et al. 2006) and the quality of specific accounting metrics such as earnings (e.g., Barth et al. 2011; Bhattacharya et al. 2003). These studies generally find that firms with low-quality accounting information are penalized with a higher cost of equity. For the sake of brevity, I only review studies using accrual quality, and *DD Measure* in particular, to proxy for the quality of accounting information.

[10] However, Liu and Wysocki (2007) argue that any asset-pricing effects of *DD Measure* are attributable to the underlying operating risk captured by cash flow variability and idiosyncratic return volatility.

To summarize, while analytical models suggest a negative link between information quality and firms' cost of equity, empirical research relating accrual quality, measured using *DD Measure*, to cost of equity (expected returns) provides mixed evidence. Importantly, standard asset-pricing tests provide little evidence that the stock market prices accrual quality risk associated with *DD Measure*.

The *DD Measure* and Cash Flow Shocks

Standard asset-pricing tests use average realized stock returns to capture the cost of equity. However, realized returns measure cost of equity with error due to information shocks (e.g., Elton 1999). Realized returns include three components: the expected return, the return due to cash flow news, and the return due to discount rate news (Campbell 1991). Of these three components, only the expected return reflects the firm's cost of equity. Conventional asset-pricing tests that use average realized stock returns to proxy for expected returns implicitly assume that realized stock returns on average are equal to expected returns. This assumption need not hold *ex post* in finite data samples. For example, Elton (1999) finds that historical realized returns deviate from expected returns for extended periods of time.[11] If firms with lower (higher) levels of accrual quality systematically received more (less) negative cash flow news during the sample period, then asset-pricing tests that use average realized returns as a proxy for expected returns are biased against finding a positive relationship between accrual quality and the cost of equity. Such bias is present in the risk premium estimates generated by all types of cross-sectional asset-pricing tests, independent of whether the analysis is at the stock or portfolio level or whether the basis of testing is characteristics or factor loadings. The magnitude of the bias is proportional to the correlation between accrual quality and future cash flow shocks.[12] A sufficiently large negative correlation may produce insignificant risk premium estimates.

The evidence in prior empirical research is indirectly consistent with firms with higher *DD Measure* (lower accrual quality) experiencing more negative cash flow shocks in the near future. Several characteristics that are correlated with *DD Measure* are also related to future abnormal stock returns, possibly due to cash flow shocks. For example, Dechow and Dichev (2002) document that firms with higher *DD Measure* have a higher frequency of losses, which is consistent with financial distress. Distressed firms earn significantly lower returns compared to the rest of the stock market (Dichev 1998), possibly due to systematic future negative cash flow shocks.

Firms with higher *DD Measure* also have more variable earnings and sales (Dechow and Dichev 2002). Mohanram (2005) shows that growth firms (with low book-to-market ratios) with more variable earnings or sales growth earn significantly lower future returns compared to their industry peers. He explains this effect by a naïve extrapolation of good past earnings or sales performance that is likely to be less sustainable for highly variable earnings or sales. In a similar vein, *DD Measure* is positively related to past sales growth (Doyle et al. 2007). Lakonishok et al. (1994) document that stocks with higher past sales growth earn negative abnormal returns, which possibly results from the market correcting initial overpricing of these stocks.

Finally, through its correlation with losses, *DD Measure* may be directly related to the fundamental signals that predict future returns. Piotroski (2000) demonstrates that the stock market does not fully appreciate the implications of fundamental signals related to the financial health of the firm for the firm's future performance.

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[11] Fama and French (2002) also provide evidence supportive of persistent deviations of average realized returns calculated *ex post* from expected returns. They write that "the high average return for 1951 to 2000 is due to a decline in discount rates that produces a large unexpected capital gain . . . The average stock return of the last half century is a lot higher than expected" (Fama and French 2002, Abstract).

[12] I use the terms "cash flow news" and "cash flow shocks" interchangeably.

Overall, prior literature identifies several characteristics that predict future abnormal returns and are associated with *DD Measure*. To the extent that the documented abnormal returns are related to cash flow shocks, the average realized returns on the stocks with high *DD Measure* and low accrual quality (low *DD Measure* and high accrual quality) reflect on average adverse (positive) cash flow news and produce a downwardly (upwardly) biased estimate of the cost of equity. The asset-pricing tests that use average realized returns are therefore biased against finding a positive risk premium related to accrual quality.

Controlling for Cash Flow Shocks

To purge realized stock returns of cash flow shocks, I rely on the framework from accounting literature on earnings response coefficients (ERCs).[13] A cash flow shock is a stock price reaction due to a revision in the expectation of the entire stream of future cash flows. Since over the life of the firm the sum of cash flows should be equal to the sum of earnings, a cash flow shock can be measured as the return associated with an earnings surprise. For example, Kormendi and Lipe (1987) express the total realized return on stock i as the sum of three components: the expected return, the return due to a revision in the expectation of future earnings (i.e., cash flow shock), and the unexpected return unrelated to the earnings surprise:

$$r_{it+1} = E(r_{it+1}) + r^{CF}_{i,t+1} + \varepsilon^{*}_{i,t+1}, \qquad r^{CF}_{i,t+1} = \sum_{s=0}^{\infty} \beta^{s} \frac{\Delta E(X_{t+1+s}|UX_{t+1})}{P_t}, \tag{1}$$

where $E(r_{it+1})$ is the expected return for t+1; $r^{CF}_{i,t+1}$ is the cash flow shock portion of the return for period t+1; $\varepsilon^{*}_{i,t+1}$ is the unexpected return unrelated to the earnings surprise; X_{t+1} is earnings; UX_{t+1} is the earnings surprise; $\Delta E(X_{t+1+s}|UX_{t+1})$ is the revision in the expectation of earnings for t+1+s occurring between periods t and t+1 as a result of observing the earnings surprise, and β is a discount factor equal to $1/(1+r)$.[14]

Collins and Kothari (1989) show that, under various assumptions regarding the time series process of earnings, the cash flow shock can be expressed as a function of only the current-period earnings surprise:

$$r^{CF}_{i,t+1} = \phi \frac{UX_{t+1}}{P_t}, \tag{2}$$

where ϕ is an ERC that is positively related to earnings surprise persistence and negatively related to the discount rate.

Substituting Equation (2) into Equation (1) produces a return decomposition that is implied by the ERC framework:

$$r_{it+1} = E(r_{it+1}) + \phi \frac{UX_{t+1}}{P_t} + \varepsilon^{*}_{i,t+1}, \qquad r^{CF}_{i,t+1} = \phi \frac{UX_{t+1}}{P_t}. \tag{3}$$

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[13] The literature includes several alternative ways of decomposing unexpected returns into cash flow shock and non-cash flow shock components. For example, Campbell (1991), Vuolteenaho (2002), Larocque (2009), and Callen and Segal (2004) decompose realized returns into cash flow shocks and discount rate shocks using a vector autoregression approach, and Campello et al. (2008) use bond returns to back out expected stock returns. While comparisons between these approaches is outside the scope of this paper, each approach has its own merits and limitations. Campello et al.'s (2008) method requires that all firms have outstanding public debt, which drastically reduces the available sample size. The disadvantages of the Campbell (1991) approach are discussed in detail in Chen and Zhao (2009), and their research design is, in turn, critiqued in Engsted et al. (2010).

[14] The decomposition assumes that the discount rate r is constant over time. To the extent that r is not constant, the realized returns also contain discount rate shocks. In Section V, I discuss the potential implications of non-constant discount rates in the accrual quality setting.

From Equation (3), the cash flow shock portion of the return, $r_{i,t+1}^{CF}$, can be estimated as the fitted value from the cross-sectional regression of realized returns on earnings surprises $UX_{i,t+1}$.[15] Accordingly, I perform a two-step estimation of $r_{i,t+1}^{CF}$. First, I estimate earnings surprises (*SURP*) based on the expectation of earnings derived from a simple statistical earnings prediction model that assumes that annual earnings follow an AR(1) process. I estimate this model cross-sectionally for the hold-out period of one year by regressing current-year earnings on the previous year's earnings. I use the resulting coefficients to construct the next year's predicted earnings. Appendix A provides details on the estimation of earnings surprises.

Second, I estimate the cash flow shock portion of the realized return using cross-sectional or time-series return decomposition. The cross-sectional decomposition involves running monthly cross-sectional regressions of excess stock returns on contemporaneous earnings surprises. The fitted values from these regressions represent cash flow shock returns ($r_{i,t+1}^{CF}$), while the residuals plus the intercept represent returns excluding cash flow shocks, which I call non-cash flow shock returns ($r_{i,t+1}^{NCF}$). Because this estimation assumes a linear relation between earnings surprises and realized returns, I refer to it as the *linear earnings response model*.

The above estimation assumes that the magnitude of ERCs is constant across all firms in the cross-section. However, the literature on the determinants of ERCs suggests that the return response is lower for extreme and/or negative earnings surprises (e.g., Freeman and Tse 1992; Cheng et al. 1992). To account for the resulting nonlinearity in the returns-earnings relation, I estimate cross-sectional regressions of excess stock returns on earnings surprise variables within 5 percent intervals over the earnings surprise distribution. This *nonlinear earnings response model* allows ERCs to vary depending on the magnitude of earnings surprises and produces more accurate cash flow shock estimates.

The time-series return decomposition involves estimating firm-specific regressions of excess returns on earnings surprises using at least 72 monthly returns over at least six fiscal years. Prior research (Lipe et al. 1998) shows that letting ERCs vary by firm explains significantly more variation in returns compared to nonlinear cross-sectional returns-earnings surprise regressions. This estimation, which I call the *firm-specific earnings response model*, estimates cash flow news more precisely. Throughout the paper I report the results obtained using all three decompositions, but I emphasize the results obtained using the more accurate firm-specific decomposition.

III. SAMPLE AND DATA

The sample consists of 92,145 firm-years with fiscal years from 1970 to 2006 and with returns available from April 1971 to March 2008.[16,17] Some tests require additional data causing further sample restrictions.

The *DD Measure* is estimated similarly to FLOS (2005). First, the following cross-sectional regression is estimated for each year and each of the 48 Fama and French (1997) industries (at least

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[15] Other studies that use earnings and earnings forecasts to proxy for cash flow news include Barth et al. (2008) and Campbell et al. (2010). Barth et al. (2008) use analysts' forecast revisions to measure cash flow news in the context of credit risk evaluation. Campbell et al. (2010) use changes in discounted sums of ROE to proxy for the cash flow shock portion of realized stock returns.

[16] The sample consists of observations with necessary data to calculate the *DD Measure*, the market value of equity, book-to-market, and CAPM beta. In addition, observations should have at least one month with a non-missing return in CRSP within the estimation period (the 12 months starting with the fourth month after the fiscal year-end).

[17] To avoid spuriously inflating the returns to the trading strategies based on accrual-related variables (Beaver et al. 2007), all asset-pricing tests incorporate delisting returns for the firms that delist from the exchanges within the estimation period. If the delisting return is missing in CRSP, I replace it with the average delisting return from Shumway (1997) and Shumway and Warther (1999).

20 observations are required for each regression):

$$TCA_{it} = \alpha_t + \beta_{1t}CFO_{it-1} + \beta_{2t}CFO_{it} + \beta_{3t}CFO_{it+1} + \beta_{4t}\Delta REV_{it} + \beta_{5t}PPE_{it} + \varepsilon_{it}, \tag{4}$$

where:

TCA_{it} = total current accruals for year t estimated using the balance sheet approach:
$TCA_{it} = \Delta CA_{it} - \Delta CL_{it} - \Delta Cash_{it} + \Delta STDEBT_{it}$,

where ΔCA_{it} is one-year change in current assets (Compustat #4); ΔCL_{it} is one-year change in current liabilities (Compustat #5); $\Delta Cash_{it}$ is one-year change in cash (Compustat #1); and $\Delta STDEBT_{it}$ is one-year change in current liabilities (Compustat #34);

CFO_{it} = cash flow from operations for year t estimated as $NIBE_{it} - TA_{it}$, where $NIBE_{it}$ is net income before extraordinary items (Compustat #18); and TA_{it} is total accruals ($TA_{it} = TCA_{it} - DEPN_{it}$ where $DEPN_{it}$ is depreciation and amortization expense in year t (Compustat #14));
ΔREV_{it} = one-year change in revenues (Compustat #12); and
PPE_{it} = property, plant, and equipment for year t (Compustat #7).

Subscript i refers to individual firms. All variables are scaled by the average value of total assets (Compustat #6) over years t–1 and t.

DD Measure for each firm i and each year t is the standard deviation of the residuals from the above cross-sectional regression over the period [t–5, t–1]. To prevent the look-ahead bias, the estimation period is lagged by one year compared to the period used in FLOS (2005).[18] *DD Measure* is converted to decile ranks in all asset-pricing tests. The sorting into deciles is performed monthly and each stock is assigned a rank based on the *DD Measure* from the most recent fiscal year that ends 16 to 4 months prior to the sorting. For example, the ranking for July 2004 is based on the *DD Measure* from the latest fiscal year that ended between April 2003 and March 2004.

IV. EMPIRICAL ANALYSES AND RESULTS

Replicating CGV using the *DD Measure* and Future Stock Returns

Accrual Quality Characteristic and Future Returns

CGV document that neither the accrual quality characteristic (*DD Measure*) nor the exposure to accrual quality risk (the loading on *AQ Factor*) can predict future stock returns. I replicate CGV's results for an extended sample period that includes five additional fiscal years from 2002 to 2006.

Panel A of Table 1 replicates the characteristic-based analysis of CGV. The following cross-sectional regression is estimated monthly using individual stock returns (the i subscript refers to individual stocks):

$$R_{i,t+1} - R^f_{t+1} = \text{Intercept}_t + q_t RDD_{it} + b_t BETA_{it} + s_t MKTV_{it} + h_t BMRATIO_{it} + \varepsilon_{it}, \tag{5}$$

where:

R_{it+1} = return on stock i for month t+1;

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[18] The tests are robust to using non-lagged *DD Measure*, as discussed in Section V.

TABLE 1

The *DD Measure* and Future Returns

Panel A: Returns on the AQ Characteristic: Firm-Level Cross-Sectional Regressions

	Intercept	*RDD*	*BETA*	*MKTV*	*BMRATIO*	Adj. R^2
Mean Coefficient	0.84	0.01				0.82%
FM p-value	(0.00)	(0.70)				
Mean Coefficient	1.59	−0.02	0.04	−0.11	0.29	3.27%
FM p-value	(0.00)	(0.20)	(0.79)	(0.00)	(0.00)	

Panel B: Returns on *AQ Factor*: Descriptive Statistics

Factor Returns	n	Mean	Std. Dev.	Min.	Median	Max.	t-statistic	p-value
R^{AQ}	444	0.08	3.23	−11.54	−0.12	21.19	0.51	(0.61)
R^{MktRf}	444	0.46	4.46	−23.13	0.76	16.05	2.19	(0.03)
R^{SMB}	444	0.15	3.22	−16.79	0.02	21.96	0.98	(0.33)
R^{HML}	444	0.45	3.02	−12.40	0.39	13.85	3.13	(0.00)

Panel C: Time-Series Regression of *AQ Factor* Returns on Fama-French Factor Returns (Dependent Variable = R_t^{AQ})

	Intercept	R_t^{MktRf}	R_t^{SMB}	R_t^{HML}	Adj. R^2
Coefficient	0.0173	0.0812	0.6409	−0.1625	54.80%
p-value	(0.87)	(0.00)	(0.00)	(0.00)	

Panel D: Returns on *AQ Factor* Loadings: Portfolio-Level Cross-Sectional Regressions

		Independent Variables				
	Intercept	$\hat{b}_{AQ}$	$\hat{b}_{MktRf}$	$\hat{b}_{SMB}$	$\hat{b}_{HML}$	Adj. R^2
25 B/M and Size Portfolios						
Regression Coefficient	2.30		−1.72	0.15	0.65	74.30%
FM p-value	(0.00)		(0.00)	(0.52)	(0.00)	
Regression Coefficient	0.71	0.16				3.61%
FM p-value	(0.00)	(0.35)				
Regression Coefficient	2.21	−0.08	−1.57	−0.06	0.68	78.42%
FM p-value	(0.00)	(0.69)	(0.00)	(0.74)	(0.00)	
100 *DD Measure* Portfolios						
Regression Coefficient	0.78		0.04	0.11	0.00	2.72%
FM p-value	(0.02)		(0.93)	(0.65)	(1.00)	
Regression Coefficient	0.83	0.07				4.23%
FM p-value	(0.00)	(0.66)				
Regression Coefficient	0.73	0.07	0.05	0.19	0.00	2.03%
FM p-value	(0.04)	(0.65)	(0.91)	(0.47)	(0.99)	
64 B/M, Size, and *DD Measure* Portfolios						
Regression Coefficient	1.18		−0.68	0.24	0.56	46.47%
FM p-value	(0.00)		(0.07)	(0.24)	(0.00)	
Regression Coefficient	0.73	0.13				2.57%
FM p-value	(0.00)	(0.41)				
Regression Coefficient	1.24	0.06	−0.66	0.04	0.59	47.22%
FM p-value	(0.00)	(0.70)	(0.07)	(0.83)	(0.00)	

(continued on next page)

TABLE 1 (continued)

The table presents results of asset-pricing tests for a sample of 92,145 firm-year observations with fiscal years ending in 1970 to 2006 and monthly returns collected from April 1971 to March 2008. All returns are reported in percentages.
Panel A reports mean coefficient estimates from the cross-sectional regression:

$$R_{i,t+1} - R^f_{t+1} = \text{Intercept}_t + q_t RDD_{it} + b_t BETA_{it} + s_t MKTV_{it} + h_t BMRATIO_{it} + \varepsilon_{it},$$

where $R_{i,t+1} - R^f_{t+1}$ denotes the realized return in excess of the one-month T-bill rate for stock *i* and month *t*+1; RDD_{it} is the decile rank based on *DD Measure*; $BETA_{it}$ is the CAPM beta estimated using five years of data ending the most recent December; $MKTV_{it}$ is the natural logarithm of the market value of equity; and $BMRATIO_{it}$ is the natural logarithm of the ratio of book value of equity to market value of equity, where book value is stockholders' book equity plus deferred taxes minus the book value of preferred stock, and the market value of equity is as previously defined. *DD Measure*, $MKTV_{it}$, and $BMRATIO_{it}$ are estimated for the most recent fiscal year ending at least three months prior to month *t*. Reported coefficients and adjusted R^2s are the average values from 444 monthly cross-sectional regressions. Two-sided Fama-MacBeth p-values are reported in parentheses.
Panel B reports descriptive statistics for the returns on *AQ Factor* and the three Fama-French factors. R^{MktRf} is the excess return on the market portfolio. R^{SMB} is the return on the size factor-mimicking portfolio. R^{HML} is the return on the book-to-market factor-mimicking portfolio. The R^{AQ} is an equal-weighted return on a hedge portfolio that is long (short) in the two top (bottom) quintiles of *DD Measure*.
Panel C reports coefficient estimates for the regression:

$$R^{AQ}_t = \alpha + \beta R^{MktRf}_t + s R^{SMB}_t + h R^{HML}_t + \varepsilon_t,$$

where $R^{AQ}{}_t$ is the return on *AQ Factor* for month *t* and all other variables are as defined above. The regression is based on a series of 444 monthly returns. Two-sided p-values are reported in parentheses.
Panel D reports regression coefficients for the portfolio-level cross-sectional regressions based on the full-period average excess returns and the full-period factor betas:

$$\bar{R}_i - \bar{R}^f = \text{Intercept} + \hat{b}_i^{AQ}\lambda^{AQ} + \hat{b}_i^{MktRf}\lambda^{MktRf} + \hat{b}_i^{SMB}\lambda^{SMB} + \hat{b}_i^{HML}\lambda^{HML} + \varepsilon_{it},$$

where $\bar{R}_i - \bar{R}^f$ is the average excess return on portfolio *i* calculated using 444 monthly portfolio returns, and $\hat{b}_i^{AQ}, \hat{b}_i^{MktRf}, \hat{b}_i^{SMB}$, and $\hat{b}_i^{HML}$ are full-period factor betas estimated from the regression:

$$R_{it} - R^f_t = \alpha_i + b_i^{AQ}R_t^{AQ} + b_i^{MktRf}R_t^{MktRf} + b_i^{SMB}R_t^{SMB} + b_i^{HML}R_t^{HML} + \varepsilon_{it},$$

where R_{it} is the equal-weighted return on portfolio *i* for month *t* and all other variables are as defined above. Each regression is based on a series of 444 monthly returns. Two-sided Fama-MacBeth p-values adjusted for the Shanken (1992) correction (reported in parentheses) are based on the coefficients obtained by estimating the cross-sectional regression monthly. The panel partitions refer to three types of portfolios: (1) "25 B/M and Size Portfolios" are based on quintiles of the book-to-market ratio and size; (2) "100 *DD Measure* Portfolios" are based on percentiles of *DD Measure*; and (3) "64 B/M, Size, and *DD Measure* Portfolios" are based on quartiles of the book-to-market ratio, size, and *DD Measure*.
For each firm and each year *t*, *DD Measure* is calculated as the standard deviation of residuals over the [*t*–5, *t*–1] period, where residuals are obtained from the following regression estimated cross-sectionally for each year, and each of the 48 Fama and French (1997) industries:

$$TCA_{it} = \alpha_t + \beta_{1t}CFO_{it-1} + \beta_{2t}CFO_{it} + \beta_{3t}CFO_{it+1} + \beta_{4t}\Delta REV_{it} + \beta_{5t}PPE_{it} + \varepsilon_{it},$$

where TCA_{it} is total current accruals for year *t* (estimated using the balance sheet approach: $TCA_{it} = \Delta CA_{it} - \Delta CL_{it} - \Delta Cash_{it} + \Delta STDEBT_{it}$, where ΔCA_{it} is one-year change in current assets (#4); ΔCL_{it} is one-year change in current liabilities (#5); $\Delta Cash_{it}$ is one-year change in cash (#1); $\Delta STDEBT_{it}$ is one-year change in debt in current liabilities (#34)); CFO_{it} is cash flow from operations for year *t* estimated as $NIBE_{it} - TA_{it}$, where $NIBE_{it}$ is net income before extraordinary items (#18); TA_{it} is total accruals ($TA_{it} = TCA_{it} - DEPN_{it}$, where $DEPN_{it}$ is depreciation and amortization expense in year *t* (#14)); ΔREV_{it} is one-year change in revenues (#12); PPE_{it} is property, plant, and equipment for year *t* (#7). All variables are scaled by the average value of total assets (Compustat #6) over years *t*–1 and *t*. At least 20 observations are required for each regression. All numbered items in the description of variables refer to Compustat annual data items.

R^f_{t+1} = one-month T-bill rate for month $t+1$;

RDD_{it} = decile rank based on the *DD Measure* determined at the end of month t as described in Section III;

$BETA_{it}$ = CAPM beta estimated using 60 months of data prior to the most recent January;

$MKTV_{it}$ = natural logarithm of market value of equity at the latest fiscal year-end preceding month t by at least three months; and

$BMRATIO_{it}$ = natural logarithm of the book-to-market ratio at the latest fiscal year preceding month t by at least three months where book value is stockholders' book equity plus deferred taxes minus the book value of preferred stock and market value of equity is as defined previously.

The mean coefficient estimates are based on monthly estimates from the cross-sectional regressions. The p-values are based on Fama-MacBeth (1973) t-statistics that rely on the time-series distribution of cross-sectional coefficients. The coefficient on the rank of *DD Measure* (*RDD*) represents the increase in the average realized return from one decile of *DD Measure* to the next; when multiplied by 9, it can be converted to the difference in average returns between the first and tenth deciles of *DD Measure*.

The results in Panel A of Table 1 are similar to those reported in CGV. Specifically, the coefficient on *RDD* is positive but not significant for the regression without controls, and is negative and not significant when the CAPM beta, market value of equity, and book-to-market ratio are added as controls.

The above results are based on the cross-section of individual stock returns. Next, I verify that *DD Measure* does not predict future returns at the portfolio level. As in FLOS and CGV, I form a factor-mimicking portfolio (*Accrual Quality* [*AQ*] *Factor*) by taking a short (long) position in the stocks falling in the two bottom (top) quintiles of *DD Measure* at the beginning of each month.

Panel B of Table 1 reports the descriptive statistics for *AQ Factor* and for the three Fama and French (1993) factors—market premium (*MktRf*), size (*SMB*), and book-to-market (*HML*). The raw return on *AQ Factor* (0.08 percent per month) is not statistically significant.

Panel C of Table 1 presents the results of regressing *AQ Factor* returns on the three Fama-French factor returns. Collectively, the Fama-French factors explain 55 percent of the variation in *AQ Factor* and there is no evidence of an abnormal return earned on *AQ Factor*. The intercept of the regression, Jensen's alpha, is equal to 0.02 percent per month and is not statistically significant. Overall, there is no evidence that lower accrual quality, as proxied by *DD Measure*, is associated with higher stock returns, consistent with CGV's result.

AQ Factor Loadings and Future Stock Returns

Although thus far I find no evidence that *DD Measure* predicts future returns, it is possible that *DD Measure* is a noisy proxy for the systematic component of accrual risk, i.e., the returns' co-variation with *AQ Factor*. To establish whether the co-variation with *AQ Factor* is priced by the market, I perform a two-stage analysis that is similar to that reported in CGV.

The two-stage technique is used extensively in the asset-pricing literature to verify whether the loadings on the candidate factor explain the cross-sectional variation in expected stock returns, i.e., whether stocks with higher "betas" earn higher returns on average (e.g., Fama and MacBeth 1973; Jagannathan and Wang 1996). The first stage involves estimating loadings on a set of factors—"factor betas"—by regressing returns for each portfolio on the contemporaneous factor returns. The first-stage regressions use the entire time-series of returns and are estimated separately for each portfolio, where subscripts i and t refer to portfolios and months, respectively:

$$R_{it} - R_t^f = \alpha_i + b_i^{AQ} R_t^{AQ} + b_i^{MktRf} R_t^{MktRf} + b_i^{SMB} R_t^{SMB} + b_i^{HML} R_t^{HML} + \varepsilon_{it}, \tag{6}$$

where:

R_{it} = return on portfolio i for month t;
R_t^f = one-month T-bill rate;
R_t^{AQ} = return on *AQ Factor*;
R_t^{MktRf} = return on a market portfolio (CRSP value-weighted index) in excess of the risk-free rate;
R_t^{SMB} = return on a factor-mimicking hedge portfolio for size; and
R_t^{HML} = return on a factor-mimicking hedge portfolio for book-to-market.

The second stage involves a cross-sectional regression of realized returns on the factor loadings estimated in the first stage:

$$\bar{R}_{it} - R_{it}^f = \text{Intercept}_t + \hat{b}_i^{AQ} \lambda_t^{AQ} + \hat{b}_i^{MktRf} \lambda_t^{MktRf} + \hat{b}_i^{SMB} \lambda_t^{SMB} + \hat{b}_i^{HML} \lambda_t^{HML} + \varepsilon_{it}, \tag{7}$$

where:

$\bar{R}_{it}$ = average return on portfolio i for month t;
R_{it}^f = one-month T-bill rate for month t; and
$\hat{b}_i^{AQ}$, $\hat{b}_i^{MktRf}$, $\hat{b}_i^{SMB}$, $\hat{b}_i^{HML}$ = factor betas estimated in the first stage.[19]

The premium magnitudes are estimated from a single cross-sectional regression (7), using averages of monthly portfolio returns. The statistical significance of the regression coefficients is derived from estimating regression (7) monthly and computing Fama-MacBeth (1973) t-statistics adjusted for the Shanken (1992) correction.

Panel D of Table 1 reports the results of a second-stage estimation for three sets of portfolios: (1) 25 portfolios formed by independently sorting stocks into quintiles based on book-to-market and size; (2) 100 portfolios based on the percentiles of *DD Measure*; and (3) 64 portfolios formed by independently sorting stocks into quartiles based on book-to-market, size, and *DD Measure*. Portfolio returns are calculated by equal-weighting the individual stock returns.[20] For each portfolio set, I estimate three types of factor models: (1) the benchmark model that includes only the three Fama-French (1993) factors; (2) the benchmark model that includes only *AQ Factor*; and (3) the full model, which includes the three Fama-French factors plus *AQ Factor*. The results are consistent with the findings in CGV—the *AQ Factor* premium, i.e., the regression coefficient on b_i^{AQ}, is not statistically significant for any model specification.

Overall, CGV's results are confirmed for my sample period. Higher exposure to accrual quality risk, as proxied by the loading on *AQ Factor*, is not associated with higher stock returns.

The *DD Measure* and Future Stock Returns: Controlling for Cash Flow Shocks

The analyses presented in this section are constrained by the availability of data required for return decomposition. The cross-sectional (time-series) return decomposition requires non-missing earnings surprises (and at least 72 months with non-missing returns and earnings surprises per firm), which constrains the sample to 86,553 (75,026) observations.

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[19] When the second-stage regressions include only a subset of estimated loadings, i.e., only $\hat{b}_i^{AQ}$, the factor betas are based on the first-stage regressions that include only the corresponding subset of factor returns, i.e., only the return on *AQ Factor*.

[20] The weighting procedure is different from that in CGV, who use value-weighting to calculate portfolio returns. While value-weighting has been used frequently in prior literature (e.g., Fama and French 1993), equal-weighting is also acceptable (e.g., Carhart 1997). To detect the effect of accrual quality, it may be preferable to use equal-weighting since it is likely that the quality of accruals, and accounting information in general, is more important for smaller firms with less saturated information environments.

Descriptive Statistics

Panels A and C of Table 2 report descriptive statistics for excess returns and its two components, cash flow shocks ($r_{i,t+1}^{CF}$) and returns excluding cash flow shocks ($r_{i,t+1}^{NCF}$). On average, cash flow shocks are either slightly negative or positive (positive) using linear or nonlinear return decomposition in Panel A (firm-specific decomposition in Panel C). As a result, returns excluding cash flow shocks are on average equal to or lower than the realized excess returns, and are negative in over 50 percent of observations across all decomposition methods. Note however that returns excluding cash flow shocks are not equal to expected returns, i.e., cost of capital. First, they represent returns in excess of risk-free rates because the decomposition is based on excess returns. Second, and more importantly, they contain discount rate shocks, which can *ex post* be negative in a given month, or even on average for a finite sample period. To the extent that discount rate shocks are not positively correlated with *DD Measure*, this shock contamination introduces noise in the analyses but does not cause spurious results. Empirical evidence presented in Section V is consistent with the absence of such positive correlation.

Univariate Analysis

In Section II, I conjecture that stocks with higher *DD Measure* (lower accrual quality) experienced more negative cash flow shocks in my sample period. To test this conjecture, I first verify that poor accrual quality stocks experienced on average more negative earnings surprises.

Panels B and D of Table 2 report mean earnings surprise proxies by *DD Measure* decile for the cross-sectional and time-series return decomposition samples. There is a strong negative monotonic relation between *DD Measure* and earnings surprises. The average differences in earnings surprises between the worst (Decile 10) and the best (Decile 1) accrual quality stocks, reported in the row "10 – 1," are negative and statistically significant (p-value 0.00).

Next, I examine whether the significantly more negative earnings surprises experienced by poor accrual quality stocks translate into significantly lower cash flow shocks. Panels B and D of Table 2 report the mean total realized excess returns decomposed into cash flow shock ($r_{i,t+1}^{CF}$) and non-cash flow shock ($r_{i,t+1}^{NCF}$) components by *DD Measure* decile. The decomposition in Panel B (Panel D) is based on the cross-sectional (time-series) estimation. The average differences in excess returns and return components between the worst (Decile 10) and the best (Decile 1) accrual quality stocks are reported in the row "10 – 1."

The second column of Panels B and D reports average excess returns by *DD Measure* decile. The differences between average returns of the extreme *DD Measure* deciles are not statistically significant, confirming CGV's result for the samples constrained by the return decomposition data requirements.

The remaining columns of Panel B (Panel D) report average cash flow shocks ($r_{i,t+1}^{CF}$) and returns excluding cash flow shocks ($r_{i,t+1}^{NCF}$) for the linear and nonlinear earnings response models (firm-specific earnings response model). Cash flow shocks are lower for stocks in the highest *DD Measure* decile across all earnings response models, with the magnitude of the "10 – 1" difference ranging from –0.25 percent to –0.35 percent monthly with p-values of at least 0.02.[21] In contrast, returns excluding cash flow shocks ($r_{i,t+1}^{NCF}$) are consistently higher for the worst (Decile 10) accrual quality stocks. The differences for returns excluding cash flow shocks reported in the row "10 – 1" range from 0.42 percent (p-value 0.19) to 0.68 percent (p-value 0.03).

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[21] When discussing the statistical significance of risk premiums, I refer to coefficients with two-sided p-values of less than 0.10 as statistically significant because corresponding one-sided p-values testing directional hypothesis of positive risk premiums would be below 0.05.

TABLE 2

Controlling for Cash Flow Shocks
Univariate Analysis

Panel A: Descriptive Statistics for Cross-Sectional Return Decomposition

Variable	Mean	1%	5%	Lower Quartile	Median	Upper Quartile	95%	99%
Total Excess Return	0.96	−33.62	−19.44	−6.45	−0.32	6.76	23.77	49.53
Linear Earnings Response Model								
r^{NCF}	0.96	−33.15	−19.30	−6.46	−0.23	6.74	23.77	49.42
r^{CF}	−0.01	−4.65	−1.13	−0.07	0.03	0.19	0.92	3.15
Nonlinear Earnings Response Model								
r^{NCF}	0.39	−48.71	−29.77	−10.73	−0.54	10.25	33.15	60.16
r^{CF}	0.57	−39.99	−20.66	−5.09	0.07	5.96	23.39	43.67

Panel B: Cross-Sectional Return Decomposition

Decile of *DD MEASURE*	*SURP*	Total Excess Return	Linear Earnings Response Model r^{CF}	Linear Earnings Response Model r^{NCF}	Nonlinear Earnings Response Model r^{CF}	Nonlinear Earnings Response Model r^{NCF}
1	0.046	0.88	0.10	0.77	0.81	0.07
2	0.032	0.87	0.07	0.80	0.59	0.28
3	0.034	0.88	0.06	0.82	0.65	0.24
4	0.018	0.87	0.03	0.84	0.60	0.27
5	0.011	0.96	0.01	0.95	0.54	0.43
6	0.002	1.08	−0.01	1.09	0.58	0.50
7	−0.015	0.88	−0.03	0.92	0.53	0.36
8	−0.024	1.02	−0.05	1.07	0.49	0.53
9	−0.035	1.07	−0.09	1.15	0.44	0.63
10	−0.055	1.05	−0.15	1.20	0.46	0.59
10 − 1	−0.101	0.17	−0.25	0.42	−0.35	0.52
p-value	(0.00)	(0.72)	(0.00)	(0.19)	(0.02)	(0.13)

Panel C: Descriptive Statistics for Time-Series Return Decomposition

Variable	Mean	1%	5%	Lower Quartile	Median	Upper Quartile	95%	99%
Total Excess Return	0.99	−31.73	−18.52	−6.18	−0.25	6.66	22.88	46.84
Firm-Specific Earnings Response Model								
r^{NCF}	0.59	−32.03	−18.79	−6.57	−0.44	6.26	22.46	46.20
r^{CF}	0.40	−5.53	−1.92	−0.11	0.18	0.85	3.20	6.88

(continued on next page)

TABLE 2 (continued)

Panel D: Time-Series Return Decomposition

Decile of *DD MEASURE*	*SURP*	Total Excess Return	Firm-Specific Earnings Response Model r^{CF}	Firm-Specific Earnings Response Model r^{NCF}
1	0.051	0.88	0.50	0.38
2	0.040	0.87	0.52	0.35
3	0.043	0.88	0.49	0.39
4	0.027	0.91	0.44	0.47
5	0.020	0.97	0.45	0.52
6	0.010	1.02	0.41	0.61
7	0.000	1.02	0.39	0.63
8	−0.012	1.04	0.34	0.70
9	−0.022	1.09	0.29	0.80
10	−0.037	1.25	0.18	1.06
10 − 1	−0.088	0.37	−0.32	0.68
p-value	(0.00)	(0.29)	(0.00)	(0.03)

The table reports descriptive statistics and compares earnings surprises, cash flow shocks, and realized returns excluding cash flow shocks from the cross-sectional (time-series) return decomposition across deciles of the accrual quality distribution for a sample of 86,553 (75,026) observations from 1970 to 2006.

Panel A (Panel C) contains descriptive statistics for returns decomposed using cross-sectional (time-series) regressions of excess returns on earnings surprises (*SURP*). The fitted values from these regressions represent cash flow shocks ($r^{CF}_{i,t+1}$), and the residuals plus intercepts represent returns excluding cash flow shocks ($r^{NCF}_{i,t+1}$). In Panel A, "Linear Earnings Response Model" refers to estimating ($r^{CF}_{i,t+1}$) and ($r^{NCF}_{i,t+1}$) from the monthly cross-sectional regressions for the full sample. The "Nonlinear Earnings Response Model" refers to estimating ($r^{CF}_{i,t+1}$) and ($r^{NCF}_{i,t+1}$) from the monthly cross-sectional regressions estimated separately within each of the 20 *SURP* portfolios. In Panel C, "Firm-Specific Earnings Response Model" refers to estimating ($r^{CF}_{i,t+1}$) and ($r^{NCF}_{i,t+1}$) from the firm-specific time-series regressions based on at least 72 monthly returns.

Panels B and D report means of earnings surprises (*SURP*), total excess returns, cash flow shocks ($r^{CF}_{i,t+1}$), and returns excluding cash flow shocks ($r^{NCF}_{i,t+1}$) by *DD Measure* decile. In Panel B (Panel D) returns are decomposed using cross-sectional (time-series) regressions of excess returns on earnings surprises (*SURP*). The average differences between earnings surprises and returns in the bottom and top deciles of accrual quality are reported in the row "10 − 1." The two-sided p-values for the return (earnings surprise) differences are computed using the Fama-MacBeth approach with a Newey and West (1987; hereafter, Newey-West) correction with six lags.

All returns are reported in percentages. Details of earnings surprise (*SURP*) estimation are in Appendix A.

Overall, the univariate results are consistent with firms with high *DD Measures* (low accrual quality) experiencing more negative cash flow shocks in my sample period. Firms with low accrual quality on average earned higher returns after excluding cash flow shocks, although the difference in returns between the extreme accrual quality deciles is statistically significant only for the time-series firm-specific result, the most powerful return decomposition.

AQ Characteristic and Future Stock Returns Excluding Cash Flow Shocks

Next, I examine whether *DD Measure* as a characteristic is significantly associated with realized returns after excluding cash flow shocks. Table 3 reports the stock-level regressions of future returns on the ranked *DD Measure* (*RDD*). The analyses reported in Panel A (Panel B) rely on the cross-sectional (time-series) return decomposition. The panels are further partitioned based

TABLE 3

Controlling for Cash Flow Shocks
Characteristic-Based Firm-Level Tests

Panel A: Controlling for Cash Flow Shocks using Cross-Sectional Return Decomposition

	Intercept	*RDD*	*BETA*	*MKTV*	*BMRATIO*	Adj. R^2
Excess Returns						
Mean Coefficient	0.76	0.02				0.87%
FM p-value	(0.00)	(0.52)				
Mean Coefficient	1.59	−0.01	0.02	−0.12	0.27	3.45%
FM p-value	(0.00)	(0.30)	(0.87)	(0.00)	(0.00)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Linear Earnings Response Model						
Mean Coefficient	0.63	0.04				0.80%
FM p-value	(0.00)	(0.10)				
Mean Coefficient	1.60	0.00	0.11	−0.16	0.29	3.31%
FM p-value	(0.00)	(0.90)	(0.44)	(0.00)	(0.00)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Nonlinear Earnings Response Model						
Mean Coefficient	0.00	0.05				0.58%
FM p-value	(0.99)	(0.08)				
Mean Coefficient	0.66	0.02	0.13	−0.12	0.19	2.12%
FM p-value	(0.02)	(0.27)	(0.40)	(0.00)	(0.02)	

Panel B: Controlling for Cash Flow Shocks using Time-Series Return Decomposition

	Intercept	*RDD*	*BETA*	*MKTV*	*BMRATIO*	Adj. R^2
Excess Returns						
Mean Coefficient	0.74	0.03				0.88%
FM p-value	(0.00)	(0.24)				
Mean Coefficient	1.67	−0.01	0.03	−0.14	0.21	3.66%
FM p-value	(0.00)	(0.54)	(0.85)	(0.00)	(0.00)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Firm-Specific Earnings Response Model						
Mean Coefficient	0.20	0.06				0.86%
FM p-value	(0.32)	(0.02)				
Mean Coefficient	0.76	0.03	0.10	−0.09	0.22	3.58%
FM p-value	(0.00)	(0.00)	(0.48)	(0.02)	(0.00)	

The table reports results of characteristic-based asset-pricing tests based on the realized returns after excluding cash flow shocks (r^{NCF}).

The sample in Panel A (Panel B) consists of 86,553 (75,026) observations over the 1970 to 2006 period and is restricted by the availability of an earnings surprise measure (*SURP*) (and at least 72 monthly returns for firm-specific return decomposition).

The table contains mean coefficient estimates from the cross-sectional regressions:

$$r^{NCF}_{i,t+1} = \text{Intercept}_t + q_t RDD_{it} + Controls + \varepsilon_{it},$$

where ($r^{NCF}_{i,t+1}$) denotes the realized return excluding cash flow shocks for stock *i* and month *t*+1; RDD_{it} is the decile rank based on *DD Measure*. All other variables are as defined in Table 1. The returns excluding cash flow shocks ($r^{NCF}_{i,t+1}$) are the residuals plus intercepts from the regressions of individual stock excess returns on earnings surprises. In Panel A, "Linear Earnings Response Model" refers to estimating ($r^{CF}_{i,t+1}$) and ($r^{NCF}_{i,t+1}$) from the monthly cross-sectional regressions for the full sample. The "Nonlinear Earnings Response Model" refers to estimating ($r^{CF}_{i,t+1}$) and ($r^{NCF}_{i,t+1}$) from the monthly cross-sectional regressions estimated separately within each of the 20 *SURP* portfolios. In Panel B, "Firm-Specific Earnings Response Model" refers to estimating ($r^{CF}_{i,t+1}$) and ($r^{NCF}_{i,t+1}$) from the firm-specific time-series regressions based on at least 72 monthly returns.

Reported coefficients and adjusted R^2s are the average values from 444 monthly cross-sectional regressions. Two-sided Fama-MacBeth p-values are reported in parentheses.

on the excess return measure used in the estimation. The first partition of each panel uses total excess returns; the remaining partitions use excess returns excluding cash flow shocks ($r^{NCF}_{i,t+1}$). The premiums on *RDD* using total excess returns are not statistically significant either before or after controlling for other risk-related characteristics (CAPM beta, market value of equity, and book-to-market), confirming that CGV's result holds for the constrained samples.

When excess returns excluding cash flow shocks are regressed on *RDD* without controlling for other risk-related characteristics, the premiums on *RDD* are consistently positive and significant at least at the 10 percent level across all return-decomposition methods in Panels A and B. The magnitude of the premium ranges from 0.04 percent to 0.06 percent monthly. The premium is also economically significant and can be interpreted as the difference between the highest and the lowest deciles of *DD Measure* of approximately 4.3 percent per annum (0.04 percent × 12 × 9) to 6.5 percent per annum (0.06 percent × 12 × 9).

The premium on *RDD* is reduced in magnitude after the CAPM beta (*BETA*), market value of equity (*MKTV*), and book-to-market ratio (*BMRATIO*) are included as additional controls.[22] The premium is no longer significant in Panel A where cash flow shocks are removed using cross-sectional return decomposition, but remains statistically significant in Panel B where cash flow shocks are removed using a more powerful firm-specific return decomposition. The premium reported in Panel B also remains economically significant—the difference in returns excluding cash flow shocks between the highest and the lowest deciles of accrual quality is approximately 3.24 percent per annum (0.03 percent × 12 × 9).[23]

In sum, accrual quality, as proxied by *DD Measure*, is significantly inversely related to future realized stock returns after controlling for cash flow shocks when the return decomposition relies on the most powerful—firm-specific time-series—model.

AQ Factor Loadings and Future Stock Returns Excluding Cash Flow Shocks

Table 4 contains the results of the second-stage regressions of realized returns on factor loadings. The analyses reported in Panel A (Panel B) rely on the sample available for the cross-sectional (time-series) return decomposition. The panels are partitioned by the types of portfolios and return measures used in the estimation.[24]

The first row of each partition in Panels A and B reports the results of the CGV replication for the constrained samples, where asset-pricing regressions are based on total excess returns. The premiums on *AQ Factor*, the coefficient on *AQ Factor* beta, are not significant in either constrained sample for any portfolio partition.

The second row of each partition in Panel A reports the results based on returns that exclude cash flow shocks using the linear earnings response model. The premium on *AQ Factor* is positive

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[22] Untabulated results suggest that the premium reduction is driven by controlling for the market value of equity. When the market value of equity is decomposed into several components corresponding to market frictions, scale of operations, earnings announcement informativeness, and size risk, the premium on accrual quality is significantly reduced only by controlling for the market frictions component. It is therefore possible that the market value of equity captures higher information asymmetry or higher information risk exposure of low accrual quality stocks. A detailed path analysis of relationships between accrual quality and expected returns (e.g., Bhattacharya et al. 2012) is outside the scope of this paper.

[23] The 3.24 percent difference is substantial in terms of its effect on the market value of equity. For example, in the simple Gordon growth model, under a zero dividend growth assumption, price equals expected dividend divided by cost of capital. So, a 1 percent decrease in cost of capital approximately translates into a 1 percent increase in firm value. From Panel C of Table 2, the average annualized non-cash flow shock return for stocks within the worst accrual quality decile is 12.72 percent (1.06 percent × 12). The 3.24 percent decrease in expected return obtained by moving from the worst to the best accrual quality decile reduces cost of capital by 25 percent (3.24 percent/12.72 percent), with a corresponding 25 percent increase in the market value of equity.

[24] The *AQ Factor* loadings are based on *AQ Factor* returns adjusted for cash flow shocks.

TABLE 4

Controlling for Cash Flow Shocks
Factor-Loading-Based, Portfolio-Level Tests

Panel A: Controlling for Cash Flow Shocks using Cross-Sectional Return Decomposition

	Independent Variables					
	Intercept	$\hat{b}_{AQ}$	$\hat{b}_{MktRf}$	$\hat{b}_{SMB}$	$\hat{b}_{HML}$	**Adj. R^2**
25 B/M and Size Portfolios						
Excess Returns						
Regression Coefficient	2.09	−0.02	−1.48	−0.13	0.67	81%
FM p-value	(0.00)	(0.93)	(0.00)	(0.51)	(0.00)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Linear Earnings Response Model						
Regression Coefficient	1.70	0.14	−1.25	0.02	0.75	82%
FM p-value	(0.00)	(0.47)	(0.01)	(0.91)	(0.00)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Nonlinear Earnings Response Model						
Regression Coefficient	0.31	0.37	−0.13	−0.21	0.59	74%
FM p-value	(0.57)	(0.10)	(0.82)	(0.34)	(0.02)	
100 *DD Measure* Portfolios						
Excess Returns						
Regression Coefficient	0.78	0.10	0.05	0.06	−0.09	7%
FM p-value	(0.02)	(0.51)	(0.91)	(0.81)	(0.76)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Linear Earnings Response Model						
Regression Coefficient	0.47	0.26	0.21	0.19	−0.03	41%
FM p-value	(0.15)	(0.10)	(0.63)	(0.45)	(0.91)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Nonlinear Earnings Response Model						
Regression Coefficient	−0.66	0.30	0.64	0.51	−0.11	38%
FM p-value	(0.19)	(0.10)	(0.19)	(0.08)	(0.72)	
64 B/M, Size, and *DD Measure* Portfolios						
Excess Returns						
Regression Coefficient	1.49	0.07	−0.91	−0.05	0.59	66%
FM p-value	(0.00)	(0.71)	(0.02)	(0.80)	(0.00)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Linear Earnings Response Model						
Regression Coefficient	1.15	0.22	−0.70	0.07	0.67	71%
FM p-value	(0.00)	(0.23)	(0.06)	(0.71)	(0.00)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Nonlinear Earnings Response Model						
Regression Coefficient	0.46	0.37	−0.30	−0.06	0.41	49%
FM p-value	(0.41)	(0.07)	(0.56)	(0.97)	(0.08)	

Panel B: Controlling for Cash Flow Shocks using Time-Series Return Decomposition

	Independent Variables					
	Intercept	$\hat{b}_{AQ}$	$\hat{b}_{MktRf}$	$\hat{b}_{SMB}$	$\hat{b}_{HML}$	**Adj. R^2**
25 B/M and Size Portfolios						
Excess Returns						
Regression Coefficient	1.69	0.18	−1.09	−0.09	0.63	83%
FM p-value	(0.00)	(0.33)	(0.02)	(0.64)	(0.00)	

(continued on next page)

TABLE 4 (continued)

	Independent Variables					
	Intercept	$\hat{b}_{AQ}$	$\hat{b}_{MktRf}$	$\hat{b}_{SMB}$	$\hat{b}_{HML}$	Adj. R^2
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Firm-Specific Earnings Response Model						
Regression Coefficient	0.40	0.33	−0.17	−0.13	0.62	86%
FM p-value	(0.27)	(0.08)	(0.71)	(0.49)	(0.00)	
100 *DD Measure* Portfolios						
Excess Returns						
Regression Coefficient	0.86	0.20	−0.10	0.11	0.04	26%
FM p-value	(0.01)	(0.21)	(0.82)	(0.67)	(0.89)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Firm-Specific Earnings Response Model						
Regression Coefficient	0.60	0.39	−0.28	0.20	−0.06	61%
FM p-value	(0.07)	(0.02)	(0.52)	(0.44)	(0.84)	
64 B/M, Size, and *DD Measure* Portfolios						
Excess Returns						
Regression Coefficient	1.26	0.19	−0.71	0.07	0.53	56%
FM p-value	(0.00)	(0.28)	(0.06)	(0.79)	(0.00)	
Excess Returns Excluding Cash Flow Shocks: r^{NCF} from Firm-Specific Earnings Response Model						
Regression Coefficient	−0.15	0.37	0.26	0.12	0.50	49%
FM p-value	(0.82)	(0.04)	(0.61)	(0.65)	(0.01)	

The table reports results of the second stage of the two-stage asset-pricing tests based on the realized returns after excluding cash flow shocks (r^{NCF}). The sample in Panel A (Panel B) consists of 86,553 (75,026) observations over the 1970 to 2006 period and is restricted by the availability of an earnings surprise measure (*SURP*) (and at least 72 monthly returns for firm-specific return decomposition).

The table reports regression coefficients from the cross-sectional regressions based on the full-period average excess returns excluding cash flow shocks and the full-period factor betas:

$$\overline{r_i^{NCF}} = \text{Intercept} + \hat{b}_i^{AQ}\lambda^{AQ} + \hat{b}_i^{MktRf}\lambda^{MktRf} + \hat{b}_i^{SMB}\lambda^{SMB} + \hat{b}_i^{HML}\lambda^{HML} + \varepsilon_{it},$$

where $\overline{r_i^{NCF}}$ is the average excess return excluding cash flow shocks on portfolio *i* calculated using 444 monthly portfolio returns, and $\hat{b}_i^{AQ}$, $\hat{b}_i^{MktRf}$, $\hat{b}_i^{SMB}$, and $\hat{b}_i^{HML}$ are full-period factor betas estimated by regressing non-cash flow shock portfolio returns on factor returns.

The returns excluding cash flow shocks ($r_{i,t+1}^{NCF}$) are the residuals plus intercepts from the regressions of individual stock excess returns on earnings surprises. In Panel A, "Linear Earnings Response Model" refers to estimating ($r_{i,t+1}^{CF}$) and ($r_{i,t+1}^{NCF}$) from the monthly cross-sectional regressions for the full sample. The "Nonlinear Earnings Response Model" refers to estimating ($r_{i,t+1}^{CF}$) and ($r_{i,t+1}^{NCF}$) from the monthly cross-sectional regressions estimated separately within each of the 20 *SURP* portfolios. In Panel B, "Firm-Specific Earnings Response Model" refers to estimating ($r_{i,t+1}^{CF}$) and ($r_{i,t+1}^{NCF}$) from the firm-specific time-series regressions based on at least 72 monthly returns.

Each regression is based on a series of 444 monthly returns. Coefficients and adjusted R^2s are from a single cross-sectional regression that uses average portfolio returns. Two-sided Fama-MacBeth p-values adjusted for the Shanken (1992) correction (reported in parentheses) are based on the coefficients obtained by estimating the cross-sectional regression monthly. The panel partitions refer to three types of portfolios: (1) "25 B/M and Size Portfolios" are based on quintiles of the book-to-market ratio and size; (2) "100 *DD Measure* Portfolios" are based on percentiles of *DD Measure*; and (3) "64 B/M, Size, and *DD Measure* Portfolios" are based on quartiles of the book-to-market ratio, size, and *DD Measure*.

across all portfolios, but statistically significant at the 10 percent level only for the "100 *DD Measure*" portfolios that have the most power to detect the premium on accrual quality. The third row of each partition in Panel A reports the results based on the returns that exclude cash flow shocks using the nonlinear earnings response model. The premium on *AQ Factor* is between 0.30

percent and 0.37 percent monthly and is statistically significant across all portfolio partitions, with p-values ranging from 0.07 to 0.10. The premiums are also economically significant, ranging from 3.6 percent per annum (0.30 percent × 12) to 4.44 percent per annum (0.37 percent × 12).[25,26]

The second row of each partition in Panel B, Table 4 reports the results based on excluding cash flow shocks using the firm-specific earnings response model. The premium on *AQ Factor* is positive and statistically significant at the 10 percent level across all portfolio partitions, with the magnitudes of the premium ranging from 0.33 percent to 0.39 percent monthly. The corresponding annualized premiums range from 3.96 percent (0.33 percent × 12) to 4.68 percent (0.39 percent × 12).

Overall, the asset-pricing tests presented in this section provide evidence that is generally consistent with *AQ Factor* being priced in stock returns after controlling for the three Fama-French (1993) factors. The *AQ Factor* betas, i.e., the loadings on *AQ Factor*, are significantly positively related to future returns excluding cash flow shocks, when the latter are excluded using the two most powerful return decomposition models that take into account the nonlinearity in the returns-earnings surprise relation or allow earnings surprise coefficients to vary by firm.

V. ADDITIONAL ANALYSES AND ROBUSTNESS CHECKS

Alternative Return Decomposition

The methodology described in Section II decomposes returns in two parts: cash flow shocks (r^{CF}) and returns excluding cash flow shocks (r^{NCF}), with the latter being used in asset-pricing tests. In this section, I perform an alternative return decomposition based on the firm-specific earnings response model.

Equation (3) implies that intercepts from the firm-specific regressions of excess returns on earnings surprises can serve as proxies for unconditional time-invariant expected returns (r^{ER}). The fitted values continue to represent cash flow shocks (r^{CF}), while the residuals (r^{RES}) contain discount rate shocks and deviations of conditional expected returns that are month-specific from unconditional expected returns.

Panel A of Table 5 provides descriptive statistics of excess return components resulting from the above return decomposition. By construction, average residual returns are equal to zero. Expected excess returns (r^{ER}) have the mean (median) of 0.59 percent (0.57 percent) per month. About 12 percent of implied raw expected returns (r^{ER} plus the risk-free rate) are negative, which could be due to intercepts absorbing *ex post* on-average negative discount rate news for individual firms during the estimation period. The next section explores potential biases arising due to cross-sectional variation in discount rate news. Panel B of Table 5 provides the results of return decomposition within deciles of *DD Measure*. The expected returns are on average 0.61 percent per month higher for firms in the highest *DD Measure* decile, compared to firms in the lowest *DD*

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[25] The premiums obtained in the second stage of the two-stage test should be equal to the average abnormal return on the factor, so the significant premiums reported in this section may seem inconsistent with the insignificant abnormal returns on *AQ Factor* in Table 1. The inconsistency is due to the fact that the factor returns reported in Table 1 are contaminated by cash flow shocks. When I reconstruct *AQ Factor* using returns excluding cash flow shocks, the abnormal returns on *AQ Factor* are consistent with the premiums reported in this section.

[26] The two-stage analyses reported in this section use equal-weighted portfolio returns. As discussed earlier in Section IV, equal-weighting is preferable for detecting the effects of accrual quality given that the quality of accruals is more important for smaller firms with less saturated information environments. The untabulated results confirm that the difference in both the total returns and returns excluding cash flow shocks between the poor and the good accrual quality firms is higher for smaller stocks compared to larger stocks. Value-weighting under-emphasizes the returns on the small stocks, which biases the tests against finding an accrual quality premium. When I replicate the analyses using value-weighted portfolio returns, the premiums on *AQ Factor* are smaller and not statistically significant.

TABLE 5

Alternative Time-Series Return Decomposition: Unconditional Expected Returns

Panel A: Descriptive Statistics

Variable	Mean	1%	5%	Lower Quartile	Median	Upper Quartile	95%	99%
Total Excess Return	0.99	−31.73	−18.52	−6.18	−0.25	6.66	22.88	46.84
Firm-Specific Earnings Response Model								
r^{ER}	0.59	−2.78	−1.11	0.06	0.57	1.12	2.30	3.91
r^{CF}	0.40	−5.53	−1.92	−0.11	0.18	0.85	3.20	6.88
r^{RES}	0.00	−32.84	−19.52	−7.10	−0.92	5.70	21.68	45.09

Panel B: Return Decomposition by Decile of *DD Measure*

			Firm-Specific Earnings Response Model		
Decile of *DD MEASURE*	*SURP*	Total Excess Return	r^{CF}	r^{E}	r^{RES}
1	0.051	0.88	0.50	0.36	0.02
2	0.040	0.87	0.52	0.40	−0.05
3	0.043	0.88	0.49	0.46	−0.07
4	0.027	0.91	0.44	0.50	−0.03
5	0.020	0.97	0.45	0.52	0.00
6	0.010	1.02	0.41	0.57	0.04
7	0.000	1.02	0.39	0.61	0.02
8	−0.012	1.04	0.34	0.69	0.01
9	−0.022	1.09	0.29	0.83	−0.02
10	−0.037	1.25	0.18	0.97	0.09
10 - 1	−0.088	0.37	−0.32	0.61	0.07
p-value	(0.00)	(0.29)	(0.00)	(0.00)	(0.98)

Panel C: Characteristic-Based Firm-Level Pooled Regressions

	Intercept	*RDD*	*BETA*	*MKTV*	*BMRATIO*	Adj. R^2
Excess Returns						
Mean Coefficient	1.61	−0.01	0.04	−0.15	0.31	1.30%
p-value	(0.00)	(0.54)	(0.85)	(0.00)	(0.00)	
Unconditional Expected Returns: r^{ER} from Firm-Specific Earnings Response Model						
Mean Coefficient	0.04	0.04	0.20	−0.03	−0.10	4.50%
p-value	(0.49)	(0.00)	(0.00)	(0.00)	(0.00)	

Panel D: Factor-Loading-Based Portfolio-Level Pooled Regressions

	Independent Variables					
	Intercept	$\hat{b}_{AQ}$	$\hat{b}_{MktRf}$	$\hat{b}_{SMB}$	$\hat{b}_{HML}$	Adj. R^2
25 B/M and Size Portfolios						
Excess Returns						
Regression Coefficient	1.53	0.18	−1.09	−0.09	0.63	6.94%
p-value	(0.00)	(0.32)	(0.00)	(0.60)	(0.00)	

(continued on next page)

TABLE 5 (continued)

	Independent Variables					
	Intercept	$\hat{b}_{AQ}$	$\hat{b}_{MktRf}$	$\hat{b}_{SMB}$	$\hat{b}_{HML}$	**Adj. R^2**
Unconditional Expected Returns: r^{ER} from Firm-Specific Earnings Response Model						
Regression Coefficient	0.13	0.30	0.27	0.06	−0.23	48.26%
p-value	(0.48)	(0.00)	(0.04)	(0.25)	(0.00)	
100 *DD Measure* Portfolios						
Excess Returns						
Regression Coefficient	0.79	0.20	−0.10	0.11	0.04	6.53%
p-value	(0.04)	(0.28)	(0.78)	(0.71)	(0.89)	
Unconditional Expected Returns: r^{ER} from Firm-Specific Earnings Response Model						
Regression Coefficient	0.34	0.35	−0.13	0.20	−0.02	35.75%
p-value	(0.00)	(0.00)	(0.21)	(0.00)	(0.67)	
64 B/M, Size, and *DD Measure* Portfolios						
Excess Returns						
Regression Coefficient	1.16	0.19	−0.75	0.05	0.53	5.66%
p-value	(0.00)	(0.34)	(0.01)	(0.83)	(0.01)	
Unconditional Expected Returns: r^{ER} from Firm-Specific Earnings Response Model						
Regression Coefficient	−0.14	0.34	0.50	0.11	−0.22	23.95%
p-value	(0.18)	(0.00)	(0.00)	(0.01)	(0.00)	

The table reports the results of alternative firm-specific return decomposition and asset-pricing tests based on unconditional expected returns from this decomposition for a sample of 75,026 observations over the 1970 to 2006 period.

Panel A contains descriptive statistics for returns decomposed using time-series regressions of excess returns on earnings surprises (*SURP*). The fitted values from these regressions represent cash flow shocks ($r^{CF}_{i,t+1}$), and the intercepts represent unconditional expected returns ($r^{ER}_{i,t+1}$). Residual returns ($r^{RES}_{i,t+1}$) are reported separately. The regressions are based on at least 72 monthly returns.

Panel B reports means of earnings surprises (*SURP*), total excess returns, cash flow shocks ($r^{CF}_{i,t+1}$), unconditional expected returns ($r^{ER}_{i,t+1}$), and residual returns ($r^{RES}_{i,t+1}$) by *DD Measure* decile. The average differences between earnings surprises and returns in the bottom and top deciles of accrual quality are reported in the row "10 − 1." The two-sided p-values for the return (earnings surprise) differences are computed using the Fama-MacBeth approach with a Newey-West correction with six lags.

Panel C contains coefficient estimates from the pooled regressions:

$$(r^{NCF}_{i,t+1}) = \text{Intercept}_t + q_t RDD_{it} + \text{Controls} + \varepsilon_{it},$$

where ($r^{NCF}_{i,t+1}$) denotes the realized return excluding cash flow shocks for stock i and month t+1; RDD_{it} is the decile rank based on *DD Measure*. All other variables are as defined in Table 1.

Panel D reports regression coefficients from the pooled regressions based on the portfolio excess returns excluding cash flow shocks and the full-period factor betas:

$$\overline{r^{ER}_i} = \text{Intercept} + \hat{b}^{AQ}_i \lambda^{AQ} + \hat{b}^{MktRf}_i \lambda^{MktRf} + \hat{b}^{SMB}_i \lambda^{SMB} + \hat{b}^{HML}_i \lambda^{HML} + \varepsilon_{it},$$

where $\overline{r^{ER}_i}$ is the average excess return excluding cash flow shocks on portfolio i calculated using 444 monthly portfolio returns, and $\hat{b}^{AQ}_i$, $\hat{b}^{MktRf}_i$, $\hat{b}^{SMB}_i$, and $\hat{b}^{HML}_i$ are full-period factor betas estimated by regressing excess portfolio returns on factor returns.

The panel partitions refer to three types of portfolios: (1) "25 B/M and Size Portfolios" are based on quintiles of the book-to-market ratio and size; (2) "100 *DD Measure* Portfolios" are based on percentiles of *DD Measure*; and (3) "64 B/M, Size, and *DD Measure* Portfolios" are based on quartiles of the book-to-market ratio, size, and *DD Measure*.

All returns are reported in percentages. Details of earnings surprises (*SURP*) estimation are in Appendix A.

Each regression is based on a pooled sample of firm-specific (portfolio-level) returns and includes (untabulated) time fixed effects. p-values reported in parentheses are based on standard errors clustered by firm and year (Petersen 2009).

Measure decile. The difference is both statistically and economically significant, amounting approximately to 7.32 percent per annum.

Panels C and D of Table 5 report the results of asset-pricing tests based on unconditional expected returns. Because each firm in the sample has a single expected return estimate, traditional asset-pricing tests that rely on the assumption of zero time-series correlation in returns can no longer be used. Instead, I estimate pooled regressions of returns on risk characteristics or factor loadings with time fixed effects and standard errors double-clustered by firm and year.

The premiums on excess returns unadjusted for cash flow news, reported in the first partition of Panels C and D, are not statistically significant in any characteristic- or factor-loading based tests. In contrast, the results based on unconditional expected returns unambiguously suggest a positive and statistically significant premium for accrual quality after controlling for conventional risk characteristics and factors. The coefficient on *RDD* in Panel C is 0.04, which corresponds to the difference in returns between the highest and the lowest deciles of *DD Measure* of 4.3 percent per annum (0.04 percent × 9 × 12). The estimates of the *AQ Factor* premium in Panel D range from 0.30 percent to 0.35 percent monthly (3.6 percent to 4.2 percent on annualized basis). Overall, the results from the alternative return decomposition are consistent with accrual quality being a priced risk factor.

Discount Rate Shocks

The proposed methodology does not control for the second component of unexpected returns, the discount rate shocks arising due to revision in the expectation of future discount rates. To the extent that discount rate shocks are positively correlated with *DD Measure*, the results in the paper are biased in favor of finding a significant accrual quality premium. To provide empirical evidence on the direction of discount rate shocks, I estimate the changes in three commonly used risk measures—the book-to-market ratio, Altman's (1968) Z-Score, and CAPM beta—by *DD Measure* decile. The changes are estimated over the same period as the realized returns (within one year of estimating *DD Measure*).

The results in Table 6 show that firms in the highest *DD Measure* decile with the poorest accrual quality on average become relatively more risky in the immediate future compared to firms in the lowest *DD Measure* decile with the best accrual quality. The book-to-market ratio (CAPM beta) increases 137 (125) percentage points more for firms with the worst accrual quality in Decile 10 compared to best accrual quality firms in Decile 1. The Altman Z-score of the firms with the worst accrual quality in Decile 10 (with the best accrual quality in Decile 1) decreases (increases) on average by 18.7 percent (1 percent), indicating an increased (decreased) probability of bankruptcy. Overall, the empirical evidence suggests that, if anything, firms with poor accrual quality become relatively more risky over time, which is consistent with discount rate shocks being negatively correlated with *DD Measure* and which biases the tests against finding a positive accrual quality premium.

Analyst Forecast Based Earnings Surprise

The earnings surprise measure used in the main analyses is based on a simple time-series model of earnings and relies on single-period earnings news. In this section, I verify that the results are robust to using earnings surprises based on analysts' forecasts pertaining to several consecutive fiscal years. In the spirit of Easton and Monahan (2005), I use realized errors in current-year forecasts and revisions in forecasts for one-year- and two-years-ahead earnings to capture earnings news. The details of variable estimation are in Appendix A. I perform cross-sectional return decomposition by regressing monthly excess returns on earnings surprises and forecast revisions, with a nonlinear earnings response model estimated within forecast-revision quintiles.

The untabulated results from the characteristic- and factor-loading-based tests are similar to those reported in Tables 3 and 4. As a characteristic, accrual quality commands a significant premium of 0.07 percent (0.05 percent) per month based on the linear (nonlinear) return

TABLE 6

Risk Proxies by *DD Measure* Decile

Decile of *DD MEASURE*	*BETA*	*BMRATIO*	*ALTM*	*%ΔBETA*	*%ΔBMRATIO*	*%ΔALTM*
1	0.783	0.884	2.262	0.227	0.023	0.010
2	0.948	0.843	2.756	0.195	0.055	−0.007
3	1.003	0.857	2.862	0.219	0.054	−0.023
4	1.044	0.889	2.879	0.235	0.059	−0.023
5	1.080	0.901	2.887	0.211	0.073	−0.039
6	1.117	0.913	2.862	0.254	0.081	−0.039
7	1.140	0.932	2.774	0.252	0.104	−0.038
8	1.175	0.932	2.596	0.254	0.107	−0.058
9	1.226	0.885	2.281	0.242	0.121	−0.093
10	1.272	0.771	1.672	0.352	0.160	−0.187
10 − 1	0.489	−0.113	−0.590	0.125	0.137	−0.197
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

The table reports average proxies for the level of risk and the change in risk for a sample of 92,145 firm-year observations with fiscal years ending in 1970 to 2006.

The average differences between the returns in the bottom and top deciles of accrual quality are reported in the row "10 − 1." The two-sided p-values for these differences are computed using the Fama-MacBeth approach with a Newey-West correction with six lags.

Variable Definitions:

$BETA_t$ = CAPM beta estimated using five years of data ending the most recent December;

$BMRATIO_t$ = ratio of the book value of equity to the market value of equity, where book value is stockholders' book equity plus deferred taxes minus the book value of preferred stock, and the market value of equity is as previously defined;

$ALTM_t$ = Altman (1968) Z-score, which is inversely related to the probability of bankruptcy;

$\%\Delta BETA_{t+1}$ = percentage change in *BETA*, calculated as $(BETA_{t+1} - BETA_t)/|BETA_t|$;

$\%\Delta BMRATIO_{t+1}$ = percentage change in *BMRATIO*, calculated as $(BMRATIO_{t+1} - BMRATIO_t)/|BMRATIO_t|$; and

$\%\Delta ALTM_{t+1}$ = percentage change in *ALTM*, calculated as $(ALTM_{t+1} - ALTM_t)/|ALTM_t|$.

decomposition before controlling for the market value of equity. The significance disappears after controlling for the market value of equity. The premium on *AQ Factor* is statistically significant after controlling for the three Fama-French factors and ranges from 0.51 percent to 1.71 percent per month depending on the return decomposition model and portfolio partitioning.[27]

Additional Robustness Checks

The analyses reported in Section IV are based on the *DD Measure* that is lagged one year relative to the measure used in CGV. The results are robust to using the non-lagged *DD Measure*.

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[27] The analysis in this section is subject to the sample-selection bias due to constraints imposed by I/B/E/S coverage availability (the sample starts in 1981 and contains only 34,929 firm-years). Importantly, *AQ Factor* commands a statistically significant (although at least 30 percent lower) premium within the I/B/E/S sample even before controlling for cash flow shocks in two out of three portfolio partitions from Table 4. Performing firm-specific return decomposition within the I/B/E/S sample would further reduce the sample to 28,393 firm-years, with attrition especially pronounced in the two top *DD Measure* deciles, where only 18 percent of observations from the main sample are retained. Such extreme attrition reduces the power of the tests and complicates making inferences. Accordingly, I do not perform firm-specific decomposition in this section.

The premiums on *DD Measure* and on *AQ Factor* remain similar in magnitude and statistical significance across all test specifications.

As in CGV, the characteristic-based asset-pricing tests (reported in Table 3) are based on decile ranks of *DD Measure*, which increases the statistical power of the tests by mitigating the influence of errors in the estimated *DD Measure*. I verify that the characteristic-based results are robust to using the raw *DD Measure*, which represents a more stringent test with potentially lower power. The untabulated results are qualitatively similar to those reported in Table 3. The premiums on *DD Measure* are significant after controlling for other risk characteristics only when cash flow shocks are excluded using a firm-specific earnings response model.

CGV also perform the two-stage analysis using individual stock returns and factor loadings. I replicate the two-stage tests for the stocks with at least 30 non-missing monthly returns. The untabulated results based on returns unadjusted for cash flow news show that the premium on the individual *AQ Factor* beta is not statistically significant, similar to CGV results. After excluding cash flow shocks estimated using the cross-sectional linear (nonlinear) earnings response model, the premium on *AQ Factor* is equal to 0.25 (0.40) with a p-value of 0.13 (0.06). The premiums are similar in magnitude but no longer statistically significant when the firm-specific return decomposition is used to estimate cash flow shocks. However, interpretation of these results is complicated by the likely lower power of the tests based on individual stock factor loadings.

In addition, the results are robust to including a decile rank of signed accruals in characteristic-based regressions, suggesting that the results are distinct from the Sloan (1996) accrual anomaly. The results from these regressions are also robust to including a decile rank of the operating cash flow variance, which is a measure of operating risk (Liu and Wysocki 2007), as an additional control variable.

VI. METHODOLOGY LIMITATIONS

The proposed return decomposition methodology is subject to several limitations. First, it does not control for discount rate shocks, i.e., the portion of returns due to changes in the expectation of future discount rates. Although the analysis performed in Section V suggests that omitting discount rate shocks is unlikely to bias accrual quality premium estimates upward, discount rate shocks may represent a correlated omitted variable in other settings. Not controlling for discount rate shocks also limits the interpretation of average magnitudes of returns excluding cash flow shocks. Although discount rate shocks explain a small portion of variation in returns on individual stocks, the corresponding portion of returns is much higher for portfolios of stocks (Vuolteenaho 2002). As a consequence, after excluding cash flow shocks, the *average* return for a sample of firms may, to a significant extent, consist of discount rate news.

Second, under the earnings response coefficients framework, where discount rates are considered constant, the relation between cash flow shocks and realized returns is linear. However, in general, returns and cash flow shocks are not linearly related. Incorporating stochastic discount rate shocks in the earnings response coefficient framework may be a fruitful path for future research.

Third, in the presence of information signals about future payoffs arriving from outside the accounting system, the relation between returns and one-period earnings surprises is positive only under restrictive assumptions, and is generally nonlinear (Antle et al. 1994). Although nonlinear and firm-specific earnings response estimation addresses this concern to some extent, it does not solve the problem completely. Overall, it is impossible to fully address Antle et al.'s (1994) criticism within the proposed methodology, since at a minimum it would require incorporating signals from outside the accounting system.

VII. CONCLUSIONS

Using a simple methodology that follows from the earnings response coefficient framework, I estimate the cash flow shock portion of realized returns and document that, over the past 37 years, poor accrual quality firms, i.e., firms with high *DD Measure*, systematically experienced more negative cash flow shocks compared to good accrual quality firms, i.e., firms with low *DD Measure*. When asset-pricing tests use realized returns that exclude cash flow shocks estimated using the more powerful return decomposition methods, the low accrual quality stocks (stocks with higher accrual quality factor betas) are associated with significantly higher expected returns compared to the high accrual quality stocks (stocks with lower accrual quality factor betas).

Evidence that the accrual quality measure based on the Dechow and Dichev (2002) model is priced by the market is important due to both the theoretical appeal of the model and its widespread use in accounting research. The model is especially appealing in the asset-pricing context because it captures an important feature of accrual and earnings quality, errors in accrual estimates, and subsequent reversals in these errors. Such errors are related to the concept of accounting information precision, which maps into theoretical work on the link between the cost of capital and information quality (e.g., Lambert et al. 2007).

The paper also has implications for research that uses realized returns to proxy for the cost of equity, and suggests a simple method to identify correlations with future cash flow shocks and to control for the effects of such shocks. The results underscore the importance of excluding cash flow shocks to avoid biased risk premium estimates.

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APPENDIX A
EARNINGS SURPRISE PROXIES

Earnings Surprises Based on Time-Series Model

The earnings surprise variable, *SURP*, is calculated as actual earnings minus expected earnings, that is, $EARN_{it+1} - E_t(EARN_{it+1})$. Expected earnings are estimated from a simple statistical model that is based on the assumption that annual earnings follow a first-order autoregressive process (AR1):

$$EARN_{it+1} = \beta_0 + \beta_1 EARN_{it} + \varepsilon_{it+1},$$

where $EARN_{it}$ denotes earnings before extraordinary items (Compustat #18) for firm *i* during fiscal year *t*, scaled by the book value of equity at the beginning of fiscal year *t* (Compustat #60).

Estimation of expected earnings is performed in two stages: (1) the predictive coefficients $\hat{\beta}_0$ and $\hat{\beta}_1$ are obtained by estimating the model cross-sectionally over the previous year (e.g., using $EARN_{it-1}$ and $EARN_{it}$); and (2) the earnings prediction is calculated by combining the estimated coefficients with variable values:

$$E_t(EARN_{it+1}) = \hat{\beta}_0 + \hat{\beta}_1 EARN_{it}.$$

For example, the 2005 earnings predicted at the end of 2004 are calculated using the values of predictive variables for fiscal 2004 and coefficients estimated by regressing earnings from fiscal 2004 on earnings from fiscal 2003. The expected earnings are then converted to an absolute basis by multiplying by the book value of equity. *SURP* for each month is scaled by the beginning-of-month market value of equity.

Earnings Surprises Based on Analysts' Forecasts

In the spirit of Easton and Monahan (2005), I use three variables based on analysts' forecasts to capture earnings news released during fiscal year *t*+1.

(1) Forecast error on the EPS_t forecast made at the end of fiscal year *t*. It captures a revision in expectations of future earnings that occurs in year *t*+1 due to announcement of realized earnings for fiscal year *t*.
(2–3) Revisions in one- and two-year-ahead earnings forecasts ($FREV_{it+1}$ and $FREV_{it+2}$), calculated as:

$$FREV_{it+n} = FEPS_{it+1,t+n} - FEPS_{it,t+n},$$

where: $FEPS_{it,t+n}$ denotes the *n*-years-ahead consensus EPS forecast from I/B/E/S at the end of fiscal year *t*.

The forecast errors and forecast revisions for each month are scaled by the beginning-of-month stock price.

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BOOK REVIEWS

Stephen A. Zeff, Editor

Editor's note: Two copies of books for review should be sent to the Book Review Editor: Stephen A. Zeff, Rice University, Jesse H. Jones Graduate School of Business, 6100 Main St., Houston, TX 77005. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

WILLIAM H. BEAVER, MARIA CORREIA, and MAUREEN F. MCNICHOLS, *Financial Statement Analysis and the Prediction of Financial Distress*, Foundations and Trends® in Accounting (Hanover, MA: now Publishers Inc., 2011, ISBN: 978-1-60198-424-1, vol. 5, no. 2, pp. x, 79).

There is no doubt that predicting and managing corporate financial distress have become increasingly important parts of doing business and making investing and lending decisions, not only in the U.S., but also throughout the world. Altman and Hotchkiss (2006) note that, as businesses and bankruptcy laws have both become more complicated, the need for professionals with specialized skills has grown as has the demand for academic research into the prediction of financial distress. *Financial Statement Analysis and the Prediction of Financial Distress*, by William Beaver, Maria Correia, and Maureen McNichols (hereafter, BCM), provides an exceptionally well-written overview of the three main aspects of the academic financial distress prediction literature: the dependent and explanatory variables that different researchers have used, the statistical methods they have used, and the way in which they model financial distress. Although the monograph is written so that practitioners, students, instructors, and researchers will all find it very useful, it concludes with suggestions for future research and thus promises to be used by scholars for years to come. I also believe that some of the ideas in the monograph can be extended and used to shape research designs in the growing areas of fraud prediction and litigation risk estimation.

The monograph begins with a discussion of the somewhat ill-defined concept of financial distress and its frequently used counterpart, insolvency. This is a good discussion to have at the beginning, because the way in which empirical prediction models are structured and used depends on exactly what the researcher is trying to predict. BCM follow this discussion with an intuitive explanation of likelihood ratios and loss ratios and their interpretation. The discussion of loss ratios is particularly well-written and provides a clear picture of the complexity of a lender's decision-making process and how output from a prediction model can affect not only the decision to lend but also the structure of the loan itself.

BCM then begin their discussion of the role of financial statement analysis in bankruptcy prediction. They outline some of the limitations of accounting information (e.g., much of it is backward looking and not timely) and then provide a brief overview of early univariate analysis and the evolution of financial distress prediction models that rely on financial statement ratios, starting with the Index of Credit Strength introduced by Wall and Dunning in 1928. BCM note that this "model" never gained popularity, largely because of Wall and Dunning's arbitrary assignment of weights to the eight included ratios. Despite this, accounting-based models have continued to be developed without an explicit theory to guide their design, although BCM note that a number of researchers have used out-of-sample testing, in part to provide indirect evidence on the stability of the weights that their empirical models produced. BCM then describe and contrast the methodologies used in the currently most well-known, early financial-statement-based prediction models: the multiple regression approach and univariate analysis in Beaver (1965, 1966), the multiple discriminant analysis in Altman (1968), and the conditional logit analysis in Ohlson (1980). They then discuss the advantages of hazard analysis, a technique that Shumway (2001) introduced into the literature a decade ago. The section ends with helpful discussion and graphical descriptions of key financial statement ratios and what we have learned about their relation to financial distress through these models.

One of the early, and enduring, criticisms of financial-statement-based prediction models is that stock price and other market-based measures summarize a more comprehensive set of information than accounting ratios and therefore should be at least as useful in predicting financial distress. Further, academic research in finance (BCM mention Merton [1974] and Black and Scholes [1973]) provides a framework for estimating the probability of distress from the market prices of securities. In the next section of the monograph, BCM describe some of the market-based variables that have been used in prior research, but also note that, although using market-based variables is intuitively appealing, it is "nontrivial" to extract a useful probability of bankruptcy or distress from these measures, particularly in the light of the evidence that prices are not semi-strong efficient as once believed.

In order to compare and contrast the usefulness of accounting-based versus market-based prediction models, BCM next assess the predictive ability of an accounting-based model, a market-based model, and a model that contains both accounting- and market-based variables. The choice of accounting variables was guided by recent research, as was the decision to used hazard analysis as the estimation technique for this comparison. In this part of the monograph, the reader learns more about the "nuts and bolts" of current state-of-the-art prediction models: the variables to include, how those variables are measured, and how the models compare, drawing on the analysis in recent work by BCM and a published paper, Beaver et al. (2005). The careful and clear comparisons in this section are likely to be useful to a broad range of readers, but will certainly be helpful to researchers who are interested in how the various models perform in a relative sense and how methodological choices affect their performance.

Before providing directions for future research, BCM end their overview of the different prediction models by describing a number of issues that affect financial distress prediction, some of which are particularly interesting and deserve a closer look. For example, does the exercise of discretion both within and outside of generally accepted accounting principles affect the relative usefulness of accounting-based versus market--based measures? Similarly, does the presence of unrecognized intangibles affect predictive ability? BCM pose these questions as well as others but do not stop with raising the questions. They also summarize currently available evidence on the effect of certain accounting characteristics on the models' performance and then move on to other alternative research design issues.

BCM end the monograph with a number of suggestions for future research, many of which are likely to move the bankruptcy prediction literature in accounting forward and have an impact on other related research streams. For example, how do financial ratios based on IFRS affect the predictive ability of the popular models? How do bankruptcy prediction models perform relative to auditors' going concern judgments in the post-SOX environment? I found this section of the monograph to be particularly thought-provoking.

Overall, BCM have produced a valuable resource for anyone looking to learn more about the prediction of financial distress and its evolution. Their monograph is written in a way that makes it accessible to a broad range of readers, from doctoral students and researchers who are interested in the evolution of prediction models as well as the current state of the art to regulators and practitioners who wish to learn more about forecasting financial distress in corporations. It is likely to be a resource that many of us will rely on for years to come.

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TIM BÜTHE and WALTER MATTLI, *The New Global Rulers: The Privatization of Regulation in the World Economy* (Princeton, NJ: Princeton University Press, 2011, ISBN 978-0-691-14479-5, pp. xvii, 301).

What drives global standard setting? Who are the new global rulers, and what constitutes their power? Can individuals or firms affected by global rules influence the process of global rule-making and effectively articulate their interest? Under what circumstances does global rule-making reflect the needs of domestic constituents? Tim Büthe and Walter Mattli, two political scientists, pose these questions in their monograph, which broaches the issue of global rule-making in a broad sense. However, their research concentrates particularly on two issues: accounting standard setting by the International Accounting Standards Board (IASB) and product standard setting by the International Organization for Standardization and the International Electrotechnical Commission (ISO/IEC).

IASB and ISO/IEC rule-making share a very similar characteristic. They are executed by what the authors call delegated, private focal institutions. Such institutions are regulators that have neither governmental status nor international competitors, due to an exclusive standard-setting authority. Rule-making is, in other words, focused on single, privately organized regulators. Also, the processes of setting rules or standards are very similar. It is common to justify this kind of regulation because of the nature of the respective standards. Those rules regulate rather technical issues and therefore require specific expertise. It is claimed that such standards could be effectively provided only by privately organized authorities. However, the authors claim, and empirically support, that the processes of setting the standards are far from being just "technical." Their research evidence shows that these processes are as political as they are in other areas of regulation.

It is not the aim of the authors to criticize this inherent political nature of global standard setting by private focal institutions. They do not intend to pose legitimacy concerns or to evaluate the efficacy of the processes and the way global ruling is delegated. Rather, the objective of their research is to provide a theoretical framework for this specific way of global rule-making in order to explain how it works and to identify the critical factors for successfully influencing the process of standards development.

The authors test their framework with a large set of empirical data collected from two multi-country, multi-industry business surveys in Europe and in the U.S., supplemented by a large number of interviews.

They therefore provide the most comprehensive and systematic empirical evaluation of global standard setting so far.

The book has nine chapters and three appendices. Chapter 1 describes the rise of private regulation in the world economy and provides a short overview of the research done by the authors. Chapter 2 places standard setting by private focal institutions into context and describes a typology of global regulation, thereby identifying four principal types of rule-making: competing standard setting by public bodies, competing standard setting by private institutions, non-market-based focal rule-making, and market-based focal rule-making. The authors make clear that both IASB and ISO/IEC standard setting belong to the last category.

In the third chapter, the authors introduce what they call the concept of institutional complementarity. The key idea of the concept is that successful and timely influence of market-based focal rule-making crucially depends on how the institutional structure of domestic standard setting complies with the international one. The more the domestic structure is focused on a single standard setter that has established processes that are organized similarly to the focal international institution, the more likely that domestic constituents ("stakeholders") will exert a successful influence. A focal domestic institution that clearly represents the broad opinion of a country's individuals affected by the international standards will be perceived as the key domestic "player" by the global ruler. When new international standards are to be developed by means of an international due process, the opinion of such an institution will be regarded as highly relevant and uncontested by domestic constituents. The authors characterize such a domestic system as hierarchical with a peak-level national standard-setting body and ascribe high institutional complementarity with the global ruler to the domestic system.

However, domestic systems could also be characterized as rather fragmented, with different institutions responsible for domestic standards and representing different constituents. All of the institutions could take part in the international due process. In that case, the global ruler could be confronted with contested national opinions on the international standard that has to be developed. In such a domestic system, which the authors characterize as non-hierarchical with low institutional complementarity, it is less likely that domestic constituents affected by international standards will successfully influence the international standard setting process.

The authors identify two factors that seem to be tied to institutional complementarity: (1) early access to information and therefore an early participation in the process itself, and (2) effective representation of domestic interests by a strong and uncontested "voice," endowed with sufficient economic resources and structured similarly, as well as providing expertise akin to the global ruler. Indeed, the way that domestic institutions are organized governs whether conflicts between constituents are projected to the international level. For competing domestic institutions, early information could be regarded as exclusive *vis-à-vis* contestants. Institutions might keep this information solely to serve their specific constituents and therefore impede quick information dissemination within the country. If information comes late, then it is difficult to take part in the due process at an early stage. The authors claim, and they prove with anecdotal evidence, that as soon as the global ruler decides on the key principles of a standard—e.g., if an accounting standard reaches the status of an exposure draft—it is difficult, if not impossible, to revise such fundamental decisions. Eventually, a strong domestic standard setter is often regarded as a partner in planning projects, agenda-setting, or even standards development due to the limited recourses of global rulers.

One might claim that such a dichotomy of domestic regulation seems too crude to characterize the variety of regulation systems worldwide. However, the authors concentrate their empirical research in later chapters in such a way as to confirm that this categorization is suitable to describe what constitutes power in global ruling.

In Chapters 4 and 5, the authors describe how their model fits international accounting standard setting, while Chapters 6 and 7 cover global regulation of product markets.

Chapters 4 and 6 start with a brief description of the history of global accounting governance and global market governance through product standards, respectively. In each of the chapters, characterizations of the standard-setting processes follow, indicating the respective importance of early and uncontested participation of national responsible institutions. Though brief, the descriptions are interesting because they show the similarity of global-ruling processes by privately organized focal institutions.

Extremely perceptive are the authors' following characterizations of domestic systems. In accounting regulation, the U.S. domestic system fits tightly to the model of a hierarchical structure, with high complementarity to the standard-setting process of the IASB. The FASB is indeed a strong focal domestic institution, with resources and technical expertise that are the envy of other domestic standard setters. The IASB and FASB work closely together when new standards are to be developed, a fact institutionalized through the Norwalk Agreement, which aims to converge the IFRS and U.S. GAAP. Opposed to that are nearly all of the European systems. Even the British system, whose financial markets are similarly structured to those in the U.S., is rather non-hierarchical, having a sharp fragmentation of institutions competing for members and status, and different accounting philosophies and priorities, reflecting different constituents. Accounting stakeholders in the U.K. seem to approach the IASB individually or through six different professional bodies and not with "one" voice.

The authors also briefly describe the German and the French systems, which differ even more from the U.S. system. Although the harmonization of accounting has been on the European agenda for decades, its institutionalization is far from being a fact. As the authors show, a transnational accounting standard-setting structure does not really exist in Europe. Fragmentation in Europe instead leads to the European endorsement mechanism for newly released IFRS. Compared to the FASB, Europe remains divided on accounting issues. European institutions like the European Financial Reporting Advisory Group (EFRAG) are contested by national standard setters; so Europeans have difficulty in speaking with one voice. Institutional complementarity in Europe is, in other words, low compared to that in the U.S.

Interestingly, the story is reversed for product standard setting. The description of the European and the U.S. systems in Chapter 6 shows a high fragmentation of domestic product standard regulation in the U.S., compared to a rather hierarchical and focused institutional setting in Europe. The European system is characterized by an effective mechanism for broad preference aggregation at the transnational, as well as at the domestic, level. Coordination takes place under the umbrella of a single domestic institution, supplemented by private sector organizations all over Europe that reinforce these structures. The European system is hierarchical and strongly complementary to the structures of ISO/IEC. The U.S. system lacks this complementarity, because a variety of domestic institutions compete nationwide for constituents. Market globalization amplifies the need for U.S. firms to comply with international product standards in order to keep their market shares. However, the institutional fragmentation impedes U.S. firms from effectively taking part in the process of international standards development. Due to the contesting domestic institutions in the U.S., it is difficult for U.S. constituents to speak with a single voice.

In Chapters 5 and 7, the authors explicate two large and comprehensive survey designs for global accounting standard setting and product standard setting, respectively. The question they pose in Chapter 5 is whether Europeans are aware of their institutional disadvantage in global accounting regulation and whether institutional fragmentation is the reason. In essence, accounting experts, that is, financial executives in European and U.S. firms, are asked about their satisfaction with the IASB and their involvement in the standard setting process. The survey is concentrated on the preparers of financial statements, not on the users. The design is similar for product markets (Chapter 7), because the survey here concentrates on technical experts in European as well as in U.S. firms. However, while such a design makes perfect sense for product markets, one might ask why other key constituents affected by accounting standards, e.g., financial analysts or other users, are not part of the survey. The authors may justify a concentration on preparers by saying that users, although affected by the rules, traditionally do not play a significant fole in the standard-setting process. Nonetheless, it seems that the authors' intention was to keep the two surveys closely comparable and therefore confined both sets of studies to company experts.

The research approach in Chapter 5 (Chapter 7) is to identify whether U.S. (European) firms are more in favor of the IASB rule-making (ISO/IEC rule-making) process than their European (U.S.) counterparts. The authors start with striking anecdotes on accounting and product-standard regulations, respectively, which indicate that their theory on institutional complementarity as the main driver for successful involvement in the regulation processes might hold empirically. In their systematic analyses, the authors use four maximum likelihood regression models. The dependent variable reflects the respective overall satisfaction of the surveyed experts with their involvement in the IASB or ISO/IEC due processes. Independent variables are several factors the authors presume to significantly affect a firm's perceived success to influence the standards-setting processes.

In a first comparison, the authors find that overall satisfaction with IASB rule-making is significantly higher for U.S. firms than for their European counterparts (Chapter 5). This result is related to survey questions on the effectiveness of the IASB in achieving specific objectives (e.g., transparency, capital market stability, comparability) and on its governance (e.g., accountability of the IASB, timeliness of up-dating, accessibility of

IFRS, due process in general). The perceived success of influencing IASB standard setting (the dependent variable "power and influence") is regressed on six explanatory control variables derived from corresponding survey questions (firm size, direct contacts with expert groups within the IASB, attempted influence via comment letters, timing of information received, frequency of influence attempts, and attempted influence via government). Testing the four regression models, the authors find that all variables have a strong explanatory power. However, in order to test their theory, the authors integrate a dummy variable with 1 = U.S. firm and 0 = other, which enhances the significance of each model considerably. The authors also document significant increases in the perceived overall influence on the IASB standard setting if the dummy is 1, that is, for U.S. firms. In other words, constituents in the U.S., a country with a domestic standard-setting system that highly complements the IFRS system, feel better served by the IASB than constituents in Europe, where complementarity of domestic standard setting with the IFRS system is low.

The research approach on the ISO/IEC rule-making (Chapter 7) is very similar to that on the IASB regulation. As expected, U.S. firms' overall satisfaction with global standards on product markets is significantly lower. The authors conduct a regression analysis similar to that on the IASB. The dependent variable is the perceived influence on international product standards, while independent variables are various control factors that the authors presume to affect the perceived influence on the rule-making of ISO/IEC. All factors have a significant influence in four alternative models. In an analogy to the study on accounting regulation, a dummy variable with 1 = European firm and 0 = other enhances the significance of the models, and increases the overall satisfaction with influence on international standards in product markets. The high complementarity of European product standard setting with ISO/IEC rule-making seems to be the driving factor. Interestingly, U.S. firms with subsidiaries in Europe show a higher overall satisfaction with international rule-making than those without. The authors attribute this finding to the fact that those firms are able to execute their influence via their European subsidiaries, that is, in a system that better complements international rule-making than the U.S.

In Chapter 8, the authors discuss their results and draw some important conclusions. Institutional complementarity seems to be crucial in transnational standard setting by focal institutions. Their comprehensive survey provides compelling evidence of their theory and invaluably enhances our understanding of international standard setting. As they claim in Chapter 1 (p. 10), the monograph is the result of eight years of extensive research on this highly relevant topic. However, the authors are also careful in their conclusions. They stand afar from drawing any normative claims. Neither do they regard high complementarity systems as superior in every sense, nor do they overlook legitimacy concerns with regard to international standard setting by focal institutions. The authors can nonetheless take credit for having developed a convincing theory on the main drivers of power within this specific and widespread phenomenon of global ruling.

The book is, without a doubt, highly recommended. While it is primarily intended for scholars, it provides very interesting insights for anyone interested in how global standard setting works, in its historical, political, and socio-economic background, and in its significance for global governance in general. The conclusions drawn by the authors contribute to the literature in political science, sociology, law, economics, and business science. Accounting scholars should be particularly interested because a major focus of the research is standard setting by the IASB.

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GARY PREVITS, PETER WALTON, and PETER WOLNIZER (editors), *A Global History of Accounting, Financial Reporting and Public Policy: Americas* (Bingley, U.K.: Emerald Group Publishing Limited, 2011, ISBN 978-0-85724-811-4, pp. xi, 160).

Each of the five chapters of this book covers the history of accounting in a major Western Hemisphere country: Argentina, Brazil, Canada, Mexico, the U.S., and Canada. These nations represent a well-balanced cultural portfolio of British, Portuguese, and Spanish origin, which provides an interesting backdrop for examining the evolution of accounting in the hemisphere. With the exception of Canada and Mexico, the chapters are authored by accounting faculty at major universities in the selected countries. Chapter 5 is dedicated to the United States and is jointly written by Stephen Moehrle and Jennifer Reynolds-Moehrle, both accounting professors at the University of Missouri–St. Louis. Given the space limitations for this review and the familiarity that most readers of *The Accounting Review* have with U.S. accounting, I will limit my comments on this chapter to observing that it is an excellent synopsis of the history of U.S. accounting. It could be required reading for all accounting majors.

Chapter 1, "Argentina," written by Sandra Aquel and Mabel Mileti, accounting professors at the Universidad Nacional de Rosario in Argentina, begins by taking the reader back to the 15th century when there really was no national economy, little import/export activity, and no significant impulse for accounting development. Eventually, growing colonial trade with Europe in the late 18th century, and the emergence of conveniently located large cattle-raising ranches as well as the advent of the railroads in the 19th century led to the growth of international commerce, large diversified businesses, national legislation, a governmental budgetary system, a monetary system, and the increased use of contracts that obligated debtors to keep books in balance and report on them as requested by creditors.

Most of the chapter on Argentina is dedicated to reporting the history of the country's mechanisms for issuing "professional" accounting standards by the accounting profession within a legal system that afforded the profession wide latitude in its choices. There were also "legal" standards issued directly by the national and provincial governments, as well as "corporate" accounting standards issued by regulatory bodies. Various accounting associations existed since 1941; every few years they would hold an assembly in which standards may be adopted, but this system of holding meetings for the adoption of accounting standards eventually gave way to the formation of successive professional bodies specifically charged with that task. Accounting principles then had to be approved by provincial accounting associations as well as by those representing Buenos Aires, the country's very large capital city and predominant economic center. Achieving consensus across these different organizations was not always easy, an important issue that impeded progress and that the authors fail to mention in their discussion. Consequently, it was only in 2005 that a body of principles (a "technical agreement") was finally adopted countrywide by all of the professional organizations. Further, motivated by globalization and recognizing the advantages of international standards, the profession, led by the Argentinean Federation of Professional Councils in Economic Sciences (FACPCE), has now adopted IFRS, subject to modifications based on local needs.

By focusing primarily on the history of the mechanisms used in issuing "professional" accounting standards, the chapter on Argentina has the narrowest focus of all of the chapters. The authors could have discussed many other issues of interest, such as specific accounting principles and practices (particularly in dealing with inflation, which reached a rate of 20,000 percent in the early 1990s), foreign influences on Argentinean accounting, standards for professional practice, the social status of the profession, etc. Further, there is no discussion of the colonial accounting systems that were employed early on and for a long time by the representatives of the Spanish crown and institutions such as the Catholic Church and its Jesuit missionaries (see the "Mexico" chapter for this information). Also, while sometimes at odds because of the Perón dictatorship and more recently the Falkland Islands sovereignty conflict, Argentina and Britain have maintained a very close cultural and economic relationship for centuries following the Spanish colonial era. The long-standing Harrods department store in Buenos Aires, British ranches, clubs, and even the country's pre-eminence in British sports like football (soccer), rugby, tennis, and polo (consistent world champions) and many other venues and endeavors are evidence to this day of a British legacy firmly entrenched in Argentina's economy and culture. It would be unthinkable for the British not to influence local accounting standards and practices—yet there is no mention in the chapter of the doubtless important contribution that British managers and other foreign influences from large multinational businesses and accounting and auditing firms must have made to the development of Argentinean accounting. There is also no discussion of the professional formation and certification of accountants (for a long time trained primarily in economics rather than in accounting and still not having to pass a professional examination or meet continuing education requirements), their social status (also for a long time relegated to lowly bookkeepers), and the threat of legal liability (much less of a concern than in the U.S. or Canada).

Chapter 2, "Brazil," is written by L. Nelson Carvalho, a professor at the Universidade de São Paulo, and Álvaro A. Ricardino Filho, a professor at Pontificia Universidade Católica de São Paulo. It is the second longest chapter (24 pages of text versus, for example, 17 for "Argentina") and is broader in scope and richer in depth and detail than Chapter 1. The content again is classified into periods that affected accounting, although these periods are based primarily on the country's political developments rather than on the accounting profession's stages of development. Political periods discussed include the country's colonial period, its years as the seat of the Portuguese throne (when seeking refuge from an impending French invasion under Napoleon, the royal family of Portugal and 10,000 courtiers and sympathizers moved to Rio de Janeiro in 1808 and stayed there until 1821), its subsequent years as an independent kingdom from 1822 to 1889, and three phases as a republic, its current status. From its early stages, the country's economy was more diversified than Argentina's, because, in addition to cattle, it produced sugar, coffee, and minerals.

Throughout Brazil's first 300 years of history, accounting practices "had as their sole purpose to report taxes to the Portuguese crown, usually ranging from 10% to 20% of gross proceeds of any kind of revenue or production" (p. 26). Legislation adopted in 1860 for the first time required financial institutions to present financial statements, prepared according to prevailing standards, to shareholders and the government. The remainder of the chapter is a fairly detailed narrative about accounting practices and requirements, the development of accounting standards, the evolution of the accounting profession in response to political and economic events (including the significant impact of inflation in the early 1980s), and a discussion of Brazil's decision, which was overturned by a law in 2007, to allow financial accounting practices to differ from those used for tax purposes. That same law in 2007 mandated IFRS to be applied in the preparation of financial statements. Currently, the Committee on Accounting Pronouncements issues accounting standards and endorses IFRS, which have to be followed by listed companies as regulated by the country's Securities and Exchange Commission.

Chapter 3, dedicated to Canada, is written by Chester H. Brearey, who teaches at Siena College in New York State. Four time periods are identified: early accountability (1850–1890), accountancy (1891–1920), consolidation and U.S. influence (1921–1951), and delegation of standard setting (1960–2010). Britain and the United States have significantly influenced the development of Canadian standards, although at times the latter has been in a leading position. Somewhat similar to the Argentinean experience, the development of the railroad industry is mentioned as an important nation-building factor, which likely fostered the accounting development of these large countries. Development of accounting in Canada has evolved without major intervention by the country's federal government, which has left the "virtually unrestricted government delegation of standard setting authority to the profession (Canadian Institute of Chartered Accountants [CICA])" (p. 51). Over time, the CICA's accounting and auditing standards-issuing functions were delegated to a series of committees, presumably searching for wider acceptability of standards. Eventually, the accounting principles-issuing function was passed on to the Accounting Standards Board. In 2006, this Board opted for the convergence of Canadian principles with IFRS, which are now required for public companies.

Chapter 4 concerns Mexico and is written by Ikseon Suh and Sandra Minaburo. Professor Suh teaches at Marquette University in the United States and Professor Minaburo is a faculty member at the Instituto Tecnológico Autónomo de México. The chapter is divided into five stages: (1) pre-Spanish conquest, (2) post-Spanish conquest, (3) transitional era, (4) era of the Mexican Institute of Public Accountants (Instituto Mexicano de Contadores Públicos, or IMCP), and (5) the convergence era.

The historical perspective reflected in this chapter stretches the furthest into the past. This is the most interesting chapter from an anthropological point of view, even though the space dedicated to the pre-Spanish period is only a little more than one page. Aztec currencies were cocoa beans, cloth, shells, and powdered gold; "a sack of 8,000 cocoa beans represented the most frequently used unit of measurement, known as '*xiquipilli*'" (p. 80). Taxes were levied by the Aztec government on inhabitants of conquered cities in exchange for protection, and were collected and recorded by government representatives, or "*calpixques*," who then submitted them to stewards in major cities who also had to keep records of the inflows and outflows. Examples of these early accounting records, though very rare, are kept in museums in Oxford, U.K. and Mexico City, Mexico.

The section on Mexico as a Spanish colony includes an excellent discussion of the accounting books, such as the *book for alms and favors* and the *book for tax collection*, kept by the Crown's representatives for a variety of economic and social functions that they performed. The colonial era was followed by the era of independence. Successive commercial codes were issued that eventually (in 1889) required merchants to maintain a *journal*, *ledger*, and a *book of inventories and balances*, as well as a series of other records. This period was followed by the era of the Mexican Institute, which in 1964 assumed the accounting rule-making authority for the country. An important item within this era is the discussion on inflation accounting. Finally, during the current era of convergence, the Consejo para la Investigación y Desarrollo de Normas de Información Financiera (CINIF), the Council for Research and Development of the Principles of Financial Information, has been given responsibility for the development and issuance of accounting standards. Beginning on January 1, 2012, Mexican public companies are preparing financial statements in conformity with IFRS under CINIF's guidance. Although the chapter does use relatively more space than others on the development of the accounting profession, it fails to mention that the Mexican equivalent of a U.S. CPA and a Canadian CA, the *contador público certificado* (CPC), was instituted only in 1988, with continuing education and other requirements. This and other changes resulted from Mexico's membership in NAFTA and subsequent international commitments, such as with the U.S.'s National Association of State Boards of Accountancy (NASBA).

Overall, it is apparent the authors were given considerable latitude in selecting the format for their contributions. This is both a weakness and strength, because, while it makes it a bit more difficult to sort through and compare the chapters' contents, which vary in depth and breadth, it adds freshness and uniqueness to each chapter. Nonetheless, the chapter on Argentina, in particular, could have been enriched with some additional content, as commented on above. Also, the reader should keep in mind that, until just a few years ago, the primary purposes of accountability in Latin America were reporting to owners and creditors, tax compliance, and stewardship and control, rather than providing information to what were until recently tiny local capital markets. Globalization and the need for external investors' financing have favored the adoption of IFRS by the three Latin countries, which, in turn, raises the status of the accounting profession. This effect is likely not as significant in the U.S. or Canada, where relatively larger capital markets and strong accounting professions have been in existence much longer. In conclusion, this is a well-written and well-edited, interesting book that may be used in international accounting classes. The book will also be an excellent addition to any professional library, and will be particularly useful to readers involved in accounting activities in the Western Hemisphere.

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GARY PREVITS, PETER WALTON, and PETER WOLNIZER (editors), *A Global History of Accounting, Financial Reporting and Public Policy: Europe* (Bingley, U.K.: Emerald Group Publishing, 2010, ISBN 978-0-85724-671-4, pp. xii, 302).

This volume of essays on ten European countries is the first in a series of four covering the history of accounting, financial reporting, and public policy in larger economies and developing economies. Later volumes will cover the Americas, Asia and Oceania, and Eurasia, Middle East, and Africa.

The goal of the editors of the series is to provide a series of historical source books for a readily converging world of accounting and financial reporting based on the premise in Walton (1995) that it is important to evaluate a country's accounting and financial reporting in the light of its national history. The ten European countries chosen for this first volume are Belgium, France, Germany, Italy, The Netherlands, Poland, Spain, Sweden, Switzerland, and the United Kingdom.

Each essay deals with a single European country and is written by one or two nationals of the relevant country. For the most part the authors structure their essays around developments in particular time periods. The essays by Claude Bocqueraz on France and Alicja Jaruga and Przemysław Kabalski on Poland are good examples of this approach. They also show the different influences on national accounting, including the impact of one country's accounting on another country's accounting.

Bocqueraz identifies three time periods in the history of accounting in France: the end of the seventeenth century to the Second World War, from 1942 to 1973, and from 1973 to the present day. Jaruga and Kabalski identify five periods in the history of accounting in Poland: the fifteenth and sixteenth centuries; the nineteenth and early twentieth century when Poland was partitioned between Austria, Prussia, and Russia; from 1919 to 1939, when Poland was independent; from 1945 to 1989, when Poland was a centrally planned economy; and from 1990 onward after Poland regained its sovereignty.

Bocqueraz explains that the first French accounting requirements were Colbert's ordinance in 1693 and the commercial code in 1807. As other essays explain, the requirements of Colbert's ordinance and the French commercial code were reflected in national codes in, for example, Belgium, Germany, Spain, and Switzerland. Furthermore, as Jaruga and Kabalski acknowledge, this French influence was extended to Poland when, under partition, it was compelled to use the commercial codes of Austria, Prussia, and Russia, each of which had, themselves, been influenced by the French code.

The second period in the history of French accounting was dominated by the creation of the *plan comptable général* (national accounting plan) that provided detailed guidance on the keeping of accounting records and the presentation of financial statements. Bocqueraz explains that the plan reflected the state's need to establish control over the production of accounting information. As a centrally planned economy between 1945 and 1989, the focus of accounting in Poland also shifted to the needs of government statisticians, which meant, according to Jaruga and Kabalski, that Poland eventually turned to the French model.

In the 1980s and 1990s, France and Poland adopted the EC directives, but their experiences of the directives differed. France had participated in the negotiations leading up to the adoption of directives. Bocquerez suggests that that level of harmonization achieved in France was minimal because of the compromises that underlay the directives. Poland joined the European Union after it had regained its sovereignty and long after the directives had been approved. Jaruga and Kabalski relate how the transformation of the economy meant changes to the accounting system. They suggest that the adoption of the directives changed the orientation of accounting in Poland from tax to business.

Other essays also highlight the different experiences of implementing the directives. Mara Cameran and Angela Pettinicchio suggest that their implementation in Italy brought greater regulation, increased comparability, and significantly improved accounting regulation. Carlos Larrinaga and Marta Macías argue that the directives drove an intense process of accounting reform in Spain, which led to a profound transformation in the audit profession and in the audit market. In contrast, Kristina Artsberg relates the disappointments of the Swedish accounting profession with the directives and how one member of that profession described the adoption of the directives as a return to the stone age of accounting.

Most of the essays deal only briefly with the influence of the European Union's IAS Regulation and any broader influences of IAS/IFRS on national accounting and financial reporting requirements. The one exception is Poland, for which the authors deal extensively with the influence of IAS/IFRS on successive revisions to the Accountancy Act and national accounting standards. While Bocqueraz admits to some convergence between French standards and IFRS, she suggests that convergence has been constrained by the French legal and taxation environment.

The link between financial reporting and taxation requirements is dealt with in all of the essays. The reader will soon be aware that the link not only varies among countries, but also that it continues to evolve. Ann Jorissen and Peter Stabel note that Belgium's commercial law of 1913 provided ample opportunity for tax laws to influence financial reporting and valuation practices, but that this opportunity was lost following the decision of the tax authorities to accept all of the rules of the accounting law of 1975 unless the tax law expressly provides otherwise. Kristina Artsberg refers to the extensive recent debates in Sweden about whether to retain a 1928 requirement that taxable profits should be based on accounting profits. Christopher Napier refers to the United Kingdom's earlier position under which entities were not required to measure accounting profit using tax rules, but omits any reference to the 1990s requirement that requires that the taxable profits of all entities should be based on their accounting profits (which makes the U.K. similar to Belgium and Sweden and may have affected whether entities elect to use IFRS instead of U.K. accounting standards in their legal-entity financial statements).

Germany is often referred to as a country in which there are strong links between accounting and taxation. Wolfgang Ballwieser explains how that link has now weakened because purely tax-driven values are no longer allowed in commercial accounts. German entities now have to follow three different systems: IFRS in their consolidated financial statements; German GAAP in their legal-entity financial statements; and tax rules for the determination of the tax liability.

This use of different accounting policies in consolidated and legal-entity financial statements is a French idea, which is what Claude Bocqueraz describes as the duality of French accounting. It has not only spread to Germany, but it has also made possible the approach in the IAS regulation, which requires the use of IFRS in the consolidated financial statements of a publicly traded entity but allows the use of national standards in the parent's legal-entity financial statements.

While all of the essays deal extensively with the different historical influences on accounting and financial reporting, only a few show how this has affected the technical accounting and financial reporting requirements. One exception to the general rule is Ballwieser's essay, which provides an insight into the underlying concept in German accounting of creditor-oriented prudent determination of profit, which is mandatory for all business entities. Another exception is Kees Camfferman's essay that explains the links between Dutch accounting and the concept of business economics (*bedrijfseconomie*) and the resulting emergence, use, and eventual demise of current cost accounting. Beyond these two examples, there is little about the technical aspects of accounting and financial reporting beyond passing mentions of hidden or secret reserves, the historical cost model, and a few other issues.

Technical issues aside, there is a wealth of material in the essays that will help researchers understand how accounting, financial reporting, and public policy in ten European countries have developed. In addition to the issues referred to earlier in this review, the country essays deal with the creation of independent private standard-setting bodies, the need for an audit, and the growth of the accounting and auditing professions. The essays on Belgium, Germany, The Netherlands, Switzerland, and the United Kingdom also address the roles of academics and practitioners who have contributed to accounting thought.

I recommend this collection of essays to anybody who wants to understand how national accounting in Europe has developed. Such an understanding will lead to a greater appreciation of the attitudes within Europe to the convergence of accounting standards. For post-graduate courses that continue to explore the history of accounting in individual countries, this volume will be an invaluable source of information. For researchers, the volume should help identify a glut of issues that warrant further study.

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London

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

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JULY 2012 PLACEMENT ADS

The deadline for free position ads to be included in this section of *The Accounting Review* is two months prior to the desired publication in the January, March, May, July, September, or November issues. Position ads, which are free with the purchase of a job posting in the AAA Career Center, should provide all relevant information about the available positions and must include contact information or application instructions for interested candidates. For more information on how to purchase a job posting in the Career Center, or for up-to-date, detailed information about the placement listings in this issue, please go to the AAA website at http://aaahq.org and click on "Career Center," or call our office at 941-921-7747.

WEBER STATE UNIVERSITY is seeking an individual to teach in the areas of tax or auditing; execute scholarship in field; provide service to university, community, and profession. Minimum Qualifications: ABD in accounting with expectation of completion of Ph.D. or D.B.A. within one year required. Area of emphasis in either tax or audit. Degree must be from a regionally accredited institution. Provide evidence of teaching effectiveness. To apply, complete the online application at: faculty/staff data form at http://jobs.weber.edu; attach a letter of application that addresses your qualifications for teaching tax and/or audit, a *cv*, and the names and contact information for three references. Screening is ongoing and will continue until position is filled. Criminal background check required as a condition of employment. For information, contact: hr@weber.edu; or (801) 626-6032. WSU is an AA/EO Employer.

VIRGINIA POLYTECHNIC INSTITUTE & STATE UNIVERSITY, Pamplin College of Business, Department of Accounting & Information Systems seeks to fill one or two tenure-track or visiting positions to begin August 10, 2012. Preference will be given to qualified candidates at the Assistant or Associate Professor levels. Individuals with teaching and research interests in the area of auditing, financial, or managerial accounting are preferred, but other areas will also be considered. Successful candidates will have a Ph.D. in accounting from an AACSB-accredited school, a program of scholarly research consistent with the rank applied for, and instructional capabilities at both the undergraduate and graduate levels. Information about the department and its programs is available at: http://www.acis.pamplin.vt.edu. Interested candidates *must* apply online at: http://www.jobs.vt.edu (posting number 0121558) and include with the electronic application a cover letter, resume, and three letters of recommendation, or names and contact information of three references. Review of applications will begin March 15, 2012. General inquiries should be directed to: Professor Greg Jenkins, Chair of the Recruiting Committee (Email: greg.jenkins@vt.edu) or Professor Reza Barkhi, Department Head, 3007 Pamplin Hall (0101), Blacksburg, VA 24061. Virginia Tech is an Equal Opportunity/Affirmative Action Employer.

WESTERN KENTUCKY UNIVERSITY invites applications from professionally qualified candidates for an Executive-in-Residence nine-month, non-tenure-track faculty position in the Department of Accounting. Qualifications include Master's in Accounting or M.B.A. from an AACSB-accredited university, CPA, minimum of five years of professional experience, and strong communication and technical skills. The superior candidate would have prior teaching experience in the field as well as the ability and willingness to teach upper-level courses in areas such as financial, auditing, or systems. The candidate must be committed to preparing students for success in multicultural business environments. The normal teaching load is 12 hours per semester. Please mail copies of application letter, resume, transcripts, and three letters of reference as soon as possible to: Accounting Search Committee, Western Kentucky University, 1906 College Heights Blvd #11061, Bowling Green, KY 42101. Additional information is available at: http://www.wku.edu/employment/faculty.php. Western is an Affirmative Action/Equal Opportunity Employer.

UNIVERSIDAD DEL TURABO, Sistema Universitario Ana G. Mendez, School of Business and Entrepreneurship (Puerto Rico) is seeking an Assistant Professor to teach Accounting and Finance courses. The ideal candidate must have a Ph.D. or D.B.A. in Accounting, or in a closely related field and be a Certified Public Accountant (CPA) with area of research in accounting. Candidates should be committed to do scholarly research. Candidates should have a proven track record of scholarly publications. Experience in teaching at the university level is preferred. Must have excellent communication skills in English and Spanish (verbal and written). If you are qualified and interested, please send your confidential resume and salary history to: Mrs. Camile Pérez, Recruitment Director at: camperez@suagm.edu

UNIVERSITY OF PITTSBURGH, Joseph M. Katz Graduate School of Business seeks to further strengthen its capability in and commitment to leadership and ethics in business, recognizing that ethics without leadership is ineffective and leadership without ethics can be misguided. Our goal is to advance teaching and research in leadership and ethics and promote interactions with both business and the non-profit sector. Our commitment to these goals is evidenced and supported by our David Berg Center for Leadership and Ethics, which plays a powerful role in focusing faculty research and teaching efforts and building relationships with business and non-profit organizations. To enrich our research, teaching, and outreach programs in leadership and ethics, we invite nominations and applications for the H. J. Zoffer Chair in Leadership and Ethics. This newly created chair, honoring Dean Emeritus H. J. Zoffer, is available beginning in September 2012. The successful candidate will be a recognized scholar with a proven teaching and research record in the area of leadership and ethics. The candidate must have the capacity to work effectively with faculty across business disciplines, motivate students at all levels—undergraduate, M.B.A., and Ph.D.—and engage the business community. Applications, nominations, or inquiries should be sent to: Dr. John C. Camillus, H. J. Zoffer Chair Search Committee, University of Pittsburgh, Mervis Hall, Pittsburgh, PA 15260; Email: camillus@katz.pitt.edu. The University of Pittsburgh is an Affirmative Action/Equal Opportunity Employer. Women and members of minority groups under-represented in academia are especially encouraged to apply.

DELAWARE STATE UNIVERSITY, College of Business Accounting invites applications for Adjunct positions for a Visiting Assistant/Associate Professor, starting Fall 2013, in The People's Republic of China. Qualified applicants will teach in China for a 14-week session. We are recruiting for positions in Introductory Accounting, Accounting Information Systems (AIS), Intermediate, and Cost Accounting. The successful candidate should have a doctorate in accounting from an accredited institution. ABD in Accounting or M.B.A./CPA will be considered. The successful candidates must be an academically qualified or professionally qualified faculty member and an effective teacher. Courses will be taught in English. Compensation is competitive. The College of Business is accredited by AACSB International and is housed in a high-tech teaching facility and serves approximately 20 percent of the University's total population of 4,000+ and offers Bachelor and M.B.A. degrees. A review of applications will begin immediately and continue until vacancies are filled. Post a letter of application and *vitae* on the university's website at: http://www.desu.edu, Administration/Human Resources/Job Openings, or go to: http://tbe.taleo.net/NA12/ats/careers/jobSearch.jsp?org=DESU&cws=1, and scroll down to end of page to apply. For inquiries, please contact: Dr. Bernadette Ruf, Chair of Accounting, Economics and Finance, College of Business, Delaware State University, 1200 N. DuPont Highway, Dover, DE 19901-2277; Email: bruf@desu.edu.

FAIRFIELD UNIVERSITY, Accounting Department invites applications for a tenure-track position at the Assistant or Associate Professor level in the Charles F. Dolan School of Business to begin Fall 2013. Qualified individuals in all specialty areas of accounting will be considered; preference will be given to those with an ability to teach in multiple areas. Candidates must have or be near completion of a doctoral degree. Professional certification in accounting is preferred. Candidates will be expected to demonstrate a strong commitment to excellence in teaching, scholarly research, and service to the Department and University. Fairfield University is Connecticut's Jesuit comprehensive university located on a scenic suburban campus near Long Island Sound, about one hour outside New York City. The Charles F. Dolan School of Business is AACSB-accredited and was recently named a "top 15" accounting program by the *Princeton Review*'s "Student Opinion Honors for Business Schools." A review of candidates will begin immediately and continue until the position is filled or September 30, 2012, whichever occurs first. Please send a letter of application, *curriculum vitae*, transcripts, and three references to: Dr. Joan Van Hise, Chair, Accounting Department, Charles F. Dolan School of Business, Fairfield University, Fairfield, CT 06824; Email: jvanhise@fairfield.edu. Fairfield University is an Equal Opportunity Employer. Women, minorities, veterans, and persons with disabilities are encouraged to apply. Visit our website at: http://www.fairfield.edu.

MORGAN STATE UNIVERSITY, Department of Accounting and Finance, Graves School of Business and Management invites applications for a tenure-track position starting in January or August 2013 at the rank of Assistant or Associate Professor. All areas in accounting will be considered. Candidates should have a Ph.D. degree in accounting from an AACSB-accredited institution. The preference will be given to those candidates who have a research record in accounting and demonstrate a commitment to high-quality teaching at both the undergraduate and graduate levels. The University has business school accreditation and accounting accreditation by the AACSB. The business school offers a B.S. degree in accounting and a Ph.D. degree in Business Administration with an accounting concentration. Master's level accounting courses are offered in the general M.B.A. program. A master's degree program in accounting will be offered starting in the Fall 2012. Morgan State University is located in Baltimore, Maryland, adjacent to the Chesapeake Bay and famous for its tourist attraction, The Inner Harbor. Baltimore is 48 miles from the District of Columbia and only a short driving distance from other major cities such as Philadelphia and New York City. Interested candidates should send academic *vitae* to: Alex P. Tang, Chair, Recruiting Committee, Department of Accounting & Finance, Morgan State University, Baltimore, MD 21251; Email: Alex.Tang@morgan.edu. We will be interviewing at the 2012 AAA Annual Meeting. Morgan State University is an affirmative action-equal opportunity employer.

PEKING UNIVERSITY, HSBC Business seeks applicants for Assistant/Associate/Full Professor levels in Management, Marketing, and Accounting. Job Responsibility: Quality research and teaching. The College: Peking University HSBC Business School is located in the thriving entrepreneurial city of Shenzhen and adjacent to the financial hub of Hong Kong. The University: Peking University is among the most prestigious education institutes in China. Qualifications: A successful applicant should hold a Ph.D. or is expected to receive one before July 2013. Senior applicants should have excellent records on research and teaching. To apply, please submit your application package to: hr_faculty@phbs.pku.edu.cn. Your package should include a cover letter, a *curriculum vita*, a research paper, three reference letters, and evidences of effective teaching.

INDIANA UNIVERSITY SOUTHEAST, School of Business is currently seeking applicants for a tenure-track appointment beginning in Fall 2013 for a Ph.D. in Accounting. Candidate will teach auditing and financial reporting. Ph.D. in accountancy required. CPA/CMA is desirable. Professional work experience preferred. Applications will be accepted until the position is filled, review to begin July 1, 2012. Starting date is August 1, 2013. Applicants should send *vita*, three letters of recommendation, and transcript to: hr@ius.edu, Attention: Tenure Track in Accounting-Search and Screen Committee. For additional information about the position, call Professor Doug Barney, Chair Search and Screen Committee (812) 941-2532. Indiana University Southeast is one of eight campuses in the Indiana University system. We are dedicated to providing high-quality education and lifetime growth opportunities for the people of Southern Indiana and the Greater Louisville region. Currently more than 7,000 students are enrolled in six master's degree programs (including two in the School of Business) and 38 bachelor's degree programs. Our campus facilities are located on a beautiful 177-acre tract at the foot of the "knobs" area of Southern Indiana, only ten minutes from downtown Louisville, KY. The campus has experienced significant growth as evidenced by construction of a new state-of-the-art library and five student residence halls. For more information on our campus and the School of Business please visit: http://www.ius.edu. We will be attending the 2012 American Accounting Association meeting in Washington, D.C. Indiana University is committed to a learning environment that actively seeks to support students, faculty, and staff from diverse backgrounds. Consistent with the University's commitment to diversity, persons from traditionally under-represented groups are strongly encouraged to apply. If you require reasonable accommodations due to a disability, please contact Human Resources. Indiana University Southeast is an Affirmative Action/Equal Opportunity Employer.

UNIVERSITY OF PENNSYLVANIA, Wharton School, Accounting Department is seeking applicants for a full-time, tenure-track faculty position at any level: Assistant, Associate, or Full Professor, in Financial, Managerial, and Tax Accounting. Demonstrated ability or potential for high-quality research and teaching are important. Applicants must have a Ph.D. (expected completion by June 30, 2014 is acceptable) from an accredited institution and have an outstanding research record or potential. The appointment is expected to begin July 1, 2013. Applications, including cover letter, *curriculum vitae*, and recent publications or working papers, must be submitted via our secure website: http://accounting.wharton.upenn.edu/. Junior-level candidates should also include contact information for up to three references who will be asked to submit letters of recommendation. The deadline for applications is November 30, 2012. Contact: Professor Brian Bushee, Accounting Department, The Wharton School, University of Pennsylvania, 1300 Steinberg-Dietrich Hall, Philadelphia, PA 19104-6365. The University of Pennsylvania values diversity and seeks talented students, faculty and staff from diverse backgrounds. The University of Pennsylvania is an Equal Opportunity/Affirmative Action Employer. Women, minority candidates, veterans, and individuals with disabilities are strongly encouraged to apply.

CENTRAL MICHIGAN UNIVERSITY, College of Business Administration, invites application for Chair for the School of Accounting beginning Fall 2012. The candidate should have an earned doctorate in Accounting, have a strong record of teaching and research commensurate with rank of Associate or Full Professor. The Chair will work with the School's administration to support and increase the intellectual contributions of the faculty and to expand academic programs. The Chair will provide oversight to undergraduate accounting majors; collaborate with faculty members and administrators to articulate program aspirations, initiatives, and policies; and will represent these programs to stakeholders inside and outside the University community. Compensation will be competitive and commensurate with qualifications and experience and includes a generous benefit package. The documents that applicants should submit are listed at Central Michigan University Jobs website: http://www.jobs.cmich.edu. CMU, an AA/EO institution, strongly and actively strives to increase diversity within its community (see http://www.cmich.edu/aaeo/).

SOUTHERN METHODIST UNIVERSITY, Cox School of Business, Tenure-Track Faculty Position in Accounting (#049944) for Fall 2013. Hiring is contingent upon a satisfactory background check. Applicants must have a Ph.D. (or equivalent) in Accounting from an accredited institution or will complete a Ph.D. by August 1, 2013, and have an interest in research and teaching in financial, managerial, or tax accounting. Candidates should be qualified to teach at the undergraduate or graduate level. Salary and teaching loads are competitive, based on qualifications and experience. Applicants should send *curriculum vitae*, published/working papers, three letters of reference and evidence of teaching effectiveness to: Accounting Recruiting Committee, Southern Methodist University, Cox School of Business, PO Box 750333, Dallas, TX 75275-0333; Email: ACCT-RECRUIT@mail.cox.smu.edu. Applications must be received by January 15, 2013, but will be accepted until the position is filled. SMU is an Affirmation Action/Equal Opportunity/Title IX Employer.

UNIVERSITY OF NEBRASKA–LINCOLN, School of Accountancy invites applications for a tenured appointment beginning August 2013. Nebraska is committed to establishing a research presence and productivity consistent with our entry into the Big Ten Conference and the Committee on Institutional Cooperation, the academic affiliation of the Conference. The successful candidate will hold the newly endowed Lienemann Chair in Accounting and will be a Full Professor. Candidates are required to hold a Ph.D. degree in accounting or related area from an AACSB-accredited College of Business and must have an established record of publication in A-level accounting and/or finance journals. All areas of teaching and research interest will be considered; we are particularly interested in individuals who are strong organizational citizens and interested in being active members of a rich academic community. Salary, benefits, and research support are very competitive and consistent with other Big Ten Universities. The College of Business Administration is under the leadership of Dean Donde Plowman and Associate Deans Kathy Farrell and Ravi Sohi; the research, teaching, and engagement missions of the College have been significantly refocused to align with the university's entrance into the Big Ten. *BusinessWeek* ranked UNL's M.B.A. program 4th in the country among part-time M.B.A. programs. The college continues to build on and further develop its research strength. Increased support for research and close proximity to thriving business communities of Omaha and Lincoln help advance the college's research goals. The college prides itself in being an open and collaborative community. Initial screening of applicants will begin July 1, 2012 and continue until the position is filled. To be considered for the position, go to http://employment.unl.edu, requisition 120368, and complete the Faculty/Academic Administrative Form. In addition, candidates need to send a letter of interest, *curriculum vitae*, a sample of publications and current research, evidence of teaching experience and ability, and contact information for at least three references to: Dr. David Smith, School of Accountancy, 387 CBA, 512 N 12th St., PO Box 880488, Lincoln, NE 68588-0488; Email: dsmith19@unl.edu. The University of Nebraska has an active National Science Foundation ADVANCE gender equity program, and is committed to a pluralistic campus community through affirmative action, equal opportunity, work-life balance, and dual careers. We assure reasonable accommodation under the ADA; contact Dr. Paul Shoemaker at (402) 472-2337 for assistance.

LONGWOOD UNIVERSITY, Department of Accounting, Economics, Finance and Real Estate, College of Business and Economics is seeking qualified applicants for a tenure-track position in Accounting to begin August 2013. For more information and to apply please visit: http://www.longwood.edu/hr/jobs.htm. Review of applications will begin immediately. Applicants should send a letter of application and *vita to* (indicate position #F0193). If additional information is needed, contact Dr. Bennie Waller at: wallerbd@longwood.edu.

UNIVERSITY OF MINNESOTA–DULUTH, Department of Accounting, Labovitz School of Business and Economics has a full-time, nine-month, tenure-track Instructor/Assistant Professor/Associate Professor position in Accounting, available beginning Fall 2013. Job duties and responsibilities include teaching, research, and service. Essential qualifications include: Applicants must be ABD (with a clear plan for completion by December 31, 2013) from a doctoral program in Accounting, or Business Administration with a concentration in Accounting, from an AACSB-International-accredited school or major internationally recognized university (no more than 25 percent of the credits for the degree can be online). If the candidate is ABD for more than two years, there must be evidence of, at minimum, manuscript submissions to a refereed accounting-related journal. Evidence of potential for scholarship is essential, as is the demonstrated commitment to developing a research agenda in accounting. Demonstrated teaching experience (graduate assistant or corporate training experience acceptable). Demonstrated written communication skills appropriate to a faculty position are also essential. The University of Minnesota requires that you apply online for this position. For a complete position description and information on how to apply online, visit: http://employment.umn.edu/, and search for Job Requisition 177885. Complete applications will be reviewed beginning September 10, 2012. The University of Minnesota is an Equal Opportunity Educator and Employer.

THE UNIVERSITY OF OKLAHOMA, John T. Steed School of Accounting anticipates one visiting faculty opening for the Fall 2012 and Spring 2013 semesters. Rank: Visiting Assistant or Associate Professor. Education: Ph.D. in accounting or Master's degree and CPA or other professional qualifications (CMA, CIA, etc.). Will consider candidates not holding a Master's degree who are CPAs with teaching experience. Teaching: Candidates must be able and willing to teach at both the undergraduate and graduate level. Candidates must possess some prior teaching experience and provide evidence of quality teaching performance. Evaluation: Qualified candidates will be evaluated based on evidence of quality teaching and fit to departmental teaching needs. Salary and fringe benefits: Open and competitive based on applicable experience. Anticipated appointment date: August 15, 2012–May 15, 2013. General: Qualified individuals are encouraged to submit a resume and evidence of teaching qualifications to: Jan Nelson, Managerial Associate, Steed School of Accounting, Price College of Business, The University of Oklahoma, 307 W. Brooks, AH 200, Norman, Oklahoma 73019-4004; Email: jnelson@ou.edu; Fax: (405) 325-7348. For questions, contact the Director of the Steed School of Accounting, Dr. Fran Ayres at: fayres@ou.edu. Interested applicants may also apply for the open Assistant Faculty position in the Steed School of Accounting. The screening of applications will begin immediately. Applications will be accepted until the position is filled. The University of Oklahoma is an Equal Opportunity/Affirmative Action Employer. Women and minorities are encouraged to apply.

UNIVERSITY OF MASSACHUSETTS LOWELL, AACSB-accredited Manning School of Business (MSB) invites applications for multiple tenure-track positions in Accounting at the rank of Full/Associate/Assistant Professor. A new M.S.A. program will launch in September 2012. In addition, the MSB expects to add a new Accounting track to its recently approved Ph.D. in Business Administration, and to occupy a new state-of-the-art instructional facility, within the next three years. The MSB serves more than 2,000 undergrad and M.B.A. students in classrooms and online. The University offers nearly 15,000 students more than 120 degrees and academic opportunities, including internships, co-op programs, and study-abroad programs. Requirements: An earned Ph.D. or D.B.A. in Accounting or a related discipline by the starting date (September 2013 or later). A demonstrated record of, or at the Assistant level a strong potential for, high-quality research. A demonstrated record of high-quality grad/undergrad instruction in some combination of Financial/Managerial Accounting, AIS, Auditing, or Taxes. Excellent interpersonal skills and the ability to speak and write English clearly. The ability to work with a diverse student and faculty population. Strong consideration given to candidates with public accounting or corporate experience, preferably with CPA, CMA, etc. Apply online at: http://jobs.uml.edu. Attach (1) a statement of professional interests and goals, (2) *cv*, (3) samples of research work, and (4) teaching philosophy and evidence of teaching effectiveness. Applications will be accepted until the posting is closed. Contact the University of Massachusetts Lowell Human Resources Department at (978) 934-3560 for information regarding benefits and related employment matters.

CALIFORNIA POLYTECHNIC STATE UNIVERSITY, SAN LUIS OBISPO, Orfalea College of Business (OCOB) seeks candidates for the Executive Director position for the Center of Excellence in Accounting. The Executive Director, under the direction of the OCOB Dean, will lead the development of the Accounting Center to enhance relationships with external stakeholders, students, and faculty, as well as serve as an instructor for undergraduate and graduate service learning programs in taxation. An important aspect of the position will be to cultivate, solicit, and steward donors and prospective donors to support and sustain the vision, development, and short- and long-term goals and objectives for the Accounting Center and its faculty and programs. Candidates must have minimum of five years of professional practice as a CPA or attorney at law. Preferred qualifications include grant and proposal writing experience and/or a Master's degree. To apply, visit: http://www.calpolyjobs.org, complete a required online application, and submit to requisition #102497. Please see online instructions regarding attaching supplemental documents. Review begin date is May 30, 2012 and will remain open until filled. Cal Poly is strongly committed to achieving excellence through cultural diversity. The University actively encourages applications and nominations of all qualified individuals. EEO.

PACE UNIVERSITY, Lubin School of Business invites applications for a full-time, tenure-track position in financial accounting, at the Assistant or Associate level, starting Fall 2013. A successful candidate for this position must reflect the School's commitment to excellence in teaching and scholarly research. A Ph.D. is required and practical experience is beneficial. The Lubin School of Business of Pace University is a recognized leader in business education and is committed to preparing professionals for successful careers in the global economy. The School offers an extensive array of undergraduate, graduate, and post-graduate programs on campuses in New York City and Westchester County. The Accounting Department is fully accredited by the AACSB-International Association for Management Education. Salary and benefit package is competitive. Please email your resume to: Dr. Rudy Jacob at: rjacob@pace.edu. Pace University is an Affirmative Action/ Equal Opportunity Employer.

UNIVERSITY OF WISCONSIN–WHITEWATER, College of Business and Economics, Accounting Department invites applications for multiple full-time, tenure-track Assistant Professor positions in the areas of Tax, Financial, and Managerial Accounting beginning Fall 2013. Applicants should have a doctoral degree in Accounting; however, applicants with doctoral degrees in closely related fields and ABDs will be considered. The College of Business and Economics is AACSB-accredited so applicants must be academically qualified under those standards. Preference will be given to applicants with CPA certification and accounting practical experience. The UWW Accounting Department strives to be among the best regional public comprehensive university accounting departments by focusing on quality teaching, a solid research portfolio, and service. The complete position announcement is at: http://www.uww.edu/employment/unclassified.html. For more information about our department go to: http://www.uww.edu/cobe/accounting. To apply, send a letter of application, resume, contact information for three references, and evidence of teaching effectiveness. Original transcripts will be required for finalists. Application materials should be emailed to Joanna Stradinger at: stradinj@uww.edu or mailed to: Dr. Carol J. Normand, Search and Screen Committee Chair, Attn: Joanna Stradinger, Accounting Department, University of Wisconsin–Whitewater, 800 W. Main Street, Whitewater, WI 53190-1790. Only complete applications will be considered. Apply by July 27, 2012 to be considered for an interview at the AAA Annual Meeting in Washington, D.C., August 4–8. Applications received by October 1, 2012 are ensured consideration. Positions will remain open until filled. UW–Whitewater is an Equal Opportunity/ Affirmative Action Employer.

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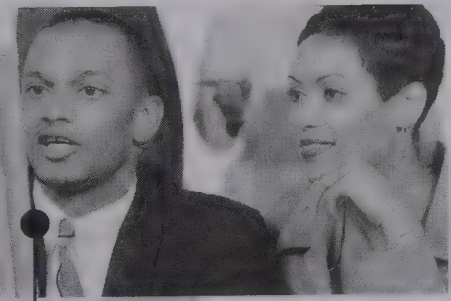

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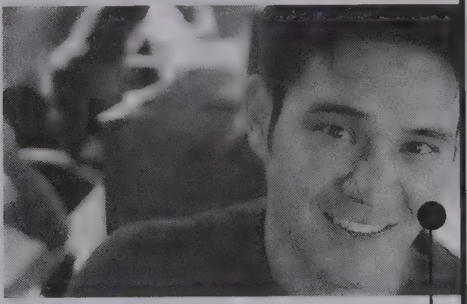

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The Executive Director of the Association is Tracey E. Sutherland.

The American Accounting Association was founded in 1916 as the American Association of University Instructors in Accounting. Its present name was adopted in 1936. The Association is a voluntary organization of persons interested in accounting education, research, and practice.

Membership in the Association is available at the following annual rates.

Full membership:

Membership dues of $205.00 include *Accounting Education News*, and the online selection of all three Association-wide journals: *The Accounting Review*, *Accounting Horizons*, and *Issues in Accounting Education*. (This does not include Section journals.) Full members may also opt to receive *hardcopies* of AAA journals for an additional cost:

The Accounting Review	with Print Option $45.00	Total U.S. $250.00
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Is U.S. Multinational Dividend Repatriation Policy Influenced by Reporting Incentives?

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ABSTRACT: This study finds evidence that public-company reporting by U.S. multinational corporations (MNCs) creates disincentives to repatriate foreign earnings to the U.S. and contributes to the accumulation of cash abroad. MNCs operate under U.S. international tax laws and financial reporting rules and face two potential consequences when they repatriate foreign earnings: a cash payment for repatriation taxes and a reduction in reported accounting earnings. Using a confidential dataset of financial and operating characteristics of foreign affiliates of MNCs combined with public-company data, we examine how repatriation amounts vary across firms that face relatively strong reporting incentives to defer an accounting expense. Our results suggest that reporting incentives reduce repatriations by about 17 to 21 percent annually.

Keywords: *repatriation policy; reporting incentives; international tax policy.*

Data Availability: *Bureau of Economic Analysis (BEA) data were made available to the authors under a legal confidentiality arrangement; all non-BEA data are available from public sources.*

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I. INTRODUCTION

The U.S. tax system plays a role in the ability of U.S. multinational corporations (MNCs) to compete in the global marketplace. Various critics of U.S. international tax policy argue that the system of worldwide taxation has a negative effect on the competitiveness of U.S. firms and creates incentives to leave or "park" foreign profits overseas. Consistent with this incentive, MNCs held an estimated $403 billion of undistributed earnings in foreign countries at the end of 1999 (Brumbaugh 2003), which doubled to $804 billion by the end of 2004 (Redmiles 2008). As a result, policy makers and regulators have an interest in understanding factors that foster the accumulation of earnings in foreign countries.

Existing literature identifies several factors that contribute to this accumulation. Growing evidence that repatriations decrease when the tax cost of repatriating increases (e.g., Hines and Hubbard 1990; Altshuler and Newlon 1993; Altshuler et al. 1995; Grubert 1998; Desai et al. 2001; Desai et al. 2007) suggests that taxes are an important factor. Other evidence suggests that firms with high foreign profitability increase both foreign retained earnings and dividend repatriations (Grubert 1998), and that firms with domestic financing needs, domestic investment needs, and intra-firm agency problems repatriate more foreign earnings (Desai et al. 2007).

More recently, accounting research investigates how accounting for income taxes contributes to the accumulation of foreign earnings abroad. Shackelford et al. (2011) model the effect of financial reporting on firms' real decisions and note that repatriation is an area for which such forces have potential importance. Graham et al. (2011) find that, when asked which factors are important in the decision to reinvest foreign earnings outside of the U.S., executives rate the importance of deferring an accounting expense for repatriation taxes as high as deferring a cash payment for repatriation taxes. We explore this issue in more detail by studying the extent to which reporting incentives to defer an accounting expense affect managers' actual repatriation decisions.[1]

As discussed in the "Repatriation and Tax Law" section, tax laws prescribe the amount and timing of cash payments for U.S. taxes on repatriations of foreign earnings, whereas accounting rules, described in the "Repatriation and Financial Reporting Rules" section, prescribe the amount and timing of the income tax expense recorded in the financial statements. Tax laws require MNCs to pay repatriation taxes when a foreign affiliate remits its earnings as a dividend to the U.S. parent.[2] Financial reporting rules require MNCs to recognize a repatriation tax expense for actual or expected repatriation taxes when earnings are generated in foreign affiliates, regardless of whether they are repatriated. However, an exception to this general rule (hereafter, the Indefinite Reversal Exception) allows them to defer recognition of a repatriation tax expense until repatriation if the earnings are indefinitely reinvested abroad (hereafter, permanently reinvested earnings or PRE).

The Indefinite Reversal Exception introduces financial reporting consequences to repatriation for MNCs that routinely utilize the PRE designation. Specifically, when an MNC reinvests foreign earnings and designates them as PRE, it recognizes foreign pre-tax earnings with no corresponding repatriation tax expense. If the MNC eventually repatriates the earnings, then it must recognize a repatriation tax expense with no corresponding pre-tax earnings, potentially decreasing after-tax earnings. As a result, the Indefinite Reversal Exception creates reporting incentives to defer repatriation incremental to the cash tax incentive documented in existing literature.

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[1] We obtain the data for this study from the Bureau of Economic Analysis (BEA) *Survey of U.S. Direct Investment Abroad*. Participation in the BEA surveys is mandated by federal law pursuant to the International Investment and Trade in Services Survey Act (P.L. 94-472, 90 Stat. 2059, 22 U.S.C. 3101-3108) and willful failure to participate can result in monetary fines and/or imprisonment. Furthermore, the BEA staff reviews the survey responses. However, as with any data source, errors occur and compliance may be less than 100 percent. See Mataloni (2003) and http://www.bea.gov/surveys/diasurv.htm for detailed information on BEA data.

[2] The BEA defines a foreign affiliate as a foreign business enterprise in which there is direct or indirect equity investment of more than 10 percent by a U.S. entity. An affiliate can be any organization, association, branch, estate, trust, venture, or subsidiary that exists to make a profit. However, we use the term foreign affiliate to refer only to foreign subsidiaries over which the U.S. parent has control, i.e., greater than a 50 percent equity investment.

These reporting incentives vary across firms in at least two ways. First, existing research finds that some firms have a stronger focus on reported earnings than do other firms. For example, evidence suggests that public firms are more likely than private firms to use accounting methods and tax-planning methods that increase earnings (Penno and Simon 1986; Cloyd et al. 1996; Badertscher et al. 2011) and, among public firms, those with greater ownership by dedicated investors exhibit a lesser tendency to fixate on earnings (Bushee 1998). Second, the magnitude of the earnings effect of repatriation will vary depending on a firm's use of the Indefinite Reversal Exception. All else equal, firms that have designated more undistributed foreign earnings as PRE have a larger unrecognized tax expense and a greater potential to experience a decrease in earnings upon repatriation.

We use this variation to develop two categories of proxies that capture the magnitude of reporting incentives to defer repatriation. The first category, our capital markets measures, captures the magnitude of any anticipated negative capital market response to the repatriation tax expense. Within this category, we estimate two measures of reporting incentives. Our first measure captures public versus private firm ownership because the financial reporting literature suggests that private firms are relatively less sensitive to capital market pressures (e.g., Cloyd et al. 1996; Penno and Simon 1986; Wolfson 1993; Graham et al. 2011). Therefore, on average, public firms face greater reporting incentives to avoid repatriation than private firms. Our second measure combines several proxies that capture the extent to which public firm managers believe that their firms' earnings face relatively more intense capital market scrutiny—earnings response coefficients, the firm's history of meeting analyst expectations, and ownership by dedicated investors. On average, we expect managers to have stronger reporting incentives to avoid repatriation as earnings response coefficients increase (because the stock price decrease will be larger per dollar of repatriation tax expense), as the history of meeting analyst forecasts improves (because the firm's history of meeting analyst forecasts suggests a greater concern about earnings) and as the proportion of shares held by dedicated institutional investors decreases (because these investors are more sophisticated financial statement users with a long-term focus).

The second category of proxies for reporting incentives, our earnings measure, uses PRE to identify firms with a relatively large unrecognized tax expense and therefore a greater potential to experience an earnings decrease when repatriating. Upon repatriation, firms that routinely designate foreign earnings as PRE must recognize a tax expense that they regularly deferred under the Indefinite Reversal Exception. Thus, we expect that reporting incentives to avoid repatriation are, on average, greater for firms that make extensive use of the PRE designation.

The effects of tax laws and financial reporting rules on repatriations are interrelated because the cash payment for repatriation taxes and the financial statement expense for repatriation taxes both depend on the repatriation tax rate. Due to this interdependence, we test whether firms are more sensitive to the repatriation tax rate as reporting incentives to defer repatriation increase in magnitude.

We test our predictions using a sample of 479 public and 98 private MNCs from 1999 through 2004. After controlling for investment opportunities, size, financing constraints, tax planning, and country-level attributes, we find that repatriation by public firms is more sensitive to the repatriation tax rate than repatriation by private firms. We also find that repatriation by public firms for which the recognition of a repatriation tax expense is expected to affect firm value more negatively is more sensitive to the repatriation tax rate than repatriation by other public firms. Finally, repatriation by public firms that make extensive use of the PRE designation under the Indefinite Reversal Exception is more sensitive to the repatriation tax rate than repatriation by other public firms. Our estimates suggest that firms with high reporting incentives repatriate, on average, 16.6 to 21.4 percent less per year than firms with low reporting incentives. This estimate amounts to a total of $6.7 billion annually for our sample of public firms and $1.5 billion annually for public firms with high amounts of PRE.

We evaluate the robustness of our results and the adequacy of our proxies for reporting incentives using two additional analyses. First, we conduct subsample analyses and find that the sensitivity of repatriation to the repatriation tax rate is greater when firms are more likely to recognize repatriation tax expense upon repatriation and anticipate a negative capital market response to this expense. Second, we extend Foley et al. (2007) to show that the effect of repatriation taxes on the accumulation of foreign cash in public companies is exacerbated by reporting incentives. Collectively, our empirical results consistently demonstrate that reporting incentives deter repatriation of foreign earnings.

This study makes important contributions to the literature by informing the broader policy debate about necessary reforms to our international tax system and by aiding our understanding of how accounting for income taxes affects repatriation behavior. The accounting literature has only very recently begun to explore the effect of accounting standards on repatriation decisions. Graham et al. (2011) provides useful initial insight by surveying executives. While their evidence tells us what managers believe is important with respect to repatriation taxes, we study whether financial accounting consequences have an economically meaningful effect on managers' actions. Specifically, we identify and measure the mechanisms that create reporting incentives to defer recognition of the repatriation tax expense and estimate the magnitude of their effect on repatriation amounts. Thus, our results shed light on the extent to which accounting standards contribute to the growing pool of undistributed foreign earnings that U.S. firms hold in foreign countries.

Section II provides a background and develops our hypotheses. Sections III and IV describe our data and our research design, respectively. Section V discusses our main results and Section VI provides results from supplemental analyses. Section VII concludes.

II. BACKGROUND AND HYPOTHESES

We analyze how tax laws and financial reporting rules interact to influence repatriation decisions. Therefore, to motivate our hypotheses, we first describe the tax and financial reporting rules that govern the repatriation of foreign earnings.

Repatriation and Tax Law

When a foreign affiliate pays a dividend to its U.S. parent, the amount of repatriation taxes equals the dividend grossed-up for foreign taxes paid times the U.S. statutory tax rate minus the foreign tax credit. Generally, the foreign tax credit equals the amount of foreign taxes paid on the foreign earnings up to the amount of the U.S. tax liability. If the foreign tax rate is greater than the U.S. tax rate, then the MNC owes no incremental U.S. taxes on repatriated earnings. Further, if an MNC repatriates earnings from more than one country, then it can use tax credits generated in high-tax countries to offset U.S. taxes on repatriations from low-tax countries. Therefore, the incremental tax rate on repatriation can be thought of as the difference between the U.S. tax rate and the average foreign tax rate paid on repatriated foreign earnings.

From a tax perspective, an MNC with foreign earnings has two choices. It can reinvest the earnings abroad or repatriate the earnings. When the MNC reinvests the earnings abroad, the current taxes paid on foreign earnings consists only of foreign income taxes. Alternatively, when the MNC repatriates the earnings, the current taxes paid on foreign earnings consists of both foreign income taxes and U.S. repatriation taxes. Thus, *tax laws—not financial reporting rules—determine when firms pay repatriation taxes.*

Existing work on repatriation decisions finds that repatriations are negatively related to the repatriation tax rate. Hartman (1985) shows theoretically that, when after-tax returns and tax rates are constant over time, the repatriation tax rate does not affect the repatriation decision because all foreign earnings will eventually be taxed at the U.S. tax rate. However, subsequent empirical evidence suggests that repatriations are decreasing in the repatriation tax rate (e.g., Hines and Hubbard 1990; Altshuler and Newlon 1993; Grubert 1998; Desai et al. 2001, 2007). Altshuler et al. (1995) reconcile theoretical work with empirical evidence by recognizing that tax rates are not constant over time. The authors estimate transitory and permanent components of firms' repatriation tax rates and find that dividend repatriations are negatively related to the transitory component and are not related to the permanent component. In contrast to evidence that U.S. tax law generally deters repatriation, Altshuler and Grubert (2003) describe sophisticated tax-planning strategies that allow low-tax affiliates to avoid repatriation taxes, but note that this type of tax planning is costly.

Repatriation and Financial Reporting Rules

From a financial reporting perspective, an MNC that reinvests foreign earnings abroad has two choices. The MNC can either currently recognize an expense for expected repatriation taxes on current earnings or it can use the Indefinite Reversal Exception to defer recognition of a repatriation tax expense until it repatriates the earnings.[3] Thus, *financial reporting rules—not tax laws—determine when firms recognize the financial statement expense for repatriation taxes.*

Once a firm designates foreign earnings as PRE, repatriation of these earnings in a subsequent accounting period will, all else equal, decrease earnings relative to prior periods because it must recognize the repatriation tax expense that it deferred in prior years. As a result, accounting expense recognition is an additional consequence of repatriation when firms repatriate earnings that were previously designated as PRE.

Some MNCs use the PRE designation extensively. To illustrate, Bear Sterns and Company (2005) find that the S&P 500 report $420 billion of PRE at the end of 2002, and Zion et al. (2010) find that these firms report nearly $1 trillion of PRE at the end of 2008. In addition, existing research finds evidence that deferral of the financial statement expense for repatriation taxes is an important factor in the decision to designate undistributed foreign earnings as PRE. Krull (2004) finds that MNCs increase amounts designated as PRE, thereby deferring recognition of the repatriation tax expense, in order to maximize reported after-tax earnings and meet earnings benchmarks. Graham et al. (2011) report that the extent to which MNCs utilize the PRE designation is positively associated with how important they rate expense deferral and negatively associated with how important they rate cash tax deferral. Evidence from both studies suggests that deferral of the repatriation tax expense is an important motivation in the decision to use the Indefinite Reversal Exception.

Hypotheses

As we discuss in the "Repatriation and Tax Law" section, existing research finds evidence that repatriation taxes deter repatriation. To examine whether reporting incentives also deter repatriation, we identify and measure firm attributes across which reporting incentives vary while holding the cash payment for repatriation taxes constant. Because the purpose of our study is to explain a continuous accumulation of funds abroad, we use measures that are designed to capture a type of firm that faces either greater capital market pressure to consistently report strong earnings or a greater potential to experience an earnings decrease when repatriating. We do not include measures that capture incentives to time expense recognition, such as variables that capture incentives to beat an earnings benchmark in a specific period, because the timing of repatriations from one year to the next likely represents small differences in overall repatriations that cannot explain an accumulation of funds abroad. With this purpose in mind, we characterize our measures of reporting incentives as capital market measures that capture an anticipated negative capital market response to the repatriation tax expense or as earnings measures that reflect a firm's potential for experiencing an earnings decrease when repatriating.

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[3] FASB ASC 740 (2009) contains both the general rule requiring MNCs to recognize an expense for expected repatriation taxes and the Indefinite Reversal Exception.

We begin by investigating hypotheses on whether capital market incentives deter repatriation. We first use public versus private firm ownership as a measure of the anticipated negative capital market response to the repatriation tax expense. This capital market measure allows reporting incentives to vary while holding the cash payment for repatriation taxes relatively constant because both public and private firms are subject to the same tax laws, but capital market pressures vary between public and private firms due to differences in the constituents to which the two types of firms report. Specifically, public firm managers have private information, report to current and potential investors, and are evaluated on their performance based on the information they provide. Therefore, public firm managers typically have a strong focus on reported earnings because of its effect on both firm value and managerial compensation (e.g., Cloyd et al. 1996; Penno and Simon 1986). In contrast, private firms have high levels of insider ownership and encounter less information asymmetry between managers and investors. Therefore, private firm managers have relatively less incentive to focus on reported earnings (Beatty and Harris 1999).

Several studies empirically test this conjecture. Using survey evidence, Penno and Simon (1986) find that public firms are more likely than private firms to choose financial accounting methods that increase earnings. Consistent with this evidence, Cloyd et al. (1996) and Badertscher et al. (2011) find that public firms are more likely than private firms to choose financial accounting methods and/or tax-planning methods that result in higher earnings on the financial statements. The results of these studies suggest that public firms place a stronger focus on increasing accounting earnings than private firms. More closely related to our study, Graham et al. (2011) find that more public firms than private firms rate the financial statement expense for repatriation taxes as important in the repatriation decision. All else equal, we expect that the potential recognition of a repatriation tax expense will have a more negative impact on public firm repatriations than private firm repatriations.

Both the cash payment for repatriation taxes and the financial statement expense for repatriation taxes depend on the repatriation tax rate. Therefore, after controlling for other determinants of repatriation, if public firms are more sensitive to the repatriation tax rate than private firms, then we can attribute this incremental sensitivity to reporting incentives.

> **H1A:** *Ceteris paribus*, repatriation by public firms is more sensitive to the repatriation tax rate than repatriation by private firms.

Because unobservable differences between public and private firms may affect the sensitivity of repatriation to tax costs, we develop another measure of the anticipated negative capital market response to repatriation tax expense to exploit cross-sectional variation in reporting incentives among public firms. This measure identifies public firms for which we expect a repatriation tax expense to have a more negative effect on firm value. This test focuses on the potential stock price response to a repatriation tax expense as an alternative capital market measure of reporting incentives.

The existing capital markets literature develops many measures that capture various aspects of a potential stock price response to the firm's accounting earnings. Because the purpose of this study is to explain a continuous accumulation of funds outside the U.S., we select three of these measures to capture a diverse set of factors that create consistent pressure to report strong earnings numbers: earnings response coefficients, the firm's history of meeting analyst forecasts, and ownership by dedicated institutions.

The first measure is the firm's long-window earnings response coefficient (ERC). The ERC is "the most common empirical measure of investor responsiveness" to earnings (Dechow et al. 2010). We use this variable to capture the extent to which the manager is more likely to believe that tax expense recognition will negatively impact stock price.[4] As ERCs increase, a firm's price-sensitivity to a repatriation tax expense increases.

The second measure is the proportion of all previous quarters that a firm appears in I/B/E/S for which it beat the median analyst EPS forecast by one cent or less (Carter et al. 2007; Degeorge et al. 1999). Prior studies find that firms enjoy a stock market premium that increases in their tendency to meet analyst expectations, creating situations in which managers act as if sustaining these patterns will prevent a negative capital market response (e.g., Degeorge et al. 1999; Abarbanell and Lehavy 2003). Tax expense recognition upon repatriation of foreign earnings can cause a firm to miss an analyst forecast, resulting in a reversal of this premium. Therefore, firms with a more consistent history of meeting earnings forecasts are likely to have a higher price-sensitivity to a repatriation tax expense.

The third measure of the potential stock price response to a repatriation tax expense is the proportion of the firm's stock held by long-term institutional investors (Bushee 1998). Bushee (1998) classifies institutional investors based on the extent to which they likely influence myopic behavior by firm managers. Dedicated investors alleviate pressures for myopic behavior (e.g., reducing R&D or repatriations to meet earnings goals) because their large, long-term holdings create incentives to monitor managers and rely on information other than earnings to assess performance. As dedicated investor ownership increases, the firm will likely experience a lower price-sensitivity to expense recognition because investors, and therefore managers, focus less on short-term earnings and more on longer term economic consequences of current period decisions.

Assuming managers want to maximize the firm's stock price, we expect that firms whose stock price will decrease more per dollar of expense will be more sensitive to a repatriation tax expense. Firms with higher ERCs, more consistent histories of beating analyst forecasts, or lower ownership by dedicated institutions face a potentially larger decrease in stock price per dollar of repatriation tax expense. We expect that this higher price-sensitivity to earnings will make these firms' repatriation decisions more sensitive to the repatriation tax rate. Therefore we predict the following:

> **H1B:** *Ceteris paribus*, repatriation by public firms with high price-sensitivity to earnings is more sensitive to the repatriation tax rate than repatriation by other public firms.

Next, we investigate whether earnings incentives deter repatriations. This test uses a measure that identifies firms for which the earnings effect of repatriations is expected to be relatively high. We identify the extent to which each public MNC in our sample relies on the Indefinite Reversal Exception by obtaining PRE disclosed in financial reports. This test also focuses on public firms because we can only observe PRE disclosures from Securities and Exchange Commission (SEC) filings.

We use PRE to measure reporting incentives to defer repatriations because the cash payment for repatriation taxes is independent of the firm's level of PRE, but the financial statement expense varies directly with the level of PRE. Thus, a firm's level of PRE is a direct measure of the potential financial reporting consequence of repatriation. All else equal, the firm's unrecognized repatriation tax expense increases in PRE. Therefore, MNCs with high amounts of PRE have a greater potential to recognize a tax expense upon repatriation than firms with low amounts of PRE. Even if an MNC only repatriates current earnings, if it previously designated high levels of undistributed foreign earnings as PRE, then, all else equal, it will have a higher tax expense and lower earnings compared to prior periods. Furthermore, MNCs that have designated all of their undistributed foreign earnings as PRE cannot avoid tax expense recognition in their financial statements by repatriating non-PRE foreign retained earnings while firms with low or no PRE can.[5]

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[4] See Kothari (2001) and Dechow et al. (2010) for a review of research in this area.

Thus, we expect that firms repatriate less as the unrecognized repatriation tax expense related to the firm's PRE increases. As in our tests of H1A and H1B, if after controlling for other determinants of repatriation, repatriation by firms that use PRE extensively is more sensitive to the repatriation tax rate than other firms, then we can attribute this incremental sensitivity to reporting incentives. Hence, we test the following hypothesis:

> **H2:** *Ceteris paribus*, repatriation by public firms that rely extensively on the PRE designation under the Indefinite Reversal Exception is more sensitive to the repatriation tax rate than repatriation by other public firms.

III. DATA

Examining whether reporting incentives affect the repatriation decisions of MNCs necessitates measuring repatriations, the repatriation tax rate, and reporting incentives. We obtain repatriation amounts and data required to estimate the repatriation tax rate from the *Survey of U.S. Direct Investment Abroad* conducted by the Bureau of Economic Analysis (BEA) each year since 1982. Federal law obligates MNCs to report certain financial and operating data for both domestic and foreign operations to the BEA. An MNC is the combination of a single U.S. entity, called the U.S. parent, and at least one foreign affiliate. The amount of information collected by the BEA varies by year and by affiliate because more detailed information is collected in benchmark years, which occur every five years (1999 and 2004 in our sample), and only affiliates that meet the applicable reporting threshold are required to report.[6]

MNCs report to the BEA on a fiscal-year basis and follow U.S. Generally Accepted Accounting Principles (GAAP) except consolidation rules. The BEA requires that the U.S. parent use the equity method of accounting for its foreign affiliates, whereas GAAP requires consolidation for ownership interests greater than 50 percent. This unique feature of the data allows us to separately identify foreign and domestic financial data (e.g., foreign assets), which is necessary to compute many of our regression variables. The BEA data also enable us to aggregate financial data across the U.S. parent and its foreign affiliates to compute firm-level variables (e.g., worldwide assets). To avoid double-counting, we eliminate intercompany accounts (e.g., assets, sales, and income) reported by the MNC to the BEA.[7]

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[5] Suppose that there are two U.S. multinational firms (A and B) and each has $900 of unremitted foreign earnings and pays $100 of foreign taxes. In addition, both firms earn $650 ($1,000) after-tax (pre-tax) domestically. Upon repatriation of the earnings to the U.S., each firm will have a $250 repatriation tax liability. Assume that Firm A always asserts that its unremitted foreign earnings are PRE, whereas Firm B never makes this assertion. Under these facts, Firm A has an effective tax rate (ETR) of 22.5 percent (450/2000) whereas Firm B has an ETR of 35 percent [(450 + 250(accrued repatriation expense))/2000]. When earnings are repatriated in a subsequent period, Firm A will recognize an additional $250 tax expense but Firm B will not. As a result, Firm A's ETR will be higher than 22.5 percent, whereas Firm B will continue to have a 35 percent ETR. In addition, if at any point in the future Firm A ceases making the PRE designation (perhaps because it intends to repatriate current earnings), then its ETR will increase as compared to prior years. Notice that as a firm's PRE increases, the potential earnings impact of repatriation increases. Said another way, the more PRE a firm has, the less flexibility it has to repatriate without an unfavorable change in its ETR.

[6] In order to reduce the reporting burden, the BEA only requires affiliates to participate if its assets, sales, or net income (loss) exceed the threshold for the year. The thresholds are higher in non-benchmark years, thus requiring fewer affiliates to report. Thresholds are $7 million in 1999, $30 million in 2000–2003, and $10 million in 2004. During the 2000–2003 non-benchmark years, most of the financial and operating data that we observe for affiliates that are not required to participate in the survey are estimated by the BEA. We use only reported data, not estimated data, in our analyses.

We combine BEA data with other data sources to construct measures of reporting incentives. Specifically, we determine public versus private ownership status for our sample of BEA firms using Compustat and the LexisNexis Corporate Affiliations database.[8] For tests that feature only public firms, we calculate ERCs using CRSP data, the percentage of quarters the firm beat analyst forecasts using I/B/E/S data, and dedicated institutional ownership using the Bushee (1998) classification. We obtain PRE from SEC filings.

Table 1, Panel A describes our sample selection process. To construct our sample, we identify MNCs that appear in the BEA data and have at least one foreign affiliate reporting for six consecutive years from 1999 through 2004. These restrictions limit our sample to MNCs with significant foreign operations and create a time period that includes two benchmark years. A total of 4,840 MNCs (57,164 affiliates) appear at least once in the BEA data from 1999 to 2004. Of these, only 805 MNCs (33,196 affiliates) have at least one affiliate reporting in all six years. We further restrict the sample by eliminating ADRs and foreign incorporated parents that are not likely to be constrained by either U.S. tax law or U.S. financial reporting rules when paying dividends to U.S. entities; we also eliminate banks and utilities.[9] Finally, we eliminate pass-through entities (e.g., partnerships, LLCs, and REITS) since they are typically not subject to corporate income taxes. Our final sample consists of 577 MNCs (25,459 affiliates); 479 MNCs (23,669 affiliates) are public and 98 MNCs (1,790 affiliates) are private. From this balanced panel, we eliminate 285 firm-years for which we do not have all required regression variables for our multivariate analyses resulting in 3,177 firm-years. Table 1, Panel B suggests that broad industry representation is present in our sample, with a heavy emphasis on textiles, manufacturing, and retail.

IV. RESEARCH DESIGN

Our hypotheses predict that firms are more sensitive to the repatriation tax rate in the presence of reporting incentives. We test our hypotheses by empirically estimating annual repatriations as a function of the estimated repatriation tax rate, a proxy for reporting incentives, an interaction term, and economic incentives to repatriate as follows:

$$\begin{aligned} Repatriations = {} & \beta_0 + \beta_1 RepTR + \beta_2 Reporting\ Incentives + \beta_3 Reporting\ Incentives \times RepTR \\ & + \sum \beta_k Controls_k + \sum \beta_t Year_t + \sum \beta_j Industry_j + \varepsilon \end{aligned} \tag{1}$$

where:

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[7] For example, under the equity method of accounting used for BEA reporting, total assets of the U.S. parent includes the "net assets" of its foreign affiliates. However, this amount is reported as a separate line item on the balance sheet of the U.S. parent, allowing us to eliminate intercompany assets without introducing measurement error in our calculation of worldwide assets. For our sample of public firms, the correlation between worldwide assets using Compustat data and worldwide assets using BEA data is 0.98 ($p < 0.01$), and for net income the correlation is 0.99 ($p < 0.01$). Our repatriations measure only captures dividends paid to the U.S. parent directly from a foreign affiliate, which avoids a double-counting problem with this variable.

[8] We identify public companies in the BEA data by matching with Compustat on company name and verifying private ownership using the LexisNexus Corporate Affiliations database.

[9] Firms operating in these industries are likely to have more opportunities to avoid repatriation taxes by redeploying capital around the world, and they face different incentives than other firms because they are regulated. As a practical matter, we are also unable to compute many of our regression variables for banks because the BEA collects substantially less data for these entities than for other entities because they report to other government agencies.

TABLE 1

Sample Composition

Panel A: Bureau of Economic Analysis

	Affiliates	Parents
In BEA data between 1999 and 2004	57,164	4,840
Less parents without 6 consecutive years of data	(12,709)	(2,283)
Less parents without 6 consecutive years of affiliate reporting	(11,259)	(1,752)
Less ADRs	(519)	(22)
Less foreign incorporated parents	(1,399)	(48)
Less banks and insurance companies	(3,433)	(65)
Less pass-through entities	(409)	(10)
Less public entities not in Compustat for 6 consecutive years	(1,126)	(61)
Less firms missing BEA data	(147)	(8)
Less firms whose parents cannot be identified[a]	(704)	(14)
Total	25,459	577
Private Entities	1,790	98
Public Entities	23,669	479

Panel B: Industry Composition of Sample

	Private	Public
1. Mining and construction	Combined	3.5%
2. Food	10.2%	4.4%
3. Textiles, printing, and publishing	13.3%	12.1%
4. Chemicals	10.2%	8.4%
5. Pharmaceuticals	Combined	2.3%
6. Extractive industries	7.1%	5.0%
7. Durable manufacturing	22.5%	25.5%
8. Computers	5.1%	12.1%
9. Transportation	5.1%	5.4%
11. Retail	13.3%	16.5%
13. Insurance and real estate	Combined	Combined
14. Services	7.1%	3.8%
15. Other and combined low reporting industries[b]	6.1%	1.0%

[a] Includes four mutual companies, four firms that changed status during the sample period (i.e., public to private, foreign ownership to domestic ownership) and six firms whose parents we could not identify as domestic or foreign owned.

[b] Any industries with less than five observations are combined with the "Other" category.

Repatriations = current-year repatriations scaled by worldwide assets;

RepTR = estimate of the rate of tax the firm would owe to the U.S. if it repatriated all undistributed foreign earnings, computed as the U.S. statutory tax rate (35 percent) minus *Creditable Tax Rate*;[10] and

Reporting Incentives = 1 for firms that face relatively high reporting incentives, and 0 otherwise.

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[10] See Appendix A for a detailed definition of this variable. This measure improves upon existing estimates of the incremental taxes due upon repatriation that use current period foreign tax rates because it is estimated in a manner similar to the U.S. foreign tax credit computation under Section 902 of the Internal Revenue Code. A detailed example of this calculation is available from the authors.

See Appendix A for definitions of all other variables.

We estimate Equation (1) at the firm level because the variables of interest, *RepTR* and *Reporting Incentives*, are firm-level constructs. We use a Tobit model for this estimation because roughly 55 percent of the observations in our sample exhibit *Repatriations* of 0.[11] *RepTR* is a measure of the firm's repatriation tax rate. Consistent with existing literature, we expect a negative coefficient on *RepTR*. For our hypotheses, our focus is on the interaction between *RepTR* and *Reporting Incentives* that tests whether repatriation is more sensitive to the repatriation tax rate in the presence of reporting incentives.

We use three proxies for reporting incentives—*Public*, *CapMkt*, and *HighPre*. Our test of H1A uses *Public*, which equals 1 for firms with public equity, 0 otherwise.[12] Our test of H1B requires that we identify a sample of public firms with a high price-sensitivity to earnings. Since all public firms face some degree of capital market pressure from various sources that create this sensitivity, we construct a composite measure that allows us to simultaneously consider multiple sources of capital market pressure: (1) investor responsiveness to earnings, estimated using earnings response coefficients (hereafter, *ERC*); (2) pressure to beat analyst forecasts, estimated by the proportion of quarters that the firm appears in I/B/E/S for which it beat the median analyst EPS forecast by one cent or less (hereafter, *BEAT*); and (3) the likelihood of investor-induced myopic behavior of managers, estimated by the proportion of stock owned by dedicated institutions (hereafter, *DED*).[13] *CapMkt* equals 1 for firms with an *ERC* in the top quartile of the sample, *BEAT* in the top quartile, or *DED* in the bottom quartile, 0 otherwise.[14] This approach increases the power of our test while allowing us to maintain a sufficient number of observations in both high ($CapMkt = 1$) and low ($CapMkt = 0$) reporting incentive subsamples.

To test H2 we must identify a subsample of public firms with the most significant unrecognized repatriation tax expense and therefore the greatest potential earnings effect of repatriation. We identify these firms by setting *HighPre* equal to 1 if PRE equals or exceeds total assets in low-tax affiliates (i.e., affiliates with *Affiliate Creditable Tax Rate*, defined in Appendix A, less than or equal to 30 percent) in either 1999 or 2004, and 0 otherwise.[15]

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[11] Tobin (1958) originally introduced the Tobit model in the context of censored data, i.e., where data on the dependent variable are lost (or limited) but data on the independent variables are not. Since the seminal work of Tobin, econometricians have clarified that the Tobit model is also appropriately used to analyze "corner solutions," a data structure similar to censored data where the dependent variable takes on the value of zero for a significant proportion of the sample (Wooldridge 2002; Greene 2003). Note that in the corner solution setting, the issue is not the inability to observe data. Numerous public finance economists have used the Tobit model in the context of repatriations (e.g., Hines and Hubbard 1990; Altshuler and Newlon 1993).

[12] Note that we eliminate less than ten MNCs in the BEA data that have public debt but not public equity as it was unclear whether these firms should be classified as public or private firms.

[13] To estimate ERCs, we measure the relation between accounting earnings and security returns using the association, or long-window, approach (e.g., Collins and Kothari 1989; Kormendi and Lipe 1987). Chaney and Jeter (1992) document that long-window rather than short-window ERCs better measure this relation for large firms because there is a greater flow of nonaccounting information between releases of earnings reports.

[14] While this measure incorporates multiple reasons a firm might face capital market pressure into a single variable and therefore captures more information about reporting incentives, it places equal weight on each variable, potentially introducing measurement error. To address this potential, we report results using each component of *CapMkt* separately in the "Earnings Measures of Reporting Incentives" section.

[15] Scaling PRE by assets in low-tax affiliates assumes that all PRE is in low-tax affiliates. In sensitivity tests we scale PRE by total foreign assets, which assumes that PRE is allocated proportionately across high- and low-tax affiliates. Our results are not sensitive to scaling PRE by total foreign assets or other definitions of this variable as discussed in the "Earnings Measures of Reporting Incentives" section. We use benchmark years (1999 and 2004) to define *HighPre* because MNCs disclose PRE at the firm-level in SEC filings, but report foreign assets at the affiliate-level in BEA data. More affiliates report in benchmark years because BEA reporting thresholds are lower in those years (described in Section III). Using only benchmark years to define PRE allows us to compare a firm-level variable to the most comprehensive estimate of foreign assets. Since the use of PRE is highly persistent, i.e., firms seldom report decreases in PRE, we identify firms that use PRE extensively as those that meet this threshold in either 1999 or 2004.

Three factors motivate our definition of *HighPre*. First, we assume that most PRE relates to activity in low-tax affiliates. Firms have no clear motivation for invoking the PRE designation for foreign earnings that they do not expect to generate a repatriation tax expense upon repatriation. Therefore, as PRE increases relative to assets in low-tax affiliates, the likelihood that a firm can repatriate without recognizing a repatriation tax expense decreases. Second, using a high threshold as a benchmark to construct an indicator variable, rather than using a continuous measure of PRE, ensures that repatriation will have an earnings effect for the subsample of firms we label *HighPre*. Firms with some undistributed foreign earnings that are *not* PRE have the ability to repatriate with no earnings effect.[16] Third, PRE technically arises from basis differences for book and tax purposes; a significant proportion of basis differences arise from undistributed foreign earnings, but these differences can arise from other sources such as the purchase of an affiliate that results in different book and tax bases in the firm's stock. Using assets allows for the possibility that some PRE arises from basis differences other than foreign retained earnings.[17]

Because public firms (i.e., $Public = 1$) face greater capital market pressure than private firms, public firms with high price-sensitivity to earnings (i.e., $CapMkt = 1$) face a more negative stock price response to a dollar of a repatriation tax expense than other public firms, and public firms with high amounts of PRE ($HighPre = 1$) deferred recognition of more repatriation tax expense. H1A, H1B, and H2 predict that repatriation by these firms is more sensitive to the repatriation tax rate than repatriation by other firms. All else equal, we expect negative coefficients on $Public \times RepTR$, $CapMkt \times RepTR$, and $HighPre \times RepTR$.

In addition to our variables of interest, we include controls for other factors that influence repatriation decisions. First, we include controls for investment opportunities abroad and in the U.S. (*Foreign ROA* and *U.S. ROA*, respectively). Hartman (1985) argues that firms will invest earnings in operations with the highest after-tax return by repatriating only when the after-tax return on foreign investment is less than the after-tax return on domestic investment. This theoretical result suggests that repatriations are negatively related to foreign investment opportunities and positively related to domestic investment opportunities. However, firms with more profitable foreign operations have more funds available to repatriate, suggesting a positive coefficient on *Foreign ROA*. Likewise, firms with profitable domestic operations have more domestic funds available for investment in the U.S., suggesting a negative coefficient on *U.S.ROA*. Thus, we make no prediction for the coefficients on *U.S. ROA* or *Foreign ROA*.[18]

We include *Indirect Ownership* and *Holding Company* to control for cross-sectional variation in the sophistication of firms' international tax-planning strategies. These variables control for a potential self-selection problem with respect to *Public* and *HighPre*. Specifically, evidence exists that public and private firms face different financial and tax reporting trade-offs that affect the type and amount of tax planning they undertake. For example, Beatty and Harris (1999) and Mikhail (1999) find evidence that private firms are more aggressive tax planners than public firms. In addition, Hanlon et al. (2007) link private ownership to greater proposed income tax deficiencies with the IRS than public ownership. Further, some firms may designate foreign earnings as PRE because they are invested in sophisticated tax-planning strategies. These relationships could bias the coefficients on *Public* × *RepTR* and *HighPre* × *RepTR* up or down depending on the effect of tax planning on repatriations.

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[16] See footnote 5 for a specific example.

[17] The Indefinite Reversal Exception applies broadly to outside basis differences, i.e., differences in the book and tax bases of a firm's stock in its affiliate (BNA 2010). Many outside basis differences have potential U.S. tax consequences upon repatriation but are not reflected in retained earnings. Therefore, assets better captures the broader concept of PRE than retained earnings.

[18] Hartman (1985) suggests that the difference between these two variables, *U.S. ROA* – *Foreign ROA*, should be included as an explanatory variable. We include them separately, which is similar to including the difference but allows the coefficients on the two variables to differ.

Sophisticated tax planning will increase repatriations if the strategy allows for tax-efficient repatriation. For example, Altshuler and Grubert (2003) describe strategies that allow the U.S. parent to effectively convert income earned in low-tax affiliates into high-tax income that can be repatriated tax free. Conversely, tax planning will decrease repatriations if the strategy calls for long-duration foreign investment projects to defer the U.S. tax liability (Desai et al. 2003). The strategies described in Altshuler and Grubert (2003) and Desai et al. (2003) use tiered-ownership in which the parent indirectly owns a foreign affiliate through another foreign affiliate. These strategies also use indirect ownership of a foreign affiliate through a holding company, which is an affiliate that does not produce goods or services, but is created solely to own another affiliate. Thus, we control for the sophistication of a firm's tax planning using two measures of the extent to which the firm uses tiered-ownership. Following Desai et al. (2003), we define *Indirect Ownership* as the proportion of the firm's foreign affiliates that are indirectly owned. Similarly, we define *Holding Company* as the proportion of the firm's foreign affiliates that are holding companies (i.e., those with North American Industry Classification System code 5512). Because these tax-planning strategies can increase or decrease repatriations, we do not make a sign prediction for these variables.

Equation (1) also includes controls for the size of foreign operations, agency costs, financing constraints, and costs of financing. *Size* equals the natural log of foreign sales. We expect a positive coefficient on *Size* because firms with larger foreign operations have more funds available to repatriate. We include *U.S. Leverage* and *Foreign Leverage* to control for firms' debt service needs and capital structure. Firms may be more reluctant to repatriate if they have high foreign debt service, suggesting a negative association between *Foreign Leverage* and repatriations. We do not make a prediction for *U.S. Leverage* because two opposing forces influence its relation with repatriations. *U.S. Leverage* will be positively associated with repatriations if firms distribute funds to service domestic debt. Alternatively, *U.S. Leverage* will be negatively associated with repatriations if domestic debt is used as a substitute for cash from repatriations, or if firms borrow domestically against undistributed foreign earnings.

We include *U.S. Interest Rate*, the interest rate on domestic debt, to control for the domestic cost of borrowing. We anticipate that firms with a higher cost of borrowing in the U.S. have higher repatriations. We include *Foreign Interest Rate*, the interest rate on foreign debt, to control for the foreign cost of borrowing. We expect that firms with a higher cost of borrowing abroad repatriate less. *%Foreign Assets* controls for the size of foreign operations relative to the size of the firm and we expect a positive coefficient on *%Foreign Assets*. We include *U.S. Loss* because Altshuler and Newlon (1993) and Power and Silverstein (2007) find that firms with a domestic net operating loss (NOL) are less likely to repatriate because repatriations convert domestic NOLs into foreign tax credits (FTCs).[19] U.S. MNCs are reluctant to give up the NOL in exchange for an FTC because the FTC has a shorter carryforward period.

Finally, we include four measures to control for country-level factors that affect repatriations. We first determine each country-level measure at the affiliate level, and then we calculate the parent-level score as the average of the affiliate scores. *Mandatory Dividend* equals 1 if the country requires dividends to be paid to shareholders, and 0 otherwise (La Porta et al. 1998). *Corruption* is an index taking values from 1 through 10, with 10 representing the lowest level of corruption (La Porta et al. 1998). Corruption will decrease repatriations if it increases the cost of doing business such that more capital must remain in the country; alternatively corruption may increase repatriations to reduce the probability of expropriation or theft. *Infrastructure* is an index taking values of 1 through 10, with 10 representing the most developed infrastructure (La Porta et al. 1999). All else equal, the less developed a country's infrastructure, the more capital the company likely needs to invest in its operations. Finally, *Capital Control* is equal to 1 if the country restricts a firm's ability to take funds out of the country, and 0 otherwise (International Monetary Fund [IMF] 2006).

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[19] Repatriation by a firm with a domestic NOL does not incur a repatriation tax because the NOL offsets the taxable income generated by the repatriation. However, because there is no current U.S. tax liability, U.S. tax law does not permit the firm to use any FTC associated with the repatriation, but instead requires it to create a FTC carryforward. Thus, when a firm with a NOL repatriates foreign earnings, ordering rules require it to use some of its NOL and create a FTC carryforward.

V. RESULTS

Descriptive Statistics

Table 2 provides descriptive statistics for our BEA sample. We report all statistics at the firm level unless otherwise noted and show statistics separately for private firms, low PRE public firms (*HighPre* = 0), and high PRE public firms (*HighPre* = 1). We winsorize all continuous measures at the top and bottom 1 percent and truncate all tax rate measures to fall between 0 and 100 percent. Finally, due to confidentiality restrictions, reported medians (Med5) represent the mean of the five middle observations.

Table 2, Panel A reports descriptive statistics for the variables in Equation (1). As expected, public and private firms exhibit several notable differences. Public firms have more extensive foreign operations that are larger overall (mean *Size* equals 13.50 for private firms, 14.74 for low PRE Public firms, and 15.14 for high PRE public firms), and they engage in more sophisticated tax planning (mean *Indirect Ownership* and *Holding Company* are 11.14 and 2.26 for private firms, 19.35 and 5.11 for low PRE public firms, and 24.92 and 7.02 for high PRE public firms). While *Foreign Leverage* is similar across the three subsamples, public firms have significantly higher *U.S. Leverage* than private firms. This difference likely results because, per our definition of *Public*, public firms in our sample can have public debt, while private firms cannot and existing research shows that firms with public debt have, on average, 35 percent more debt than firms with only private debt (Faulkender and Petersen 2006). Our multivariate tests control for these differences across public and private firms.

Within the sample of public firms, approximately 19 percent (506 observations) have *HighPre* equal to 1. Interestingly, *Repatriations* are largest for high PRE firms. The spirit of the Indefinite Reversal Exception, requiring firms to declare that foreign earnings are indefinitely invested abroad, suggests that high PRE firms might exhibit smaller *Repatriations* than other firms. However, *HighPre* is measured as a proportion of assets that are not repatriated. Therefore, high PRE firms can have higher repatriations, on average, but designate a greater proportion of earnings that are not repatriated as PRE. This result could arise if high PRE firms have made more profitable investments that generate more income and, in turn, generate more repatriations. It could also arise if high PRE firms are more likely to borrow in the U.S. using foreign assets as collateral and repatriate more foreign earnings to service U.S. debt. Our interest, however, is in whether high PRE firms are more sensitive to the repatriation tax rate than low PRE firms. As with differences across public and private firms, we control for observable differences across high PRE and low PRE firms. To reduce the possibility that unobservable differences drive our results, we use multiple proxies for reporting incentives and conduct a battery of sensitivity tests.

Table 2, Panel B reports descriptive statistics to compare tax rate measures used in existing research. *DFH Tax Rate* equals the median accounting effective tax rate for all U.S. affiliates operating in a country-year as described in Desai et al. (2001), and *Current FTR* equals current-

TABLE 2
Descriptive Statistics

Panel A: Regression Variables

	Private **n = 522**			**Low PRE Public** **n = 2,149**			**High PRE Public** **n = 506**		
Variable	**Mean**	**Med5**	**Std. Dev.**	**Mean**	**Med5**	**Std. Dev.**	**Mean**	**Med5**	**Std. Dev.**
Repatriations (%)	0.90**	0	2.34	0.74***	0	2.09	1.25	0.03	2.59
RepTR (%)	10.26###	6.97	10.49	11.48*	10.14	10.32	10.64	10.11	9.55
Foreign ROA (%)	7.13###	4.71	10.01	5.07***	4.30	7.74	6.51	5.52	7.83
U.S. ROA (%)	5.75###	4.08	9.95	4.03***	4.46	9.90	6.72	6.40	9.54
Size	13.50***,###	13.66	1.40	14.74***	14.64	1.49	15.14	15.12	1.36
Indirect Ownership (%)	11.14***,###	0	18.50	19.35***	9.14	23.27	24.92	20.39	23.31
Holding Company (%)	2.26***,###	0	4.75	5.11***	1.30	7.08	7.02	6.20	6.94
U.S. Leverage (%)	46.07**,#	31.41	129.46	59.40	34.37	142.79	68.19	39.21	152.16
Foreign Leverage (%)	47.49	45.40	23.80	49.12*	47.52	20.50	47.21	46.34	18.76
U.S. Interest Rate (%)	7.18	5.33	10.68	8.11*	5.55	12.16	7.05	5.38	8.42
Foreign Interest Rate (%)	1.94***,#	1.91	1.55	2.00***	1.70	1.72	1.68	1.58	1.27
%Foreign Assets	51.92***,###	33.10	49.86	66.40***	42.39	76.24	74.96	47.19	69.51
U.S. Loss (%)	19.16**	0	39.39	19.26	0	39.45	14.03	0	34.77
Mandatory Dividend (%)	1.04***,###	0	3.49	1.87***	0	4.15	1.69	0	2.33
Corruption	6.78	7.06	2.62	6.61	6.74	2.01	6.57	6.72	1.49
Infrastructure	5.66#	5.85	2.37	5.47	5.60	1.86	5.54	5.69	1.32
Capital Control (%)	4.71	0	11.77	5.24	0	10.67	5.62	3.27	8.73

Panel B: Tax Rate Estimates

	Private **n = 522**			**Low PRE Public** **n = 2,149**			**High PRE Public** **n = 506**		
Variable	**Mean**	**Med5**	**Std. Dev.**	**Mean**	**Med5**	**Std. Dev.**	**Mean**	**Med5**	**Std. Dev.**
DFH Tax Rate (%)	27.50***,###	28.01	7.18	25.83	26.38	6.61	25.98	26.08	4.75
Current FTR (%)	24.60***	20.43	21.94	25.88**	22.65	23.10	28.05	25.88	20.71
Creditable Tax Rate (%)	29.53##	27.90	18.61	27.66	24.84	18.22	27.94	24.85	16.29

(continued on next page)

TABLE 2 (continued)

Panel C: Other Descriptive Data

	Private n = 522			Low PRE Public n = 2,149			High PRE Public n = 506		
Variable	**Mean**	**Med5**	**Std. Dev.**	**Mean**	**Med5**	**Std. Dev.**	**Mean**	**Med5**	**Std. Dev.**
Foreign Sales	1,878.21***,###	588.30	4,959.04	6,024.99	1,637.88	15,684.76	5,808.07	2,388.84	9,965.03
U.S. Sales	775.33***,###	151.44	2,865.00	3,012.23	465.87	12,730.69	3,744.51	905.49	8,970.76
Affiliates	9.08***,###	3	24.17	22.08***	8	49.23	29.23	16.4	40.79
High Affiliates	2.15***,###	1	4.51	5.46***	2	10.82	8.16	4	11.26
PRE/Low Affiliate Assets	NA	NA	NA	26.97***	6.66	34.81	68.84	87.07	36.18
Affiliate RepDum (%)	14.66***,##	0	26.16	10.77***	0	19.35	13.97	5.32	19.34
Parent RepDum (%)	37.93###	0	48.57	43.14***	0	49.54	56.13	1	49.67

*, **, *** (#, ##, ###) Represent significant differences between the high PRE (low PRE) public firms at the 10 percent, 5 percent, and 1 percent two-tailed levels, respectively. This table reports firm attributes using data reported on the BEA Survey Form 10, Form 11, and Form 577 from 1999 to 2004. See Section III for BEA Survey details. Private firms are parent-affiliate groups with privately owned parents as described in the LexisNexis Corporate Affiliations database. Public firms are parent-affiliate groups with parents listed on a U.S. stock exchange. Low PRE public firms are those where the ratio of *PRE/Low Affiliate Assets* < 1 (i.e., *HighPre* $= 0$). High PRE public firms are those where the ratio of *PRE/Low Affiliate Assets* ≥ 1 (i.e., *HighPre* $= 1$). Med5 or Median5 represents the average of the five median observations. We report all statistics at the parent level.
All other variables are defined in Appendix A.

Variable Definitions:
DFH Tax Rate = median of all affiliate tax rates for all U.S. affiliates operating in a country-year as described in Desai et al. (2001);
Current FTR = current-period foreign income taxes paid over foreign pre-tax income for all affiliates in a group;
Creditable Tax Rate = estimates the U.S. parent's creditable taxes upon repatriation estimated as the average of the affiliates' creditable tax rates. The affiliate's creditable tax rate (*Affiliate Creditable Tax Rate*) equals aggregate foreign taxes paid on undistributed foreign earnings from the year the affiliate first enters the BEA sample or 1982, whichever is later, to the current year divided by undistributed pre-tax foreign earnings aggregated over the same period;
U.S. (Foreign) Sales = total domestic (foreign) sales (in millions);
Affiliates = total number of affiliates in a parent-affiliate group;
High Affiliates = total number of affiliates in a parent-affiliate group with *Affiliate Creditable Tax Rate* greater than or equal to 30 percent;
PRE/Low Affiliate Assets = ratio of permanently reinvested earnings reported in the 10-K to total assets in low-tax affiliates;
Affiliate RepDum = proportion of affiliates in a group that pay a dividend to the U.S. parent; and
Parent RepDum = proportion of parents that repatriate from at least one of their foreign affiliates.

period foreign income taxes paid over foreign pre-tax income. Public firms have a lower *DFH Tax Rate* than private firms, consistent with survey evidence in Graham et al. (2011) that the benefit of tax expense deferral is of primary importance in investment location decisions of public companies. Consistent with private firms facing higher income tax burdens and lower expected repatriation tax burdens, *Creditable Tax Rate* is higher for private firms than for both subsamples of public firms. However, *Current FTR* is lowest for private firms. A low *Current FTR* but high *Creditable Tax Rate* suggests that a non-trivial proportion of undistributed earnings were generated in the past when foreign tax rates were relatively higher than the U.S. tax rate. Table 2, Panel C reports general descriptive statistics for the BEA data. Public firms are larger than private firms in terms of number of affiliates and total sales. We control for these differences in our regression analysis.

Capital Market Measures of Reporting Incentives

Table 3, Column (1) reports the results of estimating Equation (1), excluding a measure of *Reporting Incentives*, for our sample of 577 public and private firms with necessary data from 1999 through 2004. Column (1a) reports the marginal effects. Consistent with existing literature, the coefficient on *RepTR* is negative and significant. Table 3, Column (2) reports the results of estimating Equation (1) using *Public* as a proxy for *Reporting Incentives*. The coefficient on *Public* is not significantly different from zero suggesting that, holding the repatriation tax rate constant, public companies are no less likely to repatriate, on average, than private companies. Consistent with H1A, the interaction between *Public* and *RepTR* is negative and significant (–0.0398; $p < 0.10$) suggesting that public firm repatriation is more sensitive to the repatriation tax rate than private firm repatriation.[20]

Table 3, Column (2a) reports the marginal effect of each variable on the unconditional expected value of *Repatriations*, which we use to interpret the coefficient estimates. We calculate the marginal effect of *Public* × *RepTR* using Ai and Norton (2003) and McDonald and Moffitt (1980).[21] The marginal effect of *Public* × *RepTR* on the unconditional expected value of *Repatriations* is –0.0151. This estimate suggests that the change in *Repatriations* when *RepTR* changes from 0 to the sample mean of 0.1115 is 0.168 percent of assets lower for public firms than for private firms. Mean worldwide assets for our sample is \$8.34 billion. Therefore, the average public firm repatriates \$14 million ($-0.0151 \times 0.1115 \times \$8{,}344 = \$14$), or 3.4 percent of foreign earnings less per year than the average private firm. For our 479 public firms, this figure amounts to \$6.7 billion in repatriations per year.

The coefficient on *U.S. ROA* is insignificant while the coefficient on *Foreign ROA* is positive and significant. These results suggest that firms with more profitable foreign operations have more cash available to repatriate and that this effect outweighs the investment opportunity effect predicted by Hartman (1985). However, for *U.S. ROA*, the investment opportunity effect is offset by a lower need for cash. *Size*, *Lag Repatriations*, *U.S. Leverage*, *Foreign Interest*, *%Foreign Assets*, and *Mandatory Dividend* are significant in the expected direction. The positive coefficient on *U.S. Leverage* suggests that firms repatriate foreign earnings, in part, to service domestic debt while the negative coefficient on *Foreign Interest* suggests that firms with a higher cost of borrowing abroad repatriate less, in part, to service foreign debt. We interpret the positive

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[20] Results in Table 3 and Table 4 are robust to scaling repatriations by worldwide sales or worldwide retained earnings. Results in Table 3 and Table 4, Columns (2) and (3) are also robust to using the log of repatriations as the dependent variable.

[21] A detailed discussion of this calculation is available upon request from the authors. This discussion includes an explanation of the McDonald and Moffitt (1980) decomposition of Tobit coefficients, as well as our STATA code for estimating the marginal effects of interaction terms.

TABLE 3

Impact of the Capital Market Measures on *Repatriations*
Dependent Variable = *Repatriations*

	(1)	(1a)	(2)	(2a)	(3)	(3a)
RepTR	−0.0311***	−0.0115	0.0010	0.0004	−0.0220**	−0.0080
	(0.0083)		(0.0226)		(0.0102)	
Public			−0.0013	−0.0005		
			(0.0036)			
Public × RepTR			−0.0398*	−0.0151		
			(0.0239)			
CapMkt					−0.0005	−0.0002
					(0.0021)	
CapMkt × RepTR					−0.0350**	−0.0123
					(0.0149)	
Foreign ROA	0.0751***	0.0278	0.0723***	0.0267	0.0849***	0.0309
	(0.0130)		(0.0129)		(0.0144)	
U.S. ROA	0.0137	0.0051	0.0114	0.0042	0.0160	0.0058
	(0.0121)		(0.0122)		(0.0135)	
Size	0.0049***	0.0018	0.0052***	0.0019	0.0056***	0.0020
	(0.0006)		(0.0006)		(0.0007)	
Lag Repatriations	0.6323***	0.2343	0.6314***	0.2330	0.6008***	0.2187
	(0.0613)		(0.0610)		(0.0666)	
Indirect Ownership	−0.0013	−0.0005	−0.0011	−0.0004	−0.0040	−0.0014
	(0.0047)		(0.0047)		(0.0042)	
Holding Company	−0.0079	−0.0029	−0.0046	−0.0017	−0.0015	−0.0001
	(0.0134)		(0.0134)		(0.0129)	
U.S. Leverage	0.0010*	0.0004	0.0011*	0.0004	0.0016***	0.0006
	(0.0006)		(0.0006)		(0.0005)	
Foreign Leverage	−0.0007	−0.0003	−0.0009	−0.0003	0.0004	0.0001
	(0.0037)		(0.0037)		(0.0040)	
U.S. Interest Rate	0.0029	0.0011	0.0038	0.0014	0.0030	0.0011
	(0.0071)		(0.0073)		(0.0075)	
Foreign Interest Rate	−0.0906**	−0.0336	−0.0951**	−0.0351	−0.0826**	−0.0300
	(0.0399)		(0.0404)		(0.0419)	
%Foreign Assets	0.0089***	0.0033	0.0091***	0.0034	0.0086***	0.0031
	(0.0015)		(0.0014)		(0.0014)	
U.S. Loss	0.0015	0.0006	0.0012	0.0004	0.0025	0.0009
	(0.0025)		(0.0024)		(0.0025)	
Mandatory Dividend	0.0444**	0.0165	0.0479***	0.0177	0.0443**	0.0161
	(0.0181)		(0.0178)		(0.0201)	
Corruption	0.0053	0.0020	0.0049	0.0018	−0.0049	−0.0018
	(0.0118)		(0.0116)		(0.0112)	
Infrastructure	−0.0121	−0.0045	−0.0114	−0.0042	0.0020	0.0007
	(0.0137)		(0.0132)		(0.0121)	
Capital Control	−0.0003	−0.0001	−0.0011	−0.0004	0.0010	0.0004
	(0.0093)		(0.0090)		(0.0102)	
Intercept	−0.0808***	−0.0299	−0.0854***	−0.0315	−0.0945	−0.0397
	(0.0112)		(0.0113)		(0.0117)	
Scale	0.0274		0.0274		0.0259	
	(0.0016)		(0.0016)		(0.0016)	

(continued on next page)

TABLE 3 (continued)

	(1)	(1a)	(2)	(2a)	(3)	(3a)
Year Fixed Effects	Yes		Yes		Yes	
Industry Fixed Effects	Yes		Yes		Yes	
Log Likelihood	2,314.21		2,321.86		2,118.46	
n	3,177		3,177		2,655	

*, **, *** Significant at the 10 percent, 5 percent, and 1 percent two-tailed levels, respectively.
Robust standard errors are reported in parentheses.
Columns (1), (2), and (3) report coefficients from the Tobit estimation. Columns (1a), (2a), and (3a) report the marginal effect of each variable on the unconditional expected value of *Repatriations*. Details on these calculations are available from the authors. See Appendix A for variable definitions.

coefficient on *%Foreign Assets* as suggesting that firms with proportionately greater foreign assets repatriate a greater proportion of total assets.

Table 3, Column (3) reports the results of estimating Equation (1) for our sample of 479 public firms using *CapMkt* as a proxy for *Reporting Incentives*. This test reduces the possibility that unobservable differences between public and private firms, such as access to capital, affect the sensitivity of repatriation to tax costs, and instead focuses on cross-sectional variation in capital market pressure among public firms. Recall that *CapMkt* equals 1 for public firms for which the *ERC* is in the top quartile of the sample, *BEAT* is in the top quartile, or *DED* is in the bottom quartile, 0 otherwise.[22] Consistent with H2, the interaction between *CapMkt* and *RepTR* is negative and significant (–0.0350; $p < 0.05$) suggesting that repatriation by public firms with a potentially larger stock price response to the repatriation tax expense is more sensitive to the repatriation tax rate relative to repatriation by other public firms. Inferences are similar if we test each condition separately, although only significant for two of the three conditions. If we define *CapMkt* as equal to 1 for firms in the top quartile of *ERC*, then the coefficient on *CapMkt* × *RepTR* is –0.0436 (standard error = 0.0174). If we define *CapMkt* as equal to 1 when *BEAT* is in the top quartile, then the coefficient on *CapMkt* × *RepTR* is –0.0338 (standard error = 0.0174). If we define *CapMkt* as equal to 1 if *DED* is in the bottom quartile, then the coefficient on *CapMkt* × *RepTR* is –0.0146 (standard error = 0.0149).[23]

The marginal effect of *CapMkt* × *RepTR* on the unconditional expected value of *Repatriations* is –0.0123. Mean worldwide assets for our public sample is $9.62 billion. Thus, the change in *Repatriations* when *RepTR* changes from 0 to the sample mean of 0.1133 is 0.139 percent of assets (–0.0123 × 0.1133) or $13.4 million (–0.0123 × 0.1133 × $9,616 = $13.4), or 2.8 percent of public firm foreign earnings lower for a firm with high capital market pressure than for a firm with low capital market pressure. This figure amounts to approximately $2.9 billion per year for our 1,298 high capital market pressure observations (($13.4 × 1,298)/6). In addition, the marginal effect of

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[22] Mean (median) *ERC* in our sample is 4.06 (2.67) with a standard deviation of 5.19. The mean (median) of *BEAT* is 23.5 percent (21.1 percent) with a standard deviation of 18.2 percent. The mean (median) of *DED* is 4.5 percent (0 percent) with a standard deviation of 7.4 percent. *CapMkt* equals 1 for 1,298 of the 2,655 observations in the sample of public firms.

[23] We also examine the robustness of these results to using different measures of *BEAT* to define *CapMkt*. Results are consistent when we define *BEAT* using three cents as a cutoff instead of one cent, using last year's earnings instead of analyst forecasts and both one cent and three cent cutoffs, and using annual instead of quarterly earnings. Results are also consistent if we define *BEAT* as equal to 1 if the number of consecutive years the firm met analyst forecasts by one cent or less, with a maximum of 5 years, is in the top quartile, similar to Barth et al. (1999). These measures also produce consistent results when we test them separately, except when we use last year's earnings as the benchmark with a three cent cutoff.

Public × *RepTR* is larger than the marginal effect of *RepTR* (–0.0080), consistent with MNCs placing at least as much importance on expense deferral relative to cash tax deferral and with the results in Graham et al. (2011).

Earnings Measures of Reporting Incentives

Table 4 reports the results of estimating Equation (1) for our sample of 479 public firms using *HighPre* as our measure of *Reporting Incentives*. *HighPre* × *RepTR* tests for the effect of reporting incentives on repatriation by examining whether repatriation by public firms that defer recognition of the repatriation tax expense is more sensitive to the repatriation tax rate relative to repatriation by other public firms. Consistent with H3, the coefficient on *HighPre* × *RepTR* is negative and significant (–0.0417; $p < 0.05$).[24] In Column (1a) we report the marginal effects. The marginal effect of *HighPre* × *RepTR* on the unconditional expected value of *Repatriations* is –0.0160. Thus, the change in *Repatriations* when *RepTR* changes from 0 to the sample mean of 0.1133 is 0.181 percent of assets (–0.0160 × 0.1133) or \$17.4 million (–0.0160 × 0.1133 × \$9,616 = \$17.4), or 3.6 percent of public firm foreign earnings lower for a high PRE public firm than for a low PRE public firm. This figure amounts to approximately \$1.5 billion per year for our 506 high PRE observations ((\$17.4 × 506)/6).

Collectively, our empirical results tell a consistent story in which reporting incentives deter repatriation. First, repatriation by public firms appears to be more sensitive to the repatriation tax rate than repatriation by private firms because public firms place relatively greater focus on accounting earnings. Second, repatriation by public firms that anticipate a more negative stock price reaction to the repatriation tax expense is more sensitive to the repatriation tax rate than repatriation by other public firms. Third, repatriation by public companies that rely extensively on PRE is more sensitive to the repatriation tax rate than repatriation by other public firms.[25]

VI. SUPPLEMENTAL ANALYSES

Firms with High PRE and High Capital Market Pressure

As discussed in Section IV, if public firms engage in different types and amounts of tax planning than private firms and/or high PRE firms engage in different types and amounts of tax planning than low PRE firms, then a self-selection problem could bias the coefficients on *Public* × *RepTR* or *HighPre* × *RepTR* up or down depending on the relationship between these tax-planning differences and repatriations. To control for this potential bias, we include *Indirect*

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[24] We evaluate the sensitivity of this result using many definitions of *HighPre*. Results are similar when we define *HighPre* as equal to 1 when PRE meets the following conditions in a benchmark year (1999 or 2004): the firm's ratio of PRE to total assets in low-tax affiliates is greater than or equal to 95 percent or 80 percent, the ratio of PRE to retained earnings in low-tax affiliates is greater than or equal to 1, 95 percent, or 80 percent, PRE divided by total foreign assets, worldwide assets, or worldwide retained earnings is greater than or equal to the 90th percentile (we use the 90th percentile for these scalars because this ratio is greater than 1 for only a few observations). We also evaluate the use of a continuous measure. Results are similar when we define *HighPre* as PRE divided by assets in low-tax affiliates, but are not significant when we define *HighPre* as PRE divided by total foreign assets ($t = -1.54$).

[25] An alternate interpretation of the negative coefficient on *HighPre* × *RepTR* is that high PRE firms lack the ability to repatriate because they are more heavily invested in operating assets abroad than other firms. We formally rule out this explanation in the "Foreign Cash Holdings" section by showing that, relative to other public firms, the cash holdings of high PRE firms increase more when the repatriation tax rate increases.

TABLE 4

Impact of the Earnings Effect on *Repatriations*
Dependent Variable = *Repatriations*

	(1)	(1a)	(2)	(2a)	(3)	(3a)
	All Public Firms		***CapMkt* = 1**		***CapMkt* = 0**	
RepTR	–0.0299***	–0.0109	–0.0489***	–0.0134	–0.0136	–0.0061
	(0.0092)		(0.0136)		(0.0117)	
HighPre	0.0059**	0.0023	0.0075	0.0023	0.0054	0.0026
	(0.0028)		(0.0046)		(0.0034)	
HighPre × *RepTR*	–0.0417**	–0.0160	–0.0576*	–0.0204	–0.0334	–0.0133
	(0.0180)		(0.0307)		(0.0231)	
Foreign ROA	0.0831***	0.0303	0.0919***	0.0251	0.0872***	0.0395
	(0.0143)		(0.0226)		(0.0176)	
U.S. ROA	0.0121	0.0044	–0.0073	–0.0020	0.0283**	0.0128
	(0.0133)		(0.0232)		(0.0142)	
Size	0.0059***	0.0022	0.0051***	0.0014	0.0060***	0.0027
	(0.0007)		(0.0008)		(0.0009)	
Lag Repatriations	0.6106***	0.2228	0.6684***	0.1828	0.5484***	0.2486
	(0.0658)		(0.1076)		(0.0805)	
Indirect Ownership	–0.0037	–0.0013	–0.0035	–0.0010	–0.0036	–0.0016
	(0.0043)		(0.0063)		(0.0062)	
Holding Company	–0.0018	–0.0007	0.0073	0.0020	–0.0056	–0.0026
	(0.0133)		(0.0259)		(0.0144)	
U.S. Leverage	0.0017***	0.0006	0.0005	0.0001	0.0022***	0.0010
	(0.0006)		(0.0008)		(0.0007)	
Foreign Leverage	0.0011	0.0004	–0.0039	–0.0011	0.0050	0.0023
	(0.0040)		(0.0061)		(0.0052)	
U.S. Interest Rate	0.0045	0.0016	0.0011	0.0003	0.0039	0.0018
	(0.0072)		(0.0132)		(0.0072)	
Foreign Interest Rate	–0.0813**	–0.0297	–0.0562	–0.0154	–0.0866	–0.0393
	(0.0419)		(0.0544)		(0.0564)	
%Foreign Assets	0.0084***	0.0031	0.0079***	0.0022	0.0091***	0.0041
	(0.0014)		(0.0018)		(0.0023)	
U.S. Loss	0.0019	0.0007	–0.0039	–0.0010	0.0057**	0.0027
	(0.0025)		(0.0047)		(0.0028)	
Mandatory Dividend	0.0450**	0.0164	0.0399**	0.0109	0.0323	0.0146
	(0.0198)		(0.0175)		(0.0374)	
Corruption	–0.0028	–0.0010	–0.0018	–0.0005	–0.0066	–0.0030
	(0.0111)		(0.0133)		(0.0178)	
Infrastructure	–0.0007	–0.0003	–0.0004	–0.0001	0.0000	0.0000
	(0.0121)		(0.0144)		(0.0197)	
Capital Control	0.0017	0.0006	0.0019	0.0005	–0.0027	–0.0012
	(0.0100)		(0.0156)		(0.0121)	
Intercept	–0.0985***	–0.0359	–0.0800***	–0.0219	–0.1063***	–0.0482
	(0.0118)		(0.0165)		(0.0163)	
Scale	0.0259		0.0261		0.0251	
	(0.0016)		(0.0024)		(0.0020)	

(continued on next page)

TABLE 4 (continued)

	(1)	(1a)	(2)	(2a)	(3)	(3a)
	All Public Firms		***CapMkt* = 1**		***CapMkt* = 0**	
Year Fixed Effects	Yes		Yes		Yes	
Industry Fixed Effects	Yes		Yes		Yes	
Log Likelihood	2,113.32		764.35		1,383.89	
n	2,655		1,298		1,357	

*, **, *** Significant at the 10 percent, 5 percent, and 1 percent two-tailed levels, respectively.
Robust standard errors are reported in parentheses.
Columns (1), (2), and (3) report coefficients from the Tobit estimation. Columns (1a), (2a), and (3a) report the marginal effect of each variable on the unconditional expected value of *Repatriations*. Details on these calculations are available from the authors. See Appendix A for variable definitions.

Ownership and *Holding Company* as control variables and run sensitivity tests using several other measures of tax-planning activity.[26] To further rule out this possibility, we investigate whether the higher sensitivity of repatriation to the repatriation tax rate for high PRE firms ($HighPre = 1$) is concentrated in firms facing greater capital market pressure ($CapMkt = 1$). We report these results in Table 4, Columns (2) and (3). Consistent with the earnings consequences of repatriation acting as a greater deterrent to repatriation in the presence of high capital market pressure, the interaction term, $HighPre \times RepTR$, is negative and significant for firms with $CapMkt = 1$ but is insignificant for firms with $CapMkt = 0$.[27,28]

Foreign Cash Holdings

Next, we evaluate the association between the unrecognized expense for repatriation taxes and foreign cash holdings for the public firms in our sample. Although we conjecture that reporting incentives lead to predictable differences in repatriation across high and low PRE firms, our results could still be attributable to varying investment opportunities. A public firm that designates earnings as PRE is declaring its intention to reinvest foreign earnings abroad indefinitely and may, as a result of investment opportunities, be more sensitive to the repatriation tax rate. To control for this possibility, we include measures of foreign and domestic after-tax return on assets in our main empirical tests. As a sensitivity test, we replace measures of foreign and domestic return on assets with proxies for anticipated domestic and foreign growth opportunities. To further rule out the possibility that our results are confounded by differences in investment opportunities, we follow Foley et al. (2007) and investigate the effect of tax expense deferral on foreign cash holdings.

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[26] Results are robust to using measures of tax aggressiveness from existing literature. Specifically, results are unchanged when we include a tax shelter score from Wilson (2009) or Lisowsky (2010). A tax shelter score is the predicted probability that a firm is engaging in a tax shelter, conditional on a set of firm characteristics. Those firm characteristics and the coefficients are documented by Wilson (2009) and Lisowsky (2010), who use samples of tax shelter participants to develop their models. Results are also unchanged when we include the percent of affiliates located in tax havens as a measure of tax aggressiveness, where tax havens are defined as in Hines and Rice (1994). In addition, the coefficients on each of these alternative measures are insignificant.

[27] Results are similar if we evaluate each condition used to define *CapMkt* separately. However, in these tests we use medians rather than quartiles to define each condition to create relatively equal sample sizes. When we define partitions based on the median of *ERC*, the coefficient on $HighPre \times RepTR$ is -0.0426 ($t = -1.92$) for observations above the median and -0.0382 ($t = -1.28$) for observations below the median. The coefficient on $HighPre \times RepTR$ is -0.0515 ($t = -2.48$) for observations with *BEAT* above the median and -0.0005 ($t = -0.02$) for observations with *BEAT* below the median. The coefficient on $HighPre \times RepTR$ is -0.0697 ($t = -2.13$) for observations with *DED* below the median and -0.0305 ($t = -1.27$) for observations with *DED* above the median.

[28] We avoid using a three-way interaction term because of the problems of interpreting such an effect in a Tobit model (Ai and Norton 2003).

Foley et al. (2007) find that repatriation taxes help explain the build-up of cash abroad and, more specifically, in affiliates located in low-tax jurisdictions. If an MNC designates foreign earnings as PRE because they are invested in profitable activities, then *HighPre* will have either a negative or insignificant effect on the association between the repatriation tax rate and foreign cash holdings because these profitable investments are not reflected in the cash account. However, to the extent that public companies designate foreign earnings as PRE because of the earnings effect of repatriations, we expect that these consequences will result in more cash held abroad, i.e., they will exacerbate the positive relation between the repatriation tax rate and foreign cash holdings.

To investigate this conjecture we estimate the following empirical model, which closely follows Foley et al. (2007), using ordinary least squares (OLS):[29]

$$\begin{aligned}LN(Cash/Net\,Assets) = {} & \beta_0 + \beta_1 RepTR + \beta_2 HighPre + \beta_3 HighPre \times RepTR \\ & + \sum \beta_k Controls_k + \sum \beta_t Year_t + \sum \beta_j Industry_j + \varepsilon. \qquad (2)\end{aligned}$$

LN(Cash/Net Assets) is the natural log of foreign cash holdings divided by non-cash worldwide assets consistent with Foley et al. (2007). *Foreign Investment* is foreign R&D plus foreign capital expenditures divided by worldwide assets. All other variables are defined as in Equation (1). We include *Foreign ROA* to control for investment opportunities abroad. As investment opportunities increase, we expect firms to hold less cash because the cash will be invested in productive assets. However, the precautionary motive to hold cash suggests that firms with better investment opportunities will hold more cash to ensure that they do not miss out on profitable investment opportunities due to adverse shocks (Opler et al. 1999). We include *U.S. ROA* to control for investment opportunities in the United States. We expect a negative coefficient if firms are more likely to repatriate when U.S. profitability increases and a positive coefficient if firms are less likely to repatriate when domestic operations are generating more cash. *Size* controls for the transaction motive to hold cash, which predicts that, since there are economies of scale when converting non-cash assets into cash, large firms hold less cash (Baumol 1952; Miller and Orr 1966). *Foreign Leverage* controls for the need to use cash to meet debt obligations and we expect a negative coefficient. *Foreign Investment* controls for the use of cash to invest in capital assets and research and development. We expect a negative coefficient to the extent that these investments consume cash and a positive coefficient to the extent that this variable proxies for growth opportunities (Bates et al. 2009). All other variables control for the effect of repatriations on cash, thus predictions are opposite those in Equation (1).

We report the results of estimating Equation (2) for our sample of public firms in Table 5. We expect and find that the coefficient on *HighPre* × *RepTR* is positive and significant.[30] This result is consistent with the view that undistributed foreign earnings are trapped abroad for firms with stronger reporting incentives and helps rule out the possibility that the results in Table 4 stem from higher investment opportunities abroad for high PRE firms. The insignificant coefficient on *Foreign ROA* suggests that the investment effect and precautionary motive offset one another. The positive

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[29] We use OLS to be consistent with Foley et al. (2007) and because it is not feasible that cash balances are negative, making Tobit inappropriate.

[30] This result is robust when we replace *Foreign ROA* and *U.S. ROA* with measures of foreign growth opportunities (three-year historical foreign sales growth by country and industry and weighted by affiliate assets in the affiliates country and industry) and domestic growth opportunities (three-year historical domestic sales growth by industry), respectively.

TABLE 5

Reporting Incentives and Foreign Cash Holdings Dependent Variable = *LN(Cash/Net Assets)*

	(1)	(2)
RepTR	−0.0655	−0.5739
	(0.0695)	(0.7519)
HighPre	0.2143	0.1849*
	(0.1447)	(0.1323)
HighPre × *RepTR*	2.0581*	2.5717**
	(1.2558)	(1.1810)
Foreign ROA	0.9738	0.8158
	(0.7001)	(0.6099)
U.S. ROA	0.2296	1.1130**
	(0.2296)	(0.4779)
Size	−0.0720**	−0.0438
	(0.0374)	(0.0351)
Foreign Leverage		0.0676
		(0.3596)
Foreign Investment		12.6904***
		(1.9205)
U.S. Loss		0.3523***
		(0.1094)
Mandatory Dividend		−0.8850
		(1.6526)
Corruption		1.0484
		(0.7305)
Infrastructure		−1.6615**
		(0.8244)
Capital Control		0.2548
		(0.4634)
Intercept	−3.8206***	−4.2266***
	(0.6458)	(0.6592)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Adj. R^2	12.32%	18.56%
n	2,655	2,655

*, **, *** Significant at the 10 percent, 5 percent, and 1 percent two-tailed levels, respectively.

Robust standard errors from OLS regressions are reported in parentheses.

LN(Cash/Net Assets) is the natural log of foreign cash divided by non-cash worldwide assets. *Foreign Investment* is foreign capital expenditures plus foreign R&D divided by worldwide assets. See Appendix A for definitions of all other variables.

coefficient on *U.S. ROA* in Column (2) suggests that domestically profitable firms' lower need for cash in the U.S. outweighs the investment effect, but only after including the full set of control variables. The negative coefficient on *Size* in Column (1) is consistent with the transaction motive to hold cash. However, this effect becomes insignificant in Column (2). The positive coefficient on *Foreign Investment* suggests that this variable is a proxy for growth opportunities and is consistent with existing research that finds that firms with better investment opportunities hold more cash (Opler et al. 1999).

VII. CONCLUSION

U.S. international tax policy plays a role in the ability of U.S. multinationals (MNCs) to compete in the global marketplace and various opponents of current tax policy argue that it creates incentives for U.S. firms to park foreign affiliate profits in low-tax countries, thereby reducing domestic investment. These incentives arise because MNCs incur a tax cost upon repatriation of foreign affiliate earnings generally equal to the difference between the U.S. tax rate and the average foreign tax rate paid on the repatriated earnings. A factor that has received little attention in the empirical literature on repatriation behavior is that the accounting for repatriation taxes can also affect repatriation decisions.

Financial reporting rules allow firms to delay recognition of a repatriation tax expense in the period in which the firm generates the earnings by designating them as permanently reinvested earnings (PRE) under the Indefinite Reversal Exception. Instead, the firm recognizes any unrecognized repatriation tax expense in the period that it repatriates those earnings. Firms that face reporting incentives to consistently report strong earnings numbers have an incentive to delay repatriation to avoid recognizing a repatriation tax expense in the financial statements.

We find that reporting incentives deter repatriations. Specifically, we find that repatriation by public firms is more sensitive to the repatriation tax rate than repatriation by private firms. In a sample of only public firms, we find that repatriation by firms with a high price-sensitivity to earnings and/or by firms that make extensive use of the PRE designation such that they would recognize a larger repatriation tax expense upon repatriation are more sensitive to the repatriation tax rate than repatriation by other firms. Our findings are relevant for both tax policy makers and accounting standard-setters because they suggest that the current financial reporting rules for the U.S. tax on foreign affiliate earnings deter repatriation and contribute to the nearly $1 trillion MNCs have parked outside the United States. In light of recent debates regarding the merits of the U.S. system of worldwide taxation with deferral and its contribution to the pool of funds overseas, our results suggest that merely eliminating deferral of income tax expense recognition could significantly reduce incentives to park profits outside the U.S. without eliminating deferral of income tax payments. However, as net income is often relied upon by market participants to evaluate firm performance, the elimination of the Indefinite Reversal Exception could adversely affect U.S. firms' international capital market competiveness.

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APPENDIX A

REGRESSION VARIABLES

Variable Name, Definition, and Source (BEA unless otherwise noted)

Dependent Variables

Repatriations = current-year repatriations scaled by worldwide assets.

Variables of Interest

Reporting Incentives:

H1A: *Public* = 1 if the firm has publicly traded equity, 0 otherwise;

H1B: *CapMkt* = 1 if the firm's *ERC* is in the top quartile of the sample, *BEAT* is in the top quartile, or *DED* is in the bottom quartile, 0 otherwise; and

H2: *HighPre* = 1 if the firm's ratio of permanently reinvested earnings divided by total assets in low-tax affiliates ≥ 1 in a benchmark year (1999 or 2004), 0 otherwise. Low-tax affiliates are those with *Affiliate Creditable Tax Rate* ≤ 30 percent.

ERC = firm's long-window *ERC* estimated by regressing 16-month cumulative abnormal returns on the change in annual earnings, excluding extraordinary items and discontinued operations, scaled by the market value of equity at the beginning of the year;

BEAT = proportion of quarters that the firm appears in I/B/E/S for which it beat the median analyst EPS forecast by 1 cent or less. We estimate *BEAT* beginning in the first year the firm appears in I/B/E/S or 1984, whichever is later, and ending in the current year;

DED = proportion of the firm's stock owned by long-term institutional investors (i.e., dedicated investors) as defined by Bushee (1998);

RepTR = our estimate of the rate of tax the firm would owe to the U.S. if it repatriated all undistributed foreign earnings, computed as the U.S. statutory rate (35 percent) minus *Creditable Tax Rate*;

Affiliate Creditable Tax Rate = for each year, we subtract dividends paid (repatriations) from pre-tax income to obtain an estimate of each affiliate's undistributed pre-tax income at the end of each year. We aggregate each affiliate's tax expense and undistributed pre-tax income over time beginning with the date the affiliate is first included in the BEA survey or 1982 (the first BEA survey), whichever comes last, and ending at the end of year *t*–1. This process results in an estimate of each affiliate's cumulative taxes paid on undistributed pre-tax income (CTP) and cumulative undistributed pre-tax income (CUPTI). *Affiliate Creditable Tax Rate* for year *t* equals year *t*–1 CTP divided by year *t*–1 CUPTI; and

Creditable Tax Rate = estimate of the ratio of an MNC's foreign tax credit to its pre-tax dividend if all undistributed earnings were repatriated. An MNC's year *t Creditable Tax Rate* equals the ratio of year *t*–1 CTP summed across all of its affiliates to year *t*–1 CUPTI summed across all of its affiliates.

Control Variables

Foreign ROA = foreign net income scaled by foreign assets;

U.S. ROA = domestic net income scaled by domestic assets;

Size = natural log of foreign sales;

Lag Repatriations = prior-year repatriations scaled by prior-year worldwide assets;

Indirect Ownership = ratio of indirectly owned affiliates to total affiliates;

Holding Company = ratio of holding company affiliates (NAICS code 5512) to total affiliates;

U.S. Leverage = ratio of domestic short-term and long-term debt to domestic assets;

Foreign Leverage = ratio of foreign short-term and long-term debt to foreign assets;

U.S. Interest Rate = ratio of domestic interest paid on domestic short-term and long-term debt;

Foreign Interest Rate = ratio of foreign interest paid divided by foreign short-term and long-term debt;

%Foreign Assets = ratio of foreign assets to worldwide assets;

U.S. Loss = 1 if the U.S. parent reports a domestic pre-tax loss in the current year, 0 otherwise;

Mandatory Dividend = country-level indicator variable equal to 1 if the country requires that dividends are paid to shareholders, 0 otherwise, weighted by the number of affiliates in each type of country (La Porta et al. 1998), available at: http://www.economics.harvard.edu/faculty/shleifer/dataset;

Corruption = country-level variable ranging from 1 to 10 indicating high and low corruption, respectively, weighted by the number of affiliates in each type of country (La Porta et al. 1999), available at: http://www.economics.harvard.edu/faculty/shleifer/dataset;

Infrastructure = country-level variable ranging from 1 to 10 indicating low and high quality infrastructure, respectively, weighted by the number of affiliates in each type of country (La Porta et al. 1999), available at: http://www.economics.harvard.edu/faculty/shleifer/dataset; and

Capital Control = country-level indicator variable equal to 1 if the country restricts firms' ability to take funds out of the country, 0 otherwise, weighted by the number of affiliates in each type of country (IMF 2006).

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The Effect of Hedge Fund Activism on Corporate Tax Avoidance

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ABSTRACT: This paper examines the impact of hedge fund activism on corporate tax avoidance. We find that relative to matched control firms, businesses targeted by hedge fund activists exhibit lower tax avoidance levels prior to hedge fund intervention, but experience increases in tax avoidance after the intervention. Moreover, findings suggest that the increase in tax avoidance is greater when activists have a successful track record of implementing tax changes and possess tax interest or knowledge as indicated by their Securities and Exchange Commission (SEC) 13D filings. We also find that these greater tax savings do not appear to result from an increased use of high-risk and potentially illegal tax strategies, such as sheltering. Taken together, the results suggest that shareholder monitoring of firms, in the form of hedge fund activism, improves tax efficiency.

Keywords: *tax avoidance; hedge fund activism; corporate governance.*

JEL Classifications: G32; G34; H26.

Data Availability: *Data are available from sources identified in the text.*

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I. INTRODUCTION

Ownership structure is an important, but under-studied, determinant of corporate tax policies (Shackelford and Shevlin 2001). Only recently have a few studies started to explore this area (e.g., Badertscher et al. 2010a, 2010b; Chen et al. 2010). This paper extends the literature by examining the role of hedge fund activists on the levels of corporate tax avoidance. Hedge fund activists have been playing an increasingly important role in capital markets and prompting profound changes in the way the corporate world operates (Kahan and Rock 2007; Brav et al. 2008, 2010; Klein and Zur 2009).[1] In this paper, we investigate whether this important and unique class of shareholders has any impact on corporate tax-planning efficiency.

There is significant variation in the level of corporate tax avoidance across publicly traded firms (e.g., Dyreng et al. 2008). While some of this variation is attributable to size and industry factors, a significant portion remains unexplained. One potential source for the unexplained variation could be due to differences in firm managers' incentives for tax planning. That is, managers' preference to limit the effort and risk associated with tax planning might contribute to the observed low levels of tax avoidance for some firms. Hedge fund activists can provide informed monitoring by pushing effort-averse firm managers to improve tax-planning efficiency. Hedge fund activists also tend to have concentrated investment portfolios, and the performance of an individual firm may significantly impact their portfolio returns (Kahan and Rock 2007; Brav et al. 2008). As a result, hedge fund activists have an incentive to exercise their influence to encourage individual firms to employ tax avoidance strategies that enhance after-tax cash flows and firm value.

Anecdotal evidence suggests that firms targeted by hedge funds tend to be tax inefficient, and activist hedge funds often demand efficiency improvements in tax and other areas from these target firms. For instance, in 2007, the hedge fund Third Point LLC targeted PDL Biopharma Inc., aiming to change the firm's business strategies and to remove its CEO, Mark McDade. When making its case to the board of directors, Third Point wrote: "Mr. McDade lacks the ability to communicate with the investment community effectively in part because he has a poor understanding of even basic financial concepts . . . He readily admitted to us that he has not properly thought through nor effectively utilized PDL's tax credits, which has and will result in reduced value for PDL shareholders." Calling the unused tax credits a "readily exploitable Company asset," Third Point identifies the firm's tax strategy as a potential source of value improvement. In another example, in October 2007, hedge fund activist Sandell Asset Management Corp. recommended to the target firm, Sybase Inc., that improving tax efficiency would provide opportunities for value creation. Specifically, the hedge fund activist stated that, "Given the company's stable base of maintenance revenue, a moderate level of leverage is more tax efficient and will improve returns on equity."[2]

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[1] This literature finds that hedge fund activists are successful in eliciting positive changes in target firms' corporate governance, business policies, and financial performance. For instance, prior studies document increases in dividend payout, CEO turnover, pay-for-performance sensitivity, and board representation in the target firms after hedge fund interventions. Please refer to Brav et al. (2010) for a detailed review.

[2] For details of these two examples, please refer to http://www.sec.gov/Archives/edgar/data/882104/000089140-07-001301.txt, and http://www.sec.gov/Archives/edgar/data/768262/000101359407000460/sybase13dppt-101207.htm. More examples are provided in Appendix A, in which hedge fund activists proposed to increase tax efficiency through strategies such as taking advantage of loss carryforwards, engaging in tax-efficient spin-offs, and adopting tax-beneficial organizational structures. However, hedge fund activists typically do not publicly discuss tax issues, probably due to the opaque nature and legal concerns of tax avoidance. In Section IV, we report that some activists exhibit interest and knowledge in tax planning in their public 13D filings. Furthermore, the results in Table 6 show that these activists have a greater impact on tax avoidance than other activists, even though firms targeted by both groups of activists experience significant increases in tax avoidance.

In this study, we collect a large sample of 2,981 hedge fund activist events from 1994 to 2008, using a hedge fund's filing of Schedule 13D as the initiation of hedge fund activism (Brav et al. 2008; Greenwood and Schor 2009; Klein and Zur 2009). We define tax avoidance as activities that reduce explicit taxes per dollar of pretax accounting earnings (Hanlon and Heitzman 2010), and infer a target firm's tax avoidance level from four commonly used metrics.[3] The first two metrics, based on effective tax rates (ETR), are measured as a firm's income tax expense and cash taxes paid scaled by pretax income, respectively. The other two measures reflect differences between a firm's reported income and taxable income (book-tax difference, or BTD.) They are the Manzon-Plesko (2002) book-tax difference, and a derivative BTD measure proposed by Desai and Dharmapala (2006) to capture differences resulting mainly from tax planning. To control for the endogeneity of hedge funds' targeting decisions and market-wide changes in tax avoidance, we follow prior studies (e.g., Li and Prabhala 2007; Lawrence et al. 2011) and use the propensity score matching method to construct a sample of control firms. The propensity score is the predicted probability of becoming a hedge fund activism target in the next year, estimated from a logistic regression model as in Brav et al. (2008). We adjust the values of all variables used in the analysis by subtracting the corresponding value for the matched firm from that for the sample firm, and use the differences for our empirical analysis. In the baseline analysis, we find that target firms have lower tax avoidance levels than control firms before hedge fund intervention. We also find that target firms experience increases in tax avoidance after the fund intervention compared with the matched control firms.

Because hedge fund interventions are often associated with profound and multi-dimensional effects on target firms (Brav et al. 2010), it is possible that indirect effects, arising from changes in other tax determinants such as operating, financing, and investment activities, drive the observed changes in tax avoidance. To isolate direct effects of hedge fund activists on corporate tax avoidance, we conduct several tests to adjust for such indirect effects. First, we construct various control samples matched on changes in operating, financing, and investment activities, and find these control samples do not experience similar increases in tax avoidance as target firms. Second, we employ a multivariate regression approach, which explicitly controls for other determinants of tax avoidance that may give rise to the indirect effects. We continue to find consistent results. Finally, we find that increases in tax avoidance in target firms depend on the associated hedge funds' past success in implementing tax changes. In addition, we create proxies for fund activists' interest in, and knowledge of, tax issues from their SEC filings. Using these proxies, we document a link between such fund characteristics and changes in target firms' tax avoidance.

We also conduct several additional analyses. Extending the event window, we find that increases in tax avoidance for target firms do not reverse over the five-year period after the intervention, consistent with hedge fund activists inducing at least intermediate-term changes in firms' tax strategies. Further, we analyze whether target firms' increased tax avoidance is associated with an increased use of high-risk and potentially illegal tax-planning strategies (e.g., tax sheltering). Using sheltering estimates from Wilson's (2009) and Lisowsky's (2010) sheltering models, we find no evidence that target firms are more likely to engage in tax sheltering after hedge fund intervention.

An alternative explanation for our results is that hedge fund activists could just be cherry-picking target firms that are already improving their tax avoidance. To address this concern, we conduct trend analyses and find that, on average, the target firms were experiencing a decreasing trend in tax avoidance prior to the activist event, while control firms do not exhibit this pattern.

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[3] To avoid the complexity of determining the legality or appropriateness of the mechanism used to reduce the tax burden, we follow the literature (e.g., Chen et al. 2010; Frank et al. 2009; Hanlon and Heitzman 2010) and define tax avoidance as a broad description of tax strategies that reduce taxes relative to book income, rather than a narrower construct such as tax evasion.

Collectively, our results suggest that the observed increases in target firms' tax avoidance levels are partially attributable to the direct effects of hedge fund activists on target firms' tax planning.

Our paper makes several contributions. First, we contribute to the growing literature on the effect of ownership structure on corporate tax avoidance. To the best of our knowledge, our paper is the first to document that hedge fund activists induce increases in target firms' tax avoidance. Second, we contribute to the current literature on the effect of hedge fund activists on target firms. Specifically, we find that the increases in tax avoidance are related to hedge fund characteristics (e.g., their past success, interest, and knowledge in target firms' tax planning), and there is no evidence of tax sheltering. These findings suggest that hedge funds' informed monitoring is associated with improvements in target firms' tax efficiencies, which increase both cash flows and firm value.[4]

The remainder of the paper is organized as follows. Section II reviews the related literature and develops the hypotheses. Section III discusses the sample and variable measurement. Section IV presents the primary results of the study, while Section V reports additional tests and the sensitivity of the results to alternative tests and specifications. Section VI concludes.

II. LITERATURE REVIEW AND HYPOTHESIS DEVELOPMENT

A number of recent papers identify hedge fund activism as an effective influence on shareholder monitoring by providing evidence of post-intervention improvements in governance structure, capital structure, and operational decisions. Brav et al. (2008) find that hedge funds are successful in mitigating agency costs and improving firm value. Target firms in their sample experience increases in payout, higher rates of CEO turnover, and improvements in operating performance and corporate governance.[5] Similarly, Klein and Zur (2009) report that hedge fund activists are successful in pressing target firms to repurchase stock, replace CEOs, and increase board representation of the funds. Bratton (2006), Briggs (2007), and Clifford (2008) show that hedge fund activists improve their target firms' short-term and long-term performance. Greenwood and Schor (2009), on the other hand, attribute positive returns of target firms to hedge funds' ability to force target firms into a takeover.[6]

Building upon this literature, we argue that hedge fund activists have the incentive and the ability to influence their target firms' tax avoidance activities. First, compared with other institutions, hedge fund activists have stronger incentive to engage in costly monitoring activities, since they are less susceptible to the free-rider problem and fund managers have stronger compensation incentives. Hedge funds are largely unregulated and are not subject to the "prudent man" rule, allowing them to accumulate a large stake in an individual company (Clifford 2008). As a result, hedge funds have incentive to undertake monitoring activities to improve the operational performance of target firms, so that their marginal returns from the improved firm governance and performance exceed their monitoring costs. In addition, hedge fund managers' pay depends largely on their funds' absolute returns (Kahan and Rock 2007). A hedge fund can invest a large proportion of its wealth in individual firms and has incentives to influence individual firms' operations, such as reducing tax payments, to generate improved returns.

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[4] In untabulated results, we find that the increase in tax avoidance following the intervention is positively associated with an increase in targets' valuation, as measured by Tobin's Q and abnormal returns.

[5] Brav et al. (2008) also argue that hedge fund activists are not short-term focused. Such funds have an average holding period of about 20 months, and their positive impact on the target firm does not fully dissipate even after the fund's exit.

[6] Most of these studies suggest that positive market returns are due to fundamental improvements in firm practices elicited by hedge fund intervention. To the best of our knowledge, Greenwood and Schor (2009) is the only paper that attributes the documented positive returns of target firms to the ability of hedge funds to involve target firms as objects of takeover activity.

Second, increases in tax avoidance can help hedge funds to achieve their goal of improving firm value. One important source of gains from intervention comes from the economic benefits of constraining inefficient managerial actions (Maug 1998; Gillan and Starks 2000). Tax avoidance increases firm value by generating tax savings that potentially improve accounting earnings and cash flows (Hanlon and Heitzman 2010), which should benefit hedge fund activists and increase the value of their investments. With statutory tax rates often in excess of one-third of a company's profits, tax avoidance provides a significant opportunity for hedge fund activists to increase free cash flow, firm value, and the value of their investments.[7]

Hedge fund activists also have the ability to push for more efficient tax management through their enhanced monitoring. They can set "the tone at the top," analogous to the effect of top managers on tax avoidance (Dyreng et al. 2010), by emphasizing the importance of tax planning to the target firm. In addition, prior literature finds that hedge funds achieve significant success in promoting corporate changes (Brav et al. 2008; Klein and Zur 2009). Hedge funds have relatively high percentages of ownership and can use leverage and derivative instruments to obtain beneficial ownerships or voting rights (Hu and Black 2006). They can exercise their shareholder rights to nominate and elect board members, sponsor shareholder proposals, and launch proxy fights (Bratton 2006; Briggs 2007; Klein and Zur 2009). They also frequently use public media to push for corporate changes, and cooperate with other institutional investors to make their interventions successful (Brav et al. 2008). Therefore, managers of target firms have incentives to meet hedge funds' demands for more efficient tax planning. In summary, hedge fund activists' strengthened monitoring of effort-averse firm managers could improve target firms' tax planning.

Anecdotal evidence supports this perspective. When targeting BNS Co. in 2002, hedge fund Hummingbird Management, LLC stated:[8]

> Our motivation is purely financial; we only seek to maximize returns from our investment. The important goals are to maximize returns on remaining assets while minimizing taxes and ongoing costs, thereby maximizing the ultimate cash payment, to the owners of the company. (*Hummingbird vs. BNS Co.* 2002)

This activist statement is consistent with hedge fund activists' intent to improve tax planning to increase firm value. In this filing, Hummingbird also proposes the following specific change in tax strategies, exhibiting its knowledge and interest in tax planning:

> We feel that efforts should be made to fully utilize the company's NOL (net operating loss). Using the NOL to shield income thrown off from the building and from the proceeds from asset sales would minimize tax expense to the company.

We provide additional anecdotal evidence that activist funds recognize the importance of target firm tax strategy in Appendix A. Based on the above discussions regarding hedge funds activists' incentive and ability to influence their target firms' tax avoidance activities, we propose the following hypothesis:

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[7] Assuming that tax planning generates economic benefits, one might ask why target firm managers do not always engage in such planning. There are frictions that could induce inefficiency in tax planning of our target firms. For example, prior literature suggests that target firm managers lack adequate incentive to maximize firm value (e.g., Brav et al. 2008; Klein and Zur 2009). Target firms may underinvest in tax planning even though it is value-enhancing. Hedge fund activism can help alleviate this friction. Brav et al. (2008) find hedge fund activism increases pay-for-performance sensitivities for corporate executives, providing stronger incentives for effort-averse managers to improve tax planning (Hanlon 2005).

[8] See: http://www.sec.gov/Archives/edgar/data/14637/0001164073-02-000022.txt

Hypothesis: Firms targeted in hedge fund activism exhibit higher levels of tax avoidance after the activist event.

III. DATA AND VARIABLE CONSTRUCTION

Sample Selection

Consistent with prior literature (e.g., Brav et al. 2008; Klein and Zur 2009), we construct our hedge fund sample from Schedule 13D filings. The 1934 Securities Exchange Act requires investors who acquire a 5 percent or greater stake in a publicly traded firm to file a Schedule 13D with the SEC within ten days and declare any intentions to influence the firm's management.[9] In addition, these investors should use Schedule 13D/A to report material changes in a held position.

We start our sample collection by obtaining all Schedule 13D and 13D/A filings between January 1, 1994, and December 31, 2008, from the EDGAR database of the SEC. We start from 1994 because company filings in the SEC EDGAR database were sporadic before 1994 (SEC 2006). In addition, the use of a post-1993 sample ensures the consistency in the accounting treatments for income taxes for our sample period due to the issuance of FAS 109, effective in 1993. From each filing, we collect the filing date, the name of the filer, and the name of the identified target. The filers are then matched with a comprehensive list of hedge funds to identify Schedule 13D and 13D/A filings by hedge funds.[10] Our sample includes 435 activist hedge funds and 2,981 activist events in the period 1994–2008.

Table 1, Panel A, shows the distribution of hedge fund activist events by year. There is an increasing trend in the number of hedge fund activist events over time. In Panel B, we present the frequency of participation by hedge funds. A majority (60.69 percent) of funds are involved in no more than three activist events in the sample period, while 14.94 percent engage in more than ten activist events in the period. This result suggests that while the majority of hedge funds do not engage in activism on a regular basis, some do so frequently. In Panel C, we show the industry distribution of the target firms based on two-digit SIC industry classification. Prior literature finds cross-industry variation in firms' effective tax rates, suggesting that industry differences may partially explain differences in levels of tax avoidance among firms. Therefore, we include industry dummies in each of our primary tests to control for industry effects. In the empirical analysis, we exclude target firms in financial and utility industries since they have unique accounting requirements and regulatory environments (Hanlon 2005).

Variable Construction

In addition to hedge fund activism data, we obtain the required financial statement data from Compustat, institutional ownership data from Thomson's 13F database, and daily stock return data from CRSP. Appendix B provides a detailed definition of each variable used in our empirical analysis. Hanlon and Heitzman (2010) call for caution with respect to the selection of tax avoidance measures. In particular, they note that different empirical metrics computed from the financial statements capture different aspects of a firm's tax strategy. Certain empirical measures do not consistently reflect the impact of tax strategies that permanently reduce tax liability (e.g., debt tax shields) versus other strategies that simply defer taxes to future periods (e.g., accelerated

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[9] When the investor has no intention to influence firm management, she should file Schedule 13G instead of 13D.

[10] We are grateful to Wei Jiang for sharing the list of 236 hedge fund activists used in Brav et al. (2008). We also thank Fei Pan for sharing a list of hedge funds collected from four hedge fund databases: Tremont Advisory Shareholder Services (TASS), HedgeFund.Net (HFN), Center for International Securities and Derivatives Markets (CISDM), and HedgeFund Intelligence.

TABLE 1

Descriptive Statistics for the Hedge Fund Activism Sample

Panel A: Number of Hedge Fund Activist Events by Year of First SC 13D Filing

Year	Frequency	Percent
1994	12	0.40
1995	40	1.34
1996	109	3.66
1997	224	7.51
1998	169	5.67
1999	151	5.07
2000	168	5.64
2001	147	4.93
2002	154	5.17
2003	194	6.51
2004	221	7.41
2005	346	11.61
2006	373	12.51
2007	384	12.88
2008	289	9.69
Total	2,981	100

Panel B: Participation Frequency of Hedge Fund Activists

Number of Events	Number of Activists	Percent
1	150	34.48
2	72	16.55
3	42	9.66
4	22	5.06
5	22	5.06
6	18	4.14
7	16	3.68
8	12	2.76
9	12	2.76
10	4	0.92
> 10	65	14.94
Total	435	100

(continued on next page)

depreciation). While we maintain our focus on tax avoidance, as opposed to more extreme tax aggressiveness, we make no formal hypotheses on the types or mechanisms of avoidance in which target firms engage. As such, we consider four broad constructs of tax avoidance that are well established in the literature and, in particular, are consistent with other studies examining tax avoidance in the agency cost context (Desai and Dharmapala 2006, 2009; Chen et al. 2010).

TABLE 1 (continued)

Panel C: Industry Distribution of Sample Firm-Years (per two-digit SIC industry classification)

SIC2	Industry Name	# of Obs.	Percent	SIC2	Industry Name	# of Obs.	Percent
1	Agricultural Production—Crops	8	0.27	45	Transportation by Air	13	0.44
7	Agricultural Services	3	0.10	47	Transportation Services	5	0.17
10	Metal, Mining	15	0.50	48	Communications	112	3.76
12	Coal Mining	13	0.44	49	Electric, Gas, and Sanitary Services	64	2.15
13	Oil and Gas Extraction	88	2.95	50	Wholesale Trade—Durable Goods	70	2.35
14	Nonmetallic Minerals, except Fuels	6	0.20	51	Wholesale Trade—Nondurable Goods	45	1.51
15	General Building Contractors	16	0.54	52	Building Materials and Gardening Supplies	2	0.07
16	Heavy Construction, Except Building	9	0.30	53	General Merchandise Stores	27	0.91
17	Special Trade Contractors	6	0.20	54	Food Stores	11	0.37
20	Food and Kindred Products	51	1.71	55	Automotive Dealers and Service Stations	10	0.34
21	Tobacco Products	4	0.13	56	Apparel and Accessory Stores	33	1.11
22	Textile Mill Products	14	0.47	57	Furniture and Home Furnishings Stores	10	0.34
23	Apparel and Other Textile Products	25	0.84	58	Eating and Drinking Places	42	1.41
24	Lumber and Wood Products	19	0.64	59	Miscellaneous Retail	87	2.92
25	Furniture and Fixtures	21	0.70	60	Depository Institutions	176	5.90
26	Paper and Allied Products	18	0.60	61	Non-depository Institutions	33	1.11
27	Printing and Publishing	46	1.54	62	Security and Commodity Brokers	30	1.01
28	Chemical and Allied Products	194	6.51	63	Insurance Carriers	67	2.25
29	Petroleum and Coal Products	10	0.34	64	Insurance Agents, Brokers, and Service	17	0.57
30	Rubber and Miscellaneous Plastics Products	38	1.27	65	Real Estate	36	1.21
31	Leather and Leather Products	13	0.44	67	Holding and Other Investment Offices	163	5.47
32	Stone, Clay, and Glass Products	17	0.57	70	Hotels and Other Lodging Places	20	0.67
33	Primary Metal Industries	38	1.27	72	Personal Services	13	0.44
34	Fabricated Metal Products	37	1.24	73	Business Services	369	12.38
35	Industrial Machinery and Equipment	126	4.23	75	Auto Repair, Services, and Parking	10	0.34
36	Electronic and Other Electric Equipment	185	6.21	76	Miscellaneous Repair Services	2	0.07
37	Transportation Equipment	43	1.44	78	Motion Pictures	19	0.64
38	Instruments and Related Products	186	6.24	79	Amusement and Recreation Services	36	1.21
39	Misc. Manuf. Industries	28	0.94	80	Health Services	58	1.95
40	Railroad Transportation	1	0.03	82	Educational Services	15	0.50
41	Local and Interurban Passenger Transit	2	0.07	83	Social Services	9	0.30
42	Trucking and Warehousing	13	0.44	87	Engineering and Management Services	62	2.08
44	Water Transportation	9	0.30	99	Non-classifiable Establishments	13	0.44

The sample consists of 2,981 hedge fund activist events initiated from 1994 to 2008. Panel A presents the frequency of activist events by years. Panel B shows the participation frequency of hedge fund activists. Panel C shows the industry distribution of sample firm-years (per two-digit SIC Industry Classification).

A firm's effective tax rate (ETR) is a popular metric for evaluating a firm's ability to minimize income taxes. We compute our first measure of tax avoidance as follows:[11]

$$Current\ ETR_{i,t} = (Total\ Tax\ Expense_{i,t} - Deferred\ Tax\ Expense_{i,t})/(Pretax\ Income_{i,t} - Special\ Items_{i,t}).$$

Current ETR captures tax strategies that give rise to both permanent and temporary book-tax differences through their exclusion of deferred tax expense. We regard *Current ETR* as a suitable measure of tax avoidance in the context of hedge fund activism, since funds could benefit from both tax reduction and tax deferral strategies. A lower *Current ETR* suggests that the firm is paying a smaller portion of its pretax book profits to taxing authorities, and is more effectively avoiding income taxes than firms with a higher *Current ETR*. Consistent with prior literature (e.g., Badertscher et al. 2010a; Chen et al. 2010), we restrict *Current ETR* to fall in the interval [0, 1].

Dyreng et al. (2008) propose an alternative version of the effective tax rate. They use the actual cash taxes paid (disclosed on the statement of cash flows) in the numerator, while retaining the traditional denominator. One advantage of this metric is that it takes into account tax benefits not recognized on the income statement, such as the tax benefits of employee stock options pre-SFAS 123R.[12] Furthermore, cash taxes paid are also free from possible accrual manipulation used to manage after-tax earnings. We use the cash effective tax rate (*Cash ETR*) as our second measure of tax avoidance, and similarly limit *Cash ETR* to fall within [0,1].[13] Specifically:

$$Cash\ ETR_{i,t} = Taxes\ Paid_{i,t}/(Pretax\ Income_{i,t} - Special\ Items_{i,t}).$$

The two remaining measures are based on book-tax differences. First, we calculate the Manzon and Plesko (2002) book-tax difference (*MP_BTD*) as follows:

$$MP\_BTD_{i,t} = \Big(Domestic\ Income_{i,t} - (Current\ Federal\ Income\ Tax\ Expense_{i,t}/0.35) - State\ Income\ Tax\ Expense_{i,t} - Other\ Income\ Tax\ Expense_{i,t} - Equity\ Income_{i,t}\Big)/(Total\ Assets_{i,t-1}).$$

We focus on domestic BTD first, because our sample of hedge fund activism targeted firms tend to be small and generally have insignificant amounts of foreign income, with average foreign income as a percentage of total income being only about 3.4 percent.[14] In addition, 70 percent of our observations have no foreign income in the event period of five years before and after the hedge fund activism. Second, focusing on domestic BTD avoids the problems associated with inferring applicable foreign tax rates (Manzon and Plesko 2002; Desai and Dharmapala 2009). Multiplying foreign tax expense by the U.S. federal statutory rate to estimate foreign tax liabilities in the calculation of total BTD is problematic, as studies document that U.S. firms (especially those with tax haven subsidiaries) tend to pay a much lower rate on foreign income (e.g., Dyreng and Lindsey 2009). Finally, this approach makes our analysis more directly comparable to prior literature (e.g., Desai and Dharmapala 2006, 2009; Chen et al. 2010). Large values of *MP_BTD* indicate greater levels of tax avoidance.

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[11] We exclude special items from the denominator for two reasons: (1) special items can be quite large and lead to volatile annual ETR measures (Dyreng et al. 2008), and (2) special items include restructuring charges and other nonrecurring charges that might result from operational changes following hedge fund activism. Untabulated analyses show that our results are robust to the inclusion of special items in the denominator, as well as to the use of the overall ETR.

[12] Before SFAS 123R, firms could deduct stock option expenses for tax purposes and record this benefit as paid-in capital.

[13] Dyreng et al. (2008) show that the one-year cash ETR measure is an unreliable measure of long-run tax avoidance with evidence of low correlation between one-year rates with rates calculated over five- and ten-year periods, but do not suggest the short-run measure is without merit. For our event study research setting, one-year measures appear to be more appropriate for the examination of year-to-year changes in tax avoidance. In addition, in the context of an event study, finding changes in a long-run cash effective tax rate would be difficult, because years prior to the event will still be more heavily weighted in the avoidance measures for the post-intervention years. Finally, the robust results across the three additional metrics should partially mitigate the concern over the validity of one-year measure of cash ETR. Alternatively, we estimate three-year cash ETRs before and after the hedge fund intervention, respectively, and find our inferences are unaltered.

[14] There are two reasons why hedge funds tend to target smaller firms with minimum foreign activities. First, these firms have fewer resources for value-enhancing activities such as tax avoidance and, thus, could benefit more from hedge fund interventions. Second, fund activists are better able to influence and implement business strategies in firms that are small and less complex than multinational firms.

MP_BTD captures tax strategies that lead to both permanent and temporary differences between book income and taxable income. Desai and Dharmapala (2006) note that federal tax expense, used to calculate taxable domestic income, is affected by income-changing discretionary accruals used for earnings management purposes. To mitigate the influence of earnings management strategies on book-tax differences, they compute an alternative book-tax difference metric by regressing *MP_BTD* on total accruals (*TA*) measured using the cash flow method suggested by Hribar and Collins (2002):[15]

$$MP\_BTD_{i,t} = \beta_1 TA_{i,t} + \mu_i + \varepsilon_{i,t}.$$

The residual of this regression (ε), which is expected to be largely free of earnings management (or at least accruals management), is the Desai and Dharmapala (2006) book-tax difference (*DD_BTD*). Similar to *MP_BTD*, larger values of *DD_BTD* imply greater levels of tax avoidance.[16]

There are limitations associated with empirical tax avoidance measures, including those used in our study. First, as noted by Bernard (1984) and Hanlon and Heitzman (2010), these measures do not capture conforming tax avoidance or those strategies reducing both book and taxable income, such as debt interest. Second, measures of book-tax differences are essentially an "ETR differential," reflecting the difference between the statutory tax rate and the GAAP ETR, since taxable income is estimated by "grossing up" reported tax expense by the statutory tax rate (Hanlon and Heitzman 2010, 142). These four measures are not independent and, therefore, neither are the corresponding tests based on these measures.

IV. PRIMARY RESULTS

Propensity Score Matching

Brav et al. (2008) show that hedge fund activists select their target firms based on certain firm characteristics. Our results will be biased if these *ex ante* characteristics of target firms lead to future increases in our tax avoidance measures even without the hedge fund intervention. Another concern is that a contemporaneous upward trend in tax avoidance occurring to all firms drives our results. To control for a potential selection bias and market-wide changes of tax avoidance, we use the propensity score matching method to construct a sample of control firms.[17] The propensity score is the predicted probability of becoming a hedge fund target in the next year, estimated from a logistic regression model, as in Brav et al. (2008, Table IV). Specifically, we regress a dummy variable of being targeted by hedge funds on the lagged values of firm size, Tobin's Q, sales growth, return on assets, debt-to-equity ratio, annual dividend yield, R&D expense, Herfindahl index, number of analysts following the firm, percentage of institutional ownership, and year dummies.[18] We estimate this logistic regression for all firms in the Compustat database with available data from 1994 to 2008, and then use the obtained coefficients to estimate the propensity score for each firm.[19] We identify non-target firms with the closest propensity score of the target firms, compute the differences between levels of target firms and those of matched control firms for all variables, and use the resulting adjusted values (i.e., the differences) for our empirical analysis.

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[15] μ_i is used to indicate the firm fixed-effect.

[16] Hanlon and Heitzman (2010) provide a comprehensive discussion of these and other tax avoidance measures.

[17] Propensity score matching is a widely used method to deal with selection bias, and measures the "treatment effect" as the outcome for the treated firm minus the outcome for an untreated firm with equal treatment probability (e.g., Li and Prabhala 2007; Lawrence et al. 2011). One advantage of using propensity score matching is to control for a general time-specific temporal trend. Since our sample does not have calendar time clustering, using simple industry matching (e.g., adjusting the variables for industry means) can control for such a trend. However, matching on industry may not sufficiently control for the self-selection bias. We find similar, but slightly weaker, results when using only industry control samples.

Univariate Test

Table 2 provides univariate tests of tax avoidance for target firms surrounding hedge fund activist events. For each measure, we present the mean adjusted annual levels from the year prior to the activist event to the second year after the event (Event Year 0 being the year of the intervention announcement), as well as the changes in these adjusted annual variables. Specifically, we adjust the values of tax avoidance measures by subtracting the corresponding value for the matched firm from that for the sample firm. For each tax avoidance measure, we require a constant sample with non-missing data across the event year window [−1, +2] and, thus, the sample size varies across different tax avoidance measures.

An important premise of our hypothesis is that the tax avoidance level of the target firm must be sufficiently low to motivate hedge funds' attempts to improve tax planning.[20] The "Adjusted Annual Values" section of Table 2 shows that in Year −1, the *Current ETR* (*Cash ETR*) has an average of 0.108 (0.143), suggesting that target firms have significantly higher effective tax rates than matched control firms prior to the intervention.[21] The two book-tax difference measures are significantly negative (−0.036 for *MP_BTD*, and −0.021 for *DD_BTD*), indicating lower book-tax differences relative to control firms. Such tax inefficiency (as indicated by significantly positive ETRs and negative BTDs) may have attracted hedge fund activists' interest to promote a change. To test whether hedge funds target firms with low tax avoidance levels, we estimate the association between tax avoidance in Year *t* and the probability of being targeted by hedge fund activists in Year *t*+1 by adding our tax measures to the logistic regression model used by Brav et al. (2008), as described previously. We find (untabulated) that our two ETR measures are significantly and positively associated with the likelihood of being targeted.[22] Taken together, our results are consistent with inefficient tax planning being a potential motivating factor for hedge funds to intervene.

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[18] In an alternative specification, we also include the prior tax avoidance level in the estimation of propensity scores to address the concern that target firms and control firms could be significantly different in tax avoidance levels before the intervention. We then repeat the analyses in Table 2 and Table 5, and find results that are largely consistent with those reported in the paper.

[19] We find largely similar results as in Brav et al. (2008). For conciseness, the results from the targeting likelihood regressions are not tabulated, but are available upon request.

[20] We thank the original editor, Thomas Omer, for raising this point.

[21] As previously discussed, we adjust values of all variables by those of matched control firms and use the resulting adjusted values for our empirical analysis. Thus, all values of variables reported in this paper are adjusted values, and statistical significance levels of these values indicate whether there are significant differences between target firms and matched control firms.

[22] The coefficients on the two BTD measures are negative and statistically insignificant.

TABLE 2

Changes in Tax Avoidance after Hedge Fund Intervention

	Current ETR	*Cash ETR*	*MP_BTD*	*DD_BTD*
Adjusted Annual Values				
Year −1	0.108***	0.143***	−0.036**	−0.021**
Year 0	0.086***	0.123***	0.011	−0.006
Year +1	0.069***	0.105***	0.042**	0.003
Year +2	0.043***	0.088***	0.042**	−0.003
Changes in Adjusted Annual Values				
(Year +1) − (Year −1)	−0.039**	−0.038**	0.079***	0.025**
(Year +2) − (Year −1)	−0.065***	−0.054**	0.078***	0.019*
No. of Observations	554	650	815	767

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (one-sided), respectively.
This table reports the mean adjusted annual levels of tax avoidance for target firms around hedge fund activist events from Event Year −1 to Event Year +2, with Event Year 0 being the year of the intervention announcement, as well as the changes in these adjusted annual variables. Adjusted values are computed as the differences between the levels of event firms and the levels of the matched control firms. Control firms are identified as firms that have never been targeted by hedge funds during our sample period, but have the closest propensity scores to target firms. Propensity score is the predicted probability of becoming a hedge fund activism target in the next year, estimated from the model as in Brav et al. (2008, Table IV). For each tax avoidance measure, we require a constant sample with non-missing data across the event window [−1, +2]. In the top section, we test whether the tax avoidance levels of target firms are statistically different from those of control firms in each event year. In the bottom section, we test whether the changes in tax avoidance levels of target firms differ significantly from those of control firms. Appendix B contains the variable definitions.

Next, we turn to changes in adjusted tax avoidance rates after hedge fund activism events. The "Changes in Adjusted Annual Values" section of Table 2 shows that target firms exhibit greater improvement in tax avoidance than matched control firms after the activist event. Specifically, for both *Current ETR* and *Cash ETR*, the differences between the year prior to the intervention and the year after (−0.039 and −0.038, respectively) are negative and significant. We find that differences between the year prior to the intervention and Year *t*+2 are even larger for both *Current ETR* and *Cash ETR* (−0.065 and −0.054, respectively), suggesting further increases in tax avoidance during Year *t*+2. The univariate results for the book-tax difference measures generate similar inferences. The measures show similar increases in book-tax differences in both years following the intervention for both *MP_BTD* (0.079 and 0.078, respectively) and *DD_BTD* (0.025 and 0.019, respectively). In summary, univariate tests across all four tax avoidance measures support our hypothesis, indicating increases in tax avoidance following hedge fund intervention.

Direct Effects versus Indirect Effects

Since hedge fund interventions often elicit profound and multi-dimensional changes in target firms (Brav et al. 2010), our results in Table 2 could be driven by either the direct effects of activism on tax avoidance as a result of improvements in tax planning, or by indirect effects arising from changes in the firm's operating performance, capital structure, and investments. For example, Brav et al. (2008) show that target firms experience significant increases in leverage, and the tax avoidance literature (e.g., Badertscher et al. 2010a, 2010b) finds that the level of leverage affects a firm's incentives for tax planning. Firms with more debt enjoy a larger interest tax shield that reduces income tax liabilities and may lower the need for tax planning or other tax shields (Badertscher et al. 2010a, 2010b). Acknowledging this possibility, we conduct additional tests to explore to what extent the indirect effects of activism influence our findings of increases in tax avoidance. First, we examine whether changes in performance, leverage, and investments cause the changes in tax avoidance after the fund intervention. Second, we use a multivariate levels regression model to control for known tax determinants, so that our variables of interest can better capture the direct effects of activism on tax. Finally, we investigate the impact of hedge fund activist characteristics, especially those indicating their interest or knowledge in tax efficiency.

Changes in Performance, Leverage, and Investments after Hedge Fund Intervention

We first examine changes in the target firms' operating performance (measured by ROE), capital structure (measured by leverage), and investments (measured by PPE and intangible assets) to assess the possibility that changes in these variables lead to the increases in tax avoidance. Table 3 reports the mean adjusted annual levels of ROE, leverage, PPE, and intangible assets from Event Year −1 to Event Year +2 for target firms, as well as the changes in these adjusted annual variables. Specifically, we adjust all variables by subtracting the corresponding value for the matched firm from that for the sample firm.

In the "Changes in Adjusted Annual Values" section of Table 3, we find no significant changes in pretax ROE, implying that our results on tax avoidance should not be driven by changes in operating performance.[23] However, we do find significant increases in leverage, similar to the findings in Brav et al. (2010). Leverage has no direct effect on our tax measures since interest expense is deductible for both book income and taxable income, and our measures capture only nonconforming tax avoidance (Bernard 1984; Hanlon and Heitzman 2010). However, leverage may be correlated with firm tax strategy due to the impact of a debt tax shield on a firm's incentives for tax planning (e.g., Badertscher et al. 2010a, 2010b). We find that target firms experience decreases in property, plant, and equipment (PPE), which is consistent with the findings in Brav et al. (2010), suggesting that activism cuts slack for target firms by tightening monitoring in investment decisions. PPE is subject to nonconforming book and tax accounting rules and can be used to increase tax avoidance (e.g., using accelerated depreciation of PPE under tax accounting rules). Decreases in PPE allow for less reduction in tax liabilities, due to accelerated tax depreciation, and also lead to lower levels of tax avoidance. Hence, decreases in PPE following the intervention cannot explain our documented increases in tax avoidance. Finally, we find no significant increases in intangible assets.

To rule out changes in firm fundamentals as alternative explanations for our results, we construct several control samples based on changes in four variables (ROE, leverage, PPE, and intangible assets) and compare changes in tax avoidance between our target sample and control samples. Specifically, we identify four separate control samples matched on changes in ROE, leverage, PPE, and intangible assets, respectively.[24] We then compare changes in tax avoidance around hedge fund intervention between the target sample and each of the control samples. Consistent with our prediction, the untabulated results indicate that target firms experience significantly greater increases in tax avoidance than their control firms in each of these matching samples.

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[23] In a univariate test, Brav et al. (2008) find that target firms' performance deteriorates during the event year and eventually recovers and exceeds pre-intervention performance in *Year +2*. We find a similar trend that ROE improves in *Year +2*, but this change is not statistically significant. The difference between our findings and theirs is likely due to sample differences. For robustness, we analyze their sample (which is a subset of our sample) and find similar results.

[24] We match target firms and control firms on only one dimension at a time (e.g., change in PPE) because very few control firms experience changes in more than one dimension similarly to the target firms.

TABLE 3

Changes in Performance, Leverage, and Investments after Hedge Fund Intervention

	ROE (pretax)	*Leverage*	*PPE*	*Intangible Asset*
Adjusted Annual Values				
Year −1	−0.109***	0.043**	−0.019***	0.028***
Year 0	−0.139***	0.087***	−0.031***	0.026***
Year +1	−0.184***	0.087***	−0.038***	0.026***
Year +2	−0.061	0.072***	−0.035***	0.023***
Changes in Adjusted Annual Values				
(Year +1) − (Year −1)	−0.075	0.044**	−0.019**	−0.003
(Year +2) − (Year −1)	0.049	0.029**	−0.016*	−0.005
No. of Observations	1,069	1,030	1,074	1,075

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (one-sided), respectively.
This table reports the mean adjusted annual levels of performance, leverage, and investments for target firms around hedge fund activist events from Event Year −1 to Event Year +2, with Event Year 0 being the year of the intervention announcement, as well as the changes in these adjusted annual values. Adjusted values are computed as the differences between the levels of event firms and the levels of the matched control firms. Control firms are identified as firms that have never been targeted by hedge funds during our sample period, but have the closest propensity scores to target firms. Propensity score is the predicted probability of becoming a hedge fund activism target in the next year, estimated from the model as in Brav et al. (2008, Table IV). For each firm characteristic measure, we require a constant sample with non-missing data across the event window [−1, +2]. In the top section, we test whether the performance, leverage, and investment levels of target firms are statistically different from those of control firms in each event year. In the bottom section, we test whether the changes in performance, leverage, and investment levels of target firms differ significantly from those of control firms. Appendix B contains the variable definitions.

Alternatively, we can control for indirect effects using a multivariate framework by explicitly including these variables in the regression model. We report our multivariate results in the next two subsections.

Multivariate Test

In testing family owners' influence on tax avoidance, Chen et al. (2010) suggest the inclusion of a comprehensive set of variables in a levels regression model to control for indirect effects. Chen et al. (2010, 44, 49–50) explain that these control variables are intended to capture the indirect effects on tax avoidance measures arising from firm characteristics, and those caused by specific tax treatments that differ from Generally Accepted Accounting Principles (GAAP).[25] We follow their approach by including a comprehensive set of variables, so that the variables of interest (i.e., event year dummies) in the multivariate regressions are likely to capture the direct effects of hedge fund activism on tax avoidance.

One concern is that our regression equation includes several tax determinant variables that can be a part of the tax avoidance strategies promoted by hedge fund activists, and controlling for these variables could purge out some "direct effects" of hedge fund activism on corporate tax avoidance, biasing against finding significant coefficients on our key variables of interest. However, as Chen et al. (2010, 49–50) note, controlling for tax tool variables is not "throwing the baby out with the bath water," in the sense that these variables simply control for the average tax avoidance associated with those variables. Our model only removes the average effects of these tax tools so that the year dummy variables still potentially capture the changes in firms' tax avoidance resulting from the direct effects.

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[25] Other studies use the same approach of controlling for firm characteristics in multivariate regressions to ensure that other firm characteristics do not cause the differences in the outcome variable being investigated, including Ashbaugh-Skaife et al. (2009) and Graham et al. (2008).

We estimate Equation (1) for each of the tax avoidance measures. The sample includes firm-years of target firms from Event Year −1 to Event Year +2 with required data:

$$\begin{aligned} Tax\ Measure_{i,t} = {} & \beta_0 + \beta_1 DYear0_{i,t} + \beta_2 DYear1_{i,t} + \beta_3 DYear2_{i,t} + \beta_4 ROE_{i,t} + \beta_5 Leverage_{i,t} \\ & + \beta_6 DNOL_{i,t} + \beta_7 \Delta NOL_{i,t} + \beta_8 Foreign\ Income_{i,t} + \beta_9 PPE_{i,t} \\ & + \beta_{10} Intangible\ Asset_{i,t} + \beta_{11} Equity\ Income_{i,t} + \beta_{12} MTB_{i,t-1} + \beta_{13} Size_{i,t-1} \\ & + Year\ Dummies + Industry\ Dummies + \varepsilon_{i,t}. \qquad (1) \end{aligned}$$

Tax Measure is the firm's *Current ETR*, *Cash ETR*, *MP_BTD*, or *DD_BTD* in period *t*. *DYear0*, *DYear1*, and *DYear2* are dummy variables that equal 1 if the current year is the Event Year 0, +1, and +2, respectively, and 0 otherwise. For the ETR (BTD) measures, a negative (positive) coefficient on *DYear1* and *DYear2* variables indicates higher levels of tax avoidance in Event Year +1 and Event Year +2 relative to Event Year −1. All variables in the regressions incorporate adjustments for the corresponding values of the propensity score matched control firms.

We include a number of control variables that have been shown by prior literature to be potential determinants of tax avoidance (e.g., Manzon and Plesko 2002; Mills 1998; Rego 2003; Dyreng et al. 2008; Frank et al. 2009; Chen et al. 2010; McGuire et al. 2011, 2012). Return on equity (*ROE*) is calculated as operating income (computed as pretax income minus extraordinary items) scaled by the lagged book value of equity, and measures a firm's financial performance. Gupta and Newberry (1997) report a positive relation between ETRs and firm profitability. They suggest that higher income is associated with higher marginal tax rates and, hence, firms with higher profits will tend to exhibit higher ETRs. However, Rego (2003), Frank et al. (2009), Rego and Wilson (2010), and McGuire et al. (2012) document a positive relation between firm profitability and tax avoidance. They argue that more-profitable firms have stronger incentives due to the larger potential savings, and more resources to engage in tax avoidance. In addition, Manzon and Plesko (2002) and Rego (2003) propose that more-profitable firms have lower costs of tax avoidance, since they can make more efficient use of tax deductions, credits, and exemptions relative to less-profitable firms, resulting in lower effective tax rates and greater book-tax differences.

Since interest expense is deductible for taxable income while dividend payments are not, a firm's capital structure is an important determinant of its expected tax liability. We include a firm's *Leverage* (long-term debt scaled by lagged total assets) to control for the effect of debt on firms' incentives in tax planning. As discussed earlier, since interest expenses are deductible for both book and tax incomes, nonconforming tax avoidance measures, such as effective tax rates and book-tax differences, will not capture tax avoidance resulting from changes in leverage (Bernard 1984; Hanlon and Heitzman 2010, 142). However, the tax deductibility of the interest will have an impact on the firm's tax-planning incentives. For instance, highly leveraged firms may have either a stronger motivation to avoid taxes in order to preserve cash to service the heavy debt burdens, or a weaker motivation due to the debt tax shield (Badertscher et al. 2010b).

Utilization of prior operating loss carryforwards (NOL) should reduce current period tax burdens. To control for this effect, we follow Chen et al. (2010) and include ΔNOL, the change in a firm's tax loss carryforwards from prior to current period, scaled by lagged total assets. The association between ΔNOL and our tax avoidance measures depends on whether it results in nonconformity between taxable income and book income. *Ceteris paribus*, a decrease in NOL implies that firms are using NOLs to lower their taxable income. When taxable income is lower, book-tax differences (BTD) will be larger and effective tax rates (ETR) will be lower. On the other hand, an increase in NOL due to current period's loss should not affect current taxable income, since its tax reduction benefits will be realized in future profitable years.[26] Taken together, the average relation between ΔNOL and tax avoidance can be significantly negative. Prior studies generally predict and find a negative relation between ΔNOL and BTD and a positive relation between ΔNOL and ETR. For example, Cooper and Knittel (2010) argue that those firms utilizing an NOL should have lower effective tax rates than their counterparts not using an NOL. Mackie (1999) suggests that the rising profitability during the 1990s allowed firms to effectively utilize NOLs and enjoy lower average tax rates during that time period. Manzon and Plesko (2002), Chen et al. (2010), and McGuire et al. (2011) all find that ΔNOL is positively (negatively) associated with ETR (BTD). To control for the existence of such exercisable benefits, we follow Chen et al. (2010) by including *DNOL*, an indicator variable equal to 1 if the firm had a positive tax loss carryforward at the beginning of the year, and 0 otherwise.

The variables *Foreign Income*, *PPE*, *Intangible Assets*, and *Equity Income* capture firm characteristics that by statute affect a firm's income tax liability. We include *Foreign Income*, the firm's foreign income scaled by lagged total assets, to control for book income that may not result in a current tax liability. This is because foreign profits are, in general, not subject to U.S. tax until repatriated (e.g., Rego 2003).[27] A firm's book income generally differs from its tax income because of different treatments of certain transactions. For instance, levels of depreciation expense calculated under book and tax rules are rarely equivalent and, therefore, firms with higher levels of depreciable assets may exhibit greater book-tax differences simply attributable to heterogeneous statutory requirements. Accordingly, we include the firm's property, plant, and equipment scaled by lagged assets, *PPE*, as an explanatory variable. Similarly, amortization rules for intangible assets vary between book and tax accounting rules. In particular, purchased goodwill is tested for impairment under GAAP, but amortized over 15 years under U.S. tax rules. Similar to prior literature (e.g., Chen et al. 2010), we note the importance of including *Intangible Asset* (intangible assets scaled by lagged total assets) because of the statutory differences in cost recovery and asset valuation. Income from affiliated entities is included in book income when recognized under the equity method, but is not necessarily included in taxable income. Thus, we control for this book-tax difference by including in our regression the amount of *Equity Income* (equity income in earnings scaled by lagged assets).

We also control for *Size* (natural logarithm of the firm's market value of equity) and *MTB* (market value of equity scaled by book value of equity). Political cost theory proposes that larger firms pay higher political costs, including taxes (Watts and Zimmerman 1986). Alternatively, larger firms have greater resources to influence the political process in their favor and to engage in tax planning (Siegfried 1972). Growth firms may be more likely to purchase tax-favored assets (e.g., Chen et al. 2010) and have more unrecovered capital for tax purposes, creating tax benefits through depreciation and amortization. On the other hand, mature firms may have more experience in tax planning. Finally, we include industry and year fixed-effects to capture variations attributable to these sources.

After removing observations with missing values for the control variables or tax avoidance metrics, our regression analyses are performed on the remaining firm-years for each of the four tax avoidance measures, with corresponding descriptive statistics reported in Panel A of Table 4. The mean (median) values for *Current ETR*, *Cash ETR*, *MP_BTD*, and *DD_BTD* are 0.102 (0.014), 0.138 (0.061), –0.023 (0.000), and –0.009 (0.008), respectively. The Pearson correlations in Panel

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[26] In addition, such deferred tax benefits are not included in our ETR tax measures, because current tax expense and cash tax taxes paid do not contain deferred tax benefits.

[27] Firms should recognize deferred tax expense on foreign earnings which are not considered permanently reinvested, but minimal guidance in GAAP regarding the definition of "permanently reinvested" results in wide variation in the application of this rule in practice.

B of Table 4 show that, as expected, the two ETR measures are significantly and negatively correlated with the two BTD measures. Across all four tax avoidance measures, tax avoidance levels are positively correlated with *ROE*, *Foreign Income*, *Equity Income*, and *Lagged Size*, and negatively correlated with ΔNOL.

Table 5 presents the results of the four regressions. The coefficients on the dummy variables for the year of the intervention (*DYear0*) are negative and significant for *Cash ETR* (–0.034), and positive and significant for *MP_BTD* (0.050). Further, the coefficients for the first year following the event (*DYear1*) are negative and significant for *Current ETR* (–0.039) and *Cash ETR* (–0.067), and positive and significant for *MP_BTD* (0.081) and *DD_BTD* (0.028), suggesting an increase in tax avoidance in the post-intervention period.[28] These increases in tax avoidance persist into the second event year, with unchanged signs and significant coefficients for *DYear2* for three measures (–0.079 for *Current ETR*, –0.111 for *Cash ETR*, 0.102 for *MP_BTD*). The coefficient for *DD_BTD* (0.024) has an unchanged sign, but is insignificant. We also find the changes in tax avoidance provide substantial economic benefits to target firms. The regression coefficients on our event year dummies reflect the average changes in our tax avoidance measures. For example, we find that target firms experience decreases in effective tax rates that range from 3.9 percent to 11.1 percent, which equates to dollar tax savings between $319,800 and $910,200 for an average firm, given that the mean pretax income of our sample is $8.2 million.

With regard to the coefficients on control variables, *ROE* is negatively associated with both *Current ETR* and *Cash ETR*, similar to the findings in Rego (2003), Rego and Wilson (2010), and McGuire et al. (2012). This finding is consistent with the argument that more-profitable firms have stronger incentives and more opportunities to engage in tax avoidance (Rego 2003; Manzon and Plesko 2002). Change in tax loss carryforwards (ΔNOL) is positively (negatively) associated with ETR (BTD), similar to the findings in Manzon and Plesko (2002), Chen et al. (2010), and McGuire et al. (2011). Our findings are consistent with the argument that the utilization of NOL carryforwards reduces current period tax liabilities. As expected, and consistent with prior studies (Rego 2003; Gupta and Newberry 1997; Chen et al. 2010; Rego and Wilson 2010; McGuire et al. 2012), *Foreign Income*, *PPE*, *Intangible Asset*, and *Equity Income* are negatively associated with the two ETR measures. Finally, we find larger firms exhibit greater tax avoidance, supporting Siegfried's (1972) assertion that larger firms have greater resources to engage in tax planning. To further examine the association between control variables and tax avoidance measures, we also conduct multivariate tests for each tax avoidance measure with only control variables. The results are similar to those with the inclusion of the event year indicators. In summary, the results from the multivariate tests in Table 5 show that increases in tax avoidance following hedge fund interventions are not driven by the indirect effects arising from changes in firm characteristics following the interventions.

Impact of Hedge Fund Activists' Characteristics

To further strengthen the validity of our results, we extend our analysis by investigating whether changes in tax avoidance of target firms following fund intervention are related to activists' heterogeneity in incentives and abilities. To identify hedge funds' incentive, we search for indications of their interest and knowledge in tax planning from their filings. We download all the

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[28] The changes in the first year (Year +1) appear dramatic. Note, also, that the changes do not reverse in Year +2, implying that hedge funds identify target firms whose tax under-avoidance can be improved in the short term (within two years). Similarly, Brav et al. (2008) also report dramatic changes in target firms' policies (e.g., compensation and dividend payout) in Year +1, indicating that activists could initiate and implement policy changes rather quickly. The large changes could potentially be due to measurement issues. However, we use four measures and find robust significant changes for each in Year +1. Hence, we believe the changes in Year +1 are not solely due to our empirical measures.

TABLE 4
Descriptive Statistics and Correlations

Panel A: Descriptive Statistics

Variables (Adjusted Values)	**n**	**Mean**	**Std.**	**5%**	**25%**	**Median**	**75%**	**95%**
Tax Avoidance Measures								
Current ETR	3,452	0.102	0.442	−0.565	−0.211	0.014	0.486	0.830
Cash ETR	3,555	0.138	0.473	−0.584	−0.202	0.061	0.565	0.882
MP_BTD	4,272	−0.023	0.794	−0.668	−0.094	0.000	0.104	0.573
DD_BTD	4,168	−0.009	0.294	−0.446	−0.108	0.008	0.120	0.391
Control Variables								
ROE	4,909	−0.136	1.631	−2.393	−0.426	−0.075	0.263	1.777
Leverage	4,909	0.039	0.377	−0.454	−0.161	−0.004	0.196	0.712
DNOL	4,909	0.069	0.608	−1.000	−0.500	0.000	0.500	1.000
ΔNOL	4,909	0.008	0.496	−0.646	−0.055	0.000	0.043	0.616
Foreign Income	4,909	−0.005	0.027	−0.049	−0.015	0.000	0.000	0.036
PPE	4,909	−0.012	0.347	−0.522	−0.200	−0.036	0.134	0.584
Intangible Asset	4,909	0.019	0.278	−0.376	−0.120	−0.009	0.124	0.555
Equity Income	4,909	0.000	0.005	−0.008	0.000	0.000	0.000	0.007
MTB	4,909	−0.545	6.661	−10.455	−2.427	−0.614	1.078	8.650
Size	4,909	−0.494	2.313	−4.229	−1.904	−0.545	0.773	3.524

(continued on next page)

TABLE 4 (continued)

Panel B: Correlations

Variables (Adjusted Values)		A	B	C	D	E	F	G	H	I	J	K	L	M	N
Current ETR	A	1.000													
Cash ETR	B	0.779 (0.000)	1.000												
MP_BTD	C	−0.118 (0.000)	−0.126 (0.000)	1.000											
DD_BTD	D	−0.168 (0.000)	−0.168 (0.000)	0.476 (0.000)	1.000										
ROE	E	−0.119 (0.000)	−0.172 (0.000)	−0.013 (0.409)	0.066 (0.000)	1.000									
Leverage	F	−0.056 (0.001)	−0.053 (0.002)	−0.114 (0.000)	0.084 (0.000)	0.021 (0.140)	1.000								
DNOL	G	0.023 (0.176)	0.010 (0.552)	−0.003 (0.845)	0.001 (0.962)	−0.042 (0.003)	0.051 (0.000)	1.000							
ΔNOL	H	0.058 (0.001)	0.060 (0.000)	−0.252 (0.000)	−0.154 (0.000)	−0.048 (0.001)	0.053 (0.000)	0.067 (0.000)	1.000						
Foreign Income	I	−0.069 (0.000)	−0.094 (0.000)	0.063 (0.000)	0.015 (0.342)	0.068 (0.000)	0.001 (0.968)	−0.019 (0.191)	−0.049 (0.001)	1.000					
PPE	J	−0.086 (0.000)	−0.107 (0.000)	−0.069 (0.000)	0.139 (0.000)	0.036 (0.011)	0.452 (0.000)	−0.048 (0.001)	0.023 (0.103)	0.006 (0.663)	1.000				
Intangible Asset	K	−0.040 (0.020)	−0.048 (0.004)	−0.040 (0.008)	0.053 (0.001)	0.011 (0.460)	0.318 (0.000)	0.034 (0.019)	0.052 (0.000)	0.043 (0.003)	−0.070 (0.000)	1.000			
Equity Income	L	−0.082 (0.000)	−0.090 (0.000)	0.040 (0.008)	0.000 (0.993)	0.050 (0.000)	0.019 (0.194)	−0.046 (0.001)	−0.040 (0.006)	0.048 (0.001)	0.029 (0.040)	−0.000 (0.984)	1.000		
MTB	M	−0.055 (0.001)	−0.026 (0.117)	0.038 (0.013)	−0.058 (0.000)	−0.315 (0.000)	−0.064 (0.000)	−0.044 (0.002)	−0.032 (0.025)	0.024 (0.096)	−0.027 (0.063)	0.004 (0.772)	−0.024 (0.099)	1.000	
Size	N	−0.112 (0.000)	−0.135 (0.000)	0.253 (0.000)	0.199 (0.000)	0.059 (0.000)	0.100 (0.000)	−0.097 (0.000)	−0.108 (0.000)	0.239 (0.000)	0.070 (0.000)	0.162 (0.000)	0.035 (0.014)	0.163 (0.000)	1.000

This table presents descriptive statistics and Pearson correlations for the sample that includes firm-years of target firms in the event window [−1, +2] with required data, where Event Year 0 is the year of the intervention announcement. Panel A reports descriptive statistics of variables used in the multivariate regressions. We compute and report adjusted values that are computed as the differences between the levels of the event firms and the levels of the matched control firms. Control firms are identified as firms that have never been targeted by hedge funds during our sample period, but have the closest propensity scores to target firms. Propensity score is the predicted probability of becoming a hedge fund activism target in the next year, estimated from the model as in Brav et al. (2008, Table IV). In Panel B, p-values are reported in parentheses. Appendix B has the variable definitions.

TABLE 5

Multivariate Analysis of Changes in Tax Avoidance after Hedge Fund Intervention

	Dependent Variables (Adjusted Values)							
	Current ETR		*Cash ETR*		*MP_BTD*		*DD_BTD*	
DYear0		−0.003		−0.034*		0.050*		0.013
		(−0.18)		(−1.75)		(1.67)		(1.07)
DYear1		−0.039*		−0.067***		0.081**		0.028**
		(−1.85)		(−2.93)		(2.37)		(2.03)
DYear2		−0.079***		−0.111***		0.102***		0.024
		(−3.26)		(−4.27)		(2.65)		(1.54)
Control Variables (Adjusted Values)								
ROE	−0.047***	−0.047***	−0.062***	−0.062***	−0.015	−0.014	0.006	0.006
	(−3.75)	(−3.76)	(−5.65)	(−5.67)	(−0.66)	(−0.65)	(0.77)	(0.79)
Leverage	0.023	0.025	0.062*	0.068*	−0.201***	−0.205***	−0.008	−0.009
	(0.68)	(0.75)	(1.73)	(1.92)	(−2.90)	(−2.94)	(−0.36)	(−0.42)
DNOL	0.020	0.019	0.008	0.009	0.046	0.046	0.018	0.018
	(1.03)	(1.01)	(0.39)	(0.44)	(1.63)	(1.62)	(1.53)	(1.52)
ΔNOL	0.125***	0.125***	0.126***	0.127***	−0.324***	−0.324***	−0.088***	−0.088***
	(3.67)	(3.68)	(3.55)	(3.57)	(−5.54)	(−5.56)	(−4.58)	(−4.58)
Foreign Income	−0.613*	−0.591*	−1.040***	−1.005***	−0.318	−0.345	−0.429*	−0.435*
	(−1.81)	(−1.74)	(−2.85)	(−2.76)	(−0.61)	(−0.66)	(−1.79)	(−1.81)
PPE	−0.195***	−0.197***	−0.203***	−0.211***	−0.226**	−0.222**	0.065**	0.066**
	(−4.77)	(−4.83)	(−4.95)	(−5.16)	(−2.41)	(−2.37)	(2.03)	(2.07)
Intangible Asset	−0.047	−0.045	−0.078*	−0.079*	−0.151	−0.153	0.065**	0.065**
	(−1.26)	(−1.23)	(−1.87)	(−1.90)	(−1.38)	(−1.40)	(2.18)	(2.16)
Equity Income	−5.224**	−5.323**	−7.367***	−7.212***	5.563**	5.407**	−0.493	−0.548
	(−2.48)	(−2.55)	(−3.45)	(−3.38)	(2.24)	(2.18)	(−0.47)	(−0.52)
MTB	−0.001	−0.001	−0.002	−0.002	−0.001	−0.001	−0.003**	−0.003**
	(−0.48)	(−0.57)	(−1.02)	(−1.04)	(−0.36)	(−0.39)	(−2.34)	(−2.35)
Size	−0.023***	−0.023***	−0.030***	−0.030***	0.091***	0.091***	0.027***	0.027***
	(−4.41)	(−4.44)	(−5.49)	(−5.53)	(4.89)	(4.90)	(6.99)	(7.00)

(continued on next page)

TABLE 5 (continued)

	Dependent Variables (Adjusted Values)							
	Current ETR		*Cash ETR*		*MP_BTD*		*DD_BTD*	
No. of Observations	3,452	3,452	3,555	3,555	4,272	4,272	4,168	4,168
Adj. R^2	0.110	0.113	0.132	0.137	0.168	0.169	0.128	0.128

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

This table presents the results on changes in tax avoidance of target firms around hedge fund activist events. The sample includes firm-years of target firms in the event window [−1, +2] with required data, where Event Year 0 is the year of the intervention announcement. For all dependent and control variables in the regressions, we adjust their values by the levels of the matched control firms. Control firms are identified as firms that have never been targeted by hedge funds during our sample period, but have the closest propensity scores to target firms. Propensity score is the predicted probability of becoming a hedge fund activism target in the next year, estimated from the model as in Brav et al. (2008, Table IV). Intercept, industry, and year dummies are included, but not reported. t-statistics reported in parentheses are based on heteroscedasticity robust standard errors clustered by firm. Appendix B contains the variable definitions.

SC 13D and 13D/A filings for our 2,981 hedge fund activist events (17,152 filings). Using a full-text search program, we identify 98 events (involving 59 hedge funds) in which hedge fund activists exhibit their interest or knowledge in target firms' tax issues. Due to the opaque nature of tax planning and the potential risk associated with its tax strategy being ruled as improper (Desai and Dharmapala 2006, 2009; Chen et al. 2010), it is likely that many hedge fund activists refrain from explicitly expressing their interest in tax issues or disclosing detailed tax-planning strategies in public filings.[29] To overcome the small-sample problem, instead of only focusing on these 98 special cases, we assume that the hedge fund activists associated with these events have interest and knowledge in tax issues and are more likely to exert influence over tax planning of their target firms.[30]

We also employ two other proxies for hedge funds' ability to implement tax changes. Specifically, we conjecture that hedge funds with more activism experience and greater past success of inducing tax changes have more ability to promote changes in their target firms. The regression model for these tests is specified below:

$$\begin{aligned} Tax\,Measure_{i,t} = {} & \beta_0 + \beta_1 DYear0_{i,t} + \beta_2 DYear1_{i,t} + \beta_3 DYear2_{i,t} + \beta_4 ActivistChar_{i,t} \\ & + \beta_5 DYear0_{i,t} \times ActivistChar_{i,t} + \beta_6 DYear1_{i,t} \times ActivistChar_{i,t} \\ & + \beta_7 DYear2_{i,t} \times ActivistChar_{i,t} + \beta_8 ROE_{i,t} + \beta_9 Leverage_{i,t} + \beta_{10} DNOL_{i,t} \\ & + \beta_{11} \Delta NOL_{i,t} + \beta_{12} Foreign\,Income_{i,t} + \beta_{13} PPE_{i,t} + \beta_{14} IntangibleAssets_{i,t} \\ & + \beta_{15} EquityIncome_{i,t} + \beta_{16} MTB_{i,t-1} + \beta_{17} Size_{i,t-1} + Year\,Dummies \\ & + Industry\,Dummies + \varepsilon_{i,t}, \end{aligned} \tag{2}$$

where *ActivistChar* represents *Activists' Experience*, *Tax Avoidance Changes in Past Activist Events*, and *Activist Exhibiting Tax Interest or Knowledge*, respectively, in separate tests. Specifically, *Activists' Experience* is proxied by the number of prior intervention events the fund activist has initiated in the past five years, *Tax Avoidance Changes in Past Activist Events* is proxied by the average changes in tax avoidance in the firms targeted by the activist during the past five years, and *Activist Exhibiting Tax Interest or Knowledge* is an indicator variable if the hedge fund activist is one of these 59 activists that have exhibited tax interest or knowledge in one of their 13D or 13D/A filings during the sample period. To test whether the increase in tax avoidance following interventions is affected by these activist characteristics, we interact *DYear0*, *DYear1*, and *DYear2*, respectively, with *ActivistChar*. We also adjust all dependent and control variables for the levels of the propensity score matched control firms.

Table 6 reports the results of estimating Equation (2). In Panel A, we find no significant coefficients on these interactions between *DYear0*, *DYear1*, and *DYear2* and *Activists' Experience*. However, we find that the coefficients on the interactions between *DYear1*, *DYear2*, and *Tax Avoidance Changes in Past Activist Events* are significantly positive when the dependent variables are *Current ETR*, *Cash ETR*, and *DD_BTD*. The results indicate that activists' past success in implementing tax changes is positively and significantly associated with increases in the target firm's tax avoidance in the current intervention. Table 6, Panel B, shows that the interactions between *DYear1*, *DYear2*, and dummy variable *Activist Exhibiting Tax Interest or Knowledge* are

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[29] We classify these interventions into the following five categories: (1) the activist states that she will discuss tax issues with managers, (2) the activist emphasizes the importance of tax efficiency in a specific business context (such as spin-off of a subsidiary in a tax efficient manner), (3) the activist applies advanced tax knowledge in her proposal to the target firm's manager to improve firm performance, (4) the activist explicitly suggests tax strategies, and (5) the activist indicates that she has either a tax director or has an external tax consultant. There are only around 20 cases in the fourth category.

[30] We also compare these 98 cases with other cases targeted by the same hedge fund activists, and find that the increases in tax avoidance levels associated with these 98 cases are greater than for other activist events (untabulated).

TABLE 6

Impact of Hedge Fund Activists' Characteristics

Panel A: Activists' Experience and Past Success in Eliciting Tax Changes

	Dependent Variables (Adjusted Values)			
	Current ETR	*Cash ETR*	*MP_BTD*	*DD_BTD*
Dummy for Year 0	−0.013	−0.016	0.018	0.007
	(−0.55)	(−0.65)	(0.38)	(0.47)
Dummy for Year +1	−0.009	−0.033	0.057	0.034**
	(−0.31)	(−1.17)	(1.32)	(2.21)
Dummy for Year +2	−0.056*	−0.073**	0.095***	0.021
	(−1.89)	(−2.37)	(2.64)	(1.34)
Activists' Experience	−0.009	−0.001	0.044*	0.001
	(−0.28)	(−0.03)	(1.72)	(0.06)
Dummy for Year 0 × *Activists' Experience*	0.020	0.013	−0.048	0.000
	(0.60)	(0.39)	(−0.94)	(0.02)
Dummy for Year +1 × *Activists' Experience*	−0.038	0.028	0.005	−0.023
	(−0.95)	(0.70)	(0.13)	(−1.46)
Dummy for Year +2 × *Activists' Experience*	−0.006	−0.015	−0.038	0.008
	(−0.13)	(−0.33)	(−1.17)	(0.46)
Tax Avoidance Changes in Past Activist Events	−0.075	−0.085	0.013	−0.358**
	(−1.06)	(−1.40)	(0.10)	(−2.13)
Dummy for Year 0 × *Tax Avoidance Changes in Past Activist Events*	0.102	0.145**	0.275	0.318**
	(1.44)	(2.12)	(1.38)	(2.18)
Dummy for Year +1 × *Tax Avoidance Changes in Past Activist Events*	0.210**	0.254***	0.069	0.428**
	(2.55)	(3.37)	(0.39)	(2.56)
Dummy for Year +2 × *Tax Avoidance Changes in Past Activist Events*	0.187**	0.220***	−0.054	0.341**
	(2.45)	(2.63)	(−0.41)	(1.99)
Control Variables (Adjusted Values)				
ROE	−0.043***	−0.067***	−0.017	0.006
	(−3.17)	(−5.22)	(−0.59)	(0.63)
Leverage	0.020	0.060	−0.199**	−0.023
	(0.48)	(1.38)	(−2.55)	(−0.98)

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TABLE 6 (continued)

	Dependent Variables (Adjusted Values)			
	Current ETR	*Cash ETR*	*MP_BTD*	*DD_BTD*
DNOL	0.022	0.002	0.029	0.017
	(0.97)	(0.08)	(0.86)	(1.34)
ΔNOL	0.158***	0.156***	−0.405***	−0.090***
	(3.54)	(3.92)	(−5.80)	(−4.10)
Foreign Income	−0.350	−0.738	0.022	−0.462*
	(−0.62)	(−1.14)	(0.04)	(−1.95)
PPE	−0.155***	−0.190***	−0.116	0.115***
	(−3.02)	(−4.03)	(−1.28)	(3.34)
Intangible Asset	−0.025	−0.115**	−0.166	0.042
	(−0.56)	(−2.34)	(−1.53)	(1.39)
Equity Income	−8.313***	−7.799***	2.356	−0.809
	(−2.60)	(−2.73)	(0.86)	(−0.80)
MTB	−0.003	−0.004*	−0.003	−0.003**
	(−1.36)	(−1.67)	(−0.86)	(−2.29)
Size	−0.027***	−0.030***	0.088***	0.024***
	(−4.28)	(−4.49)	(3.62)	(5.61)
No. of Observations	2,500	2,617	3,146	3,051
Adj. R^2	0.091	0.116	0.162	0.090

Panel B: Activists' Tax Interest and Knowledge

	Dependent Variables (Adjusted Values)			
	Current ETR	*Cash ETR*	*MP_BTD*	*DD_BTD*
Dummy for Year 0	−0.019	−0.031*	−0.005	−0.005
	(−1.32)	(−1.91)	(−0.46)	(−0.73)
Dummy for Year +1	−0.067***	−0.088***	0.013	−0.003
	(−3.59)	(−4.41)	(1.13)	(−0.31)
Dummy for Year +2	−0.119***	−0.154***	0.035**	0.000
	(−5.21)	(−6.31)	(2.55)	(0.03)

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TABLE 6 (continued)

	Dependent Variables (Adjusted Values)			
	Current ETR	***Cash ETR***	***MP_BTD***	***DD_BTD***
Activist Exhibiting Tax Interest or Knowledge	0.082	0.015	0.023	−0.018
	(1.57)	(0.28)	(0.76)	(−0.73)
Dummy for Year 0 × *Activist Exhibiting Tax Interest or Knowledge*	−0.184***	−0.080	0.035	0.057**
	(−3.39)	(−1.30)	(0.84)	(2.05)
Dummy for Year +1 × *Activist Exhibiting Tax Interest or Knowledge*	−0.214***	−0.137*	0.030	0.074*
	(−2.86)	(−1.87)	(0.60)	(1.87)
Dummy for Year +2 × *Activist Exhibiting Tax Interest or Knowledge*	−0.334***	−0.246***	0.073	0.057
	(−3.77)	(−2.80)	(1.27)	(1.24)
Control Variables (Adjusted Values)				
ROE	−0.060***	−0.073***	0.054***	0.015***
	(−4.49)	(−5.59)	(7.28)	(3.92)
Leverage	0.016	0.057	−0.048*	0.017
	(0.47)	(1.50)	(−1.87)	(1.16)
DNOL	0.069***	0.051**	−0.030***	−0.008
	(3.80)	(2.57)	(−2.93)	(−1.17)
ΔNOL	0.172***	0.186***	−0.265***	−0.114***
	(4.99)	(5.31)	(−10.25)	(−8.05)
Foreign Income	−0.937***	−1.296***	0.218	−0.245
	(−2.88)	(−3.60)	(1.10)	(−1.30)
PPE	−0.097**	−0.107**	0.001	0.084***
	(−2.37)	(−2.45)	(0.05)	(4.74)
Intangible Asset	−0.040	−0.095**	0.069**	0.058***
	(−1.01)	(−2.17)	(2.47)	(3.34)
Equity Income	−4.222**	−4.942**	2.009	−0.758
	(−2.02)	(−2.32)	(1.57)	(−0.91)
MTB	−0.000	−0.001	−0.004***	−0.003***
	(−0.12)	(−0.62)	(−2.80)	(−4.16)
Size	0.022***	0.021***	0.004	0.009***
	(5.84)	(5.05)	(1.53)	(5.31)
No. of Observations	3,452	3,555	4,272	4,168
Adj. R^2	0.162	0.160	0.271	0.207

(continued on next page)

TABLE 6 (continued)

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*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

This table presents the results on the impact of hedge fund activists' characteristics on changes in tax avoidance of target firms around the hedge fund activist events. The sample includes firm-years of target firms in the event window [−1, +2] with required data, where Event Year 0 is the year of the intervention announcement. *Activists' Experience* is measured by the number of intervention events initiated by the hedge fund activist during the past five years. (*Activists' Experience* is divided by 100 for exposition.) *Tax Avoidance Changes in Past Activist Events* is measured by the average changes in tax avoidance in the firms targeted by the hedge fund activist during the past five years. *Activist Exhibiting Tax Interest or Knowledge* is an indicator variable if the hedge fund activist has exhibited interest or knowledge in tax issues in one of her 13D or 13D/A filings during the sample period. For all dependent and control variables in the regressions, we adjust their values by the levels of the matched control firms. Control firms are identified as firms that have never been targeted by hedge funds during our sample period, but have the closest propensity scores to target firms. Propensity score is the predicted probability of becoming a hedge fund activism target in the next year, estimated from the model as in Brav et al. (2008, Table IV). Intercept, industry, and year dummies are included, but not reported. t-statistics reported in parentheses are based on heteroscedasticity robust standard errors clustered by firm. Appendix B contains the variable definitions.

TABLE 7

Changes in Tax Avoidance in Five Years after Intervention

	Current ETR	*Cash ETR*	*MP_BTD*	*DD_BTD*
Adjusted Annual Values				
Year −1	0.120***	0.124***	−0.053	−0.023
Year 0	0.068***	0.134***	0.014	0.002
Year +1	0.074***	0.102***	−0.008	−0.009
Year +2	0.046**	0.052**	0.047***	0.004
Year +3	0.046**	0.057**	−0.007	0.006
Year +4	0.072***	0.091***	0.034	0.012
Year +5	0.039**	0.058***	0.033	0.020*
Changes in Adjusted Annual Values				
(Year +4) − (Year −1)	−0.048*	−0.033	0.088**	0.035**
(Year +5) − (Year −1)	−0.081***	−0.066**	0.086**	0.043**
No. of Observations	221	269	343	319

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (one-sided), respectively.
This table reports the mean adjusted annual levels of tax avoidance for target firms around hedge fund activist events from Event Year −1 to Event Year +5, with Event Year 0 being the year of the intervention announcement, as well as the changes in these adjusted annual values. Adjusted values are computed as the differences between the levels of event firms and the levels of the matched control firms. Control firms are identified as firms that have never been targeted by hedge funds during our sample period, but have the closest propensity scores to target firms. Propensity score is the predicted probability of becoming a hedge fund activism target in the next year, estimated from the model as in Brav et al. (2008, Table IV). For each tax avoidance measure, we require a constant sample with non-missing data across the event window [−1, +5]. In the top section, we test whether the tax avoidance levels of target firms are statistically different from those of control firms in each event year. In the bottom section, we test whether the changes in tax avoidance levels of target firms differ significantly from those of control firms. Appendix B contains the variable definitions.

negatively and significantly associated with *Current ETR* and *Cash ETR*. We also find that the interaction between *DYear1* and dummy variable *Activist Exhibiting Tax Interest or Knowledge* is positively and significantly associated with *DD_BTD*. The results indicate that target firms experience greater improvements in their tax planning if their activists are known for expressing their tax interest or knowledge in SC 13D filings.

In summary, our evidence shows that hedge fund activists differ systematically in their impact on target firms' tax avoidance.[31] Target firms of activists with greater past success or expertise in implementing tax changes experience greater increases in tax avoidance following intervention.

V. ADDITIONAL TESTS

Changes in Tax Avoidance in Five Years after the Intervention

We extend the univariate analyses in Table 2 to examine the longer-term persistence of changes in tax avoidance. Similar to Table 2, we require a constant sample across the period in an attempt to mitigate the survivorship bias. We also adjust all values of tax avoidance measures for the levels of the propensity score matched control firms. Table 7 shows that the decreases in effective tax rates do not reverse across the ETR measures of tax avoidance. For instance, the change from Event Year −1 to Event Year +5 is −0.081 for the *Current ETR*, −0.066 for the *Cash ETR*, 0.086 for the *MP_BTD*, and 0.043 for the *DD_BTD*, respectively; all significant at the 5 percent level. Results in Table 7 show that, in general, changes in tax avoidance after hedge fund intervention do not reverse, at least in the intermediate term, indicating that hedge funds' influence on target firms' tax planning is not limited to short-term only.

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[31] We also conduct changes analysis, where we regress changes in tax avoidance on hedge fund characteristics and changes in control variables in Equation (2). Changes in both tax avoidance measures and control variables are computed relative to the levels in Year −1, and also adjusted for the corresponding values of control firms identified by propensity score matching. We find that all three hedge fund characteristics are associated with changes in tax avoidance. In particular, *Activists' Experience* is positively and significantly associated with changes in tax avoidance. Taken together, we conclude that both the levels and changes analyses provide evidence linking changes in corporate tax avoidance around intervention to hedge fund characteristics.

Is Increased Tax Avoidance Associated with Increased Use of Tax Sheltering?

We further analyze whether the increased tax avoidance is associated with target firms' increased use of high-risk, and potentially illegal, tax-planning strategies such as tax sheltering. The costs and benefits to engaging in a tax shelter are a frequent subject of interest in the literature. Graham and Tucker (2006) find that tax shelters provide firms with tax savings. Hanlon and Slemrod (2009) find a negative stock price reaction immediately following public revelation of a firm's involvement in a tax shelter. Firms have to balance the income-increasing and cash-saving benefits of aggressive tax planning with the potential monetary, reputation, and agency costs associated with such activities (Desai and Dharmapala 2006; Wilson 2009; Chen et al. 2010). Under certain circumstances, the benefits could be large enough to justify participation in a shelter. Thus, it is an empirical question as to whether hedge fund activist interventions are associated with an increase in the use of tax shelters. Wilson (2009) develops a model of firm characteristics to estimate the probability that a firm is currently engaging in tax sheltering based on firms publicly revealed to have been engaged in shelters. Lisowsky (2010) develops an alternative model using confidential IRS data for firms employing listed tax shelters.[32] Using the sheltering probability estimates from Wilson (2009) and Lisowsky (2010), we examine whether hedge fund activism is associated with increased probabilities of tax sheltering behaviors.[33] Untabulated results show no evidence that target firms are more likely to engage in tax sheltering after hedge fund intervention.[34] Our results, therefore, suggest that the greater tax savings achieved by target firms likely result from more efficient tax planning, rather than the use of egregious and risky tax evasion strategies.[35]

Do Hedge Fund Activists Simply Pick Target Firms that are Already Experiencing Increases in Tax Avoidance?

Brav et al. (2008) investigate whether hedge fund activists simply identify undervalued targets without adding to firms' fundamental values. Using various methodologies, they conclude that the average positive market response to an announcement of fund intervention is not likely driven by the stock picking effect. In our setting, we face a similar problem: hedge funds may simply pick target firms that were already experiencing increases in tax avoidance. Presumably, these target firms might continue the trend in tax avoidance even without hedge fund intervention. In previous sections, we show that changes in tax avoidance after hedge fund intervention are related to hedge fund characteristics such as past success and expertise in implementing tax changes. These results are inconsistent with a pure "stock-picking effect," and are more consistent with the "direct effects" of hedge fund activism on tax avoidance. To further strengthen our analysis, in this section, we conduct a trend analysis of tax avoidance prior to hedge fund intervention. If hedge funds simply cherry-pick target firms that are expected to continue to increase their tax avoidance, then we should expect target firms to exhibit an increasing trend for tax avoidance in years before fund intervention. We estimate the following regression to test the trends of tax avoidance prior to hedge fund intervention:

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[32] We thank Petro Lisowsky for kindly sharing the sheltering score estimates with us.

[33] Even though the use of tax sheltering might be pervasive, sheltering activities are notoriously difficult to detect (e.g., Graham and Tucker 2006). Academic studies on tax sheltering (Desai and Dharmapala 2006; Graham and Tucker 2006; Wilson 2009) usually rely on court documents and press reports to identify tax shelter firms, and the samples tend to be small. For instance, Wilson (2009) identifies only 59 shelter firms between 1975 and 2007.

[34] The univariate event-study analysis shows a slight decrease (about 1.9 percent) in the average industry-adjusted estimated sheltering probability for target firms after the intervention. After controlling for firm characteristics, the changes in the sheltering probabilities become insignificant in multivariate regressions.

[35] Our ability to make inferences is limited by the extent to which these sheltering models capture the profile of the broader set of tax shelter participants. Therefore, this analysis of sheltering activities is preliminary, and we caution readers not to over-interpret our results.

$$\begin{aligned} Tax\ Measure_{i,t} = {} & \beta_0 + \beta_1 TIME_{i,t} + \beta_2 ROE_{i,t} + \beta_3 Leverage_{i,t} + \beta_4 DNOL_{i,t} + \beta_5 \Delta NOL_{i,t} \\ & + \beta_6 Foreign\ Income_{i,t} + \beta_7 PPE_{i,t} + \beta_8 Intangible\ Asset_{i,t} \\ & + \beta_9 Equity\ Income_{i,t} + \beta_{10} MTB_{i,t-1} + \beta_{11} Size_{i,t-1} + Industry\ Dummies \\ & + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where *TIME* measures the number of years prior to the Event Year 0 (e.g., −10 to −1), and all other variables are as previously defined in Equation (1). For robustness, we examine several different pre-intervention event windows (untabulated): Years [−10, −1], Years [−5, −1], and Years [−3, −1], respectively. We find significantly positive coefficients on *TIME* for the ETR measures regardless of the event window length we use. For the BTD measures, the coefficients are either zero or negative, and mostly significant. Focusing on the period of [−5,−1], the coefficients on *TIME* for the *Current ETR*, *Cash ETR*, *MP_BTD*, and *DD_BTD* measures are 0.014, 0.017, −0.011, and −0.007, respectively, with all coefficients being significant.[36] The positive coefficients for the ETR measures and the negative coefficients for the BTD measures imply that, on average, the target firms were experiencing decreases in tax avoidance prior to hedge fund intervention. We conduct the same analyses for the matched control firms and do not find similar trends. The finding that the target firms do not continue the decrease in the levels of tax avoidance, but instead increase their levels of tax avoidance after hedge fund intervention, provides additional evidence on a direct effect of hedge fund intervention.

Other Robustness Checks

Desai and Dharmapala (2009) find that tax avoidance is positively associated with firm value when levels of institutional ownership are high, and McGuire et al. (2012) show that auditor expertise can affect a firm's tax avoidance. In unreported analyses, we also control for potential changes in external monitoring by institutional investors and auditors around hedge fund intervention. Specifically, we include the level of institutional ownership and a dummy variable that equals 1 if the target firm used a Big 6 auditor for the period. Our results are robust to these additional controls.

To address the possibility that our results are driven by time-invariant unobservable factors, we include firm-level fixed-effects in Equation (1) and Table 5. We find the results are similar to our original results.

Finally, to mitigate survivorship bias, we use the event window [−1, +2] in our tabulated analyses. One potential limitation with this approach arises from possible changes in tax strategy in the period prior to the intervention, as firm management may suspect a future activism event. To control for this possibility, in untabulated tests, we extend the event window from Years [−1, +2] to Years [−2, +2] and our conclusions remain similar.

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[36] These untabulated results are available upon request.

VI. DISCUSSION AND CONCLUSION

We extend the tax avoidance literature by examining the impact of an increasingly important class of shareholders—activist hedge funds on corporate tax avoidance. A main goal for hedge fund activists is to identify under-performing firms and adopt intervention and monitoring activities to improve the target firms' value. We propose that taxes can be an important consideration for hedge fund activists when they push target firms for value improvements, and that hedge fund activists' effective monitoring of effort-averse firm managers would improve the target firms' tax planning. Using propensity score matching to construct a control sample, we find that prior to hedge fund intervention, our target firms exhibit significantly lower tax avoidance than their control firms, as measured by both effective tax rates (ETRs) and book-tax differences (BTDs). In addition, we find the target firms experience significant increases in tax avoidance following activist funds' intervention.

An important concern for our research is that increases in tax avoidance may not come directly from hedge fund activists' influence on corporate tax planning, but could arise indirectly from other corporate changes (e.g., operating performance, leverage, and investment decisions) induced by the activism. To control for these indirect effects, we conduct several analyses. We first identify control samples matched on target firms' changes in operating performance, leverage, PPE, and intangible assets, and we find these control firms do not exhibit similar increases in tax avoidance as target firms do. We then use multivariate models with controls for a comprehensive set of potential tax determinants variables suggested in prior literature. To control for selection bias and market-wide changes of tax avoidance, we adjust values of all variables in our study by those of control firms identified through propensity score matching. We find consistent results under all these empirical methods. In addition, we find that increases in tax avoidance are associated with hedge funds' prior track record of implementing tax changes, and their interest and knowledge in tax planning as indicated in their SEC filings. Taken as a whole, the results suggest that our findings of improvements in tax avoidance in target firms are likely due to direct effects of hedge fund intervention.

One limitation of our study is that while we find robust support for increases in tax avoidance following hedge fund intervention events, we do not explicitly identify the tax mechanism underlying the changes. Unraveling the "black box" of tax strategies used to improve tax efficiency is difficult, if not impossible. This empirical challenge leads the most recent review on the tax literature to conclude that "the field cannot explain the variation in tax avoidance very well" (Hanlon and Heitzman 2010). While we provide anecdotal evidence on some specific tax strategies suggested by hedge fund activists in public filings, firms generally refrain from disclosing their tax mechanisms in detail, possibly out of concern with drawing the IRS's attention. Hence, future studies might seek to identify these specific tax strategies by using alternative methodologies, such as conducting surveys of hedge fund activists.

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APPENDIX A

ANECDOTAL EVIDENCE ON HEDGE FUND ACTIVISTS TARGETING CORPORATE TAX-PLANNING EFFICIENCY

Lawndale Capital Management vs. Westmore Land Coal Co

In September, 1996, hedge fund activist, Lawndale Capital Management Inc., in its Schedule 13D filing against its target firm, Westmore Land Coal Co, disclosed that it had discussed with the company's management and board of directors about strategies to maximize shareholder value. The discussion included issues on using the company's net operating loss and tax carryforwards for tax benefits.

Source: http://www.sec.gov/Archives/edgar/data/106455/0000935836-96-000034.txt

Third Point vs. Cypress Semiconductor

In August 2007, hedge fund activist Third Point LLC acquired more than 5.1 percent stake at Cypress Semiconductor and demanded the company address its undervaluation quickly.

Specifically, Third Point LLC insisted that there are tax-efficient ways to expedite the process of selling off Cypress Semiconductor's stake in Sunpower, and the company should immediately explore them.

Source: http://www.secinvestor.com/2007/08/10/Cypress+50+Undervalued+CY.aspx

New Mountain Ventures vs. National Fuel Gas

In October 2007, hedge fund activist New Mountain Ventures stated that it plans to nominate its own slate of directors for the board of its target firm, National Fuel Gas. One of New Mountain Ventures' demands for National Fuel Gas was to "change the ownership structure of some of its business in a way that would cut its tax bill."

Source: http://www.highbeam.com/doc/1P2-21663005.html

Greenlight Capital vs. MI Developments

In April 2008, hedge fund activist Greenlight Capital, Inc. publicly announces that it is not in favor of the Reorganization Proposal that its target firm, MI Developments, Inc. (MID), received from entities affiliated with Mr. Frank Stronach and intends to vote against it. One major concern for Greenlight Capital, Inc. about this reorganization is the tax consequence. Specifically, Greenlight Capital, Inc. stated, "The Term Sheet does not disclose the tax effects of the transaction on any of the parties. The Reorganization Proposal presents the following tax issues that should be evaluated, quantified and disclosed both as to the benefit to Mr. Stronach and as to the cost to MID and MID shareholders." Greenlight Capital, Inc. discussed the tax issues in detail, including issues on loss carryforwards, tax attributes of debt (historic tax basis), taxable foreign gains, capital gain taxes, Canadian non-resident withholding taxes, etc.

Source: http://www.sec.gov/Archives/edgar/data/1040272/000136231008002102/c73026exv13.htm

APPENDIX B
VARIABLE DEFINITIONS

Current ETR = current effective tax rate, which equals total income tax expense (#16) minus deferred income tax expense (#50), divided by pretax net income (#170) minus special items (#17) in year *t*. We truncate the values at 0 and 1;

Cash ETR = cash effective tax rate, which equals cash taxes paid (#317) divided by pretax net income (#170) minus special items (#17) in year *t*. We truncate the values at 0 and 1;

MP_BTD = Manzon and Plesko (2002) book-tax difference, which equals U.S. domestic income (#272) minus U.S. domestic taxable income minus state income taxes (#173) minus other income taxes (#211) minus equity in earnings (#55), scaled by lagged assets (#6). U.S. domestic taxable income is estimated as the current federal tax expense (#63) divided by the statutory maximum corporate tax rate;

DD_BTD = Desai and Dharmapala (2006) residual book-tax difference, which equals the residual from the following firm fixed-effect regression: $BT_{i,t} = \beta_1 TA_{i,t} + \mu_i + \varepsilon_{i,t}$, where *BT* is the Manzon-Plesko book-tax difference and *TA* is total accruals measured using the cash flow method per Hribar and Collins (2002). Both variables are scaled by lagged total assets and are winsorized at 1 percent and 99 percent levels for regression purposes;

Activists' Experience = number of intervention events initiated by the hedge fund activist during the past five years. (*Activists' Experience* is divided by 100 for exposition);
Tax Avoidance Changes in Past Activist Events = average changes in tax avoidance in the firms targeted by the hedge fund activist during the past five years;
Activist Exhibiting Tax Interest or Knowledge = indicator variable if the hedge fund activist has exhibited tax interest or knowledge in one of her 13D or 13D/A filings during the sample period;
TIME = number of years prior to the Event Year 0 (e.g., −10 to −1);
Dummy for Year $t = 1$ if the current year is the Event Year t, with Year 0 being the year in which hedge fund activism is initiated, 0 otherwise;
ROE = return on equity, measured as operating income (#170 – #192), scaled by lagged book value of equity (#60);
Leverage = long-term debt (#9), scaled by lagged assets (#6);
Dummy for Positive Lagged NOL (*DNOL*) = indicator variable coded as 1 if loss carryforward (#52) is positive at the beginning of the year;
Δ*NOL* = change in loss carryforward (#52), scaled by lagged assets (#6);
Foreign Income = foreign income (#273), scaled by lagged assets (#6);
PPE = property, plant, and equipment (#8), scaled by lagged assets (#6);
Intangible Asset = intangible assets (#33), scaled by lagged assets (#6);
Equity Income = equity income in earnings (#55), scaled by lagged assets (#6);
Lagged Market-to-Book = market-to-book ratio at the beginning of the year, measured as market value of equity (#199 × #25), and scaled by book value of equity (#60); and
Lagged Size = natural logarithm of market value of equity (#199 × #25) at the beginning of the year.

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Analyst Initiations of Coverage and Stock Return Synchronicity

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ABSTRACT: We examine how the information produced by analysts when they *initiate* coverage contributes to the mix of firm-specific, industry-, and market-wide information available about the firm. We hypothesize that the first analyst to initiate coverage provides low-cost market and industry information allowing him/her to follow more stocks, whereas subsequent analysts provide firm-specific information to distinguish themselves from existing analysts. We use stock return synchronicity to measure the mix of information available about a firm, with higher synchronicity indicating more industry and market information. Coverage initiations of firms with no prior analyst coverage increase synchronicity, suggesting that analysts produce industry- and market-wide information. In contrast, analysts initiating coverage on firms with existing coverage appear to focus on producing firm-specific information as these initiations lead to reduced synchronicity. Together, our findings indicate that the type of information that analysts produce at initiation depends on the information provided by other analysts.

Keywords: *stock return synchronicity; financial analysts; analyst initiations.*

Data Availability: *All data are available from public sources identified in the paper.*

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I. INTRODUCTION

A commonly held belief is that analysts primarily contribute firm-specific information to the market, which is logical given that much of the information produced by analysts relates directly to the firm in question, e.g., earnings forecasts and stock-specific recommendations. The belief is also supported by many studies that document associations between the information produced by analysts and market-adjusted firm returns (e.g., Schipper 1991; Ramnath et al. 2008). However, analysts also provide industry and market information to support their firm-level assessments, and many analysts make explicit recommendations regarding the industry outlook (Asquith et al. 2005; Kadan et al. 2011). Empirical evidence supports the use and importance of market- and industry-level information in analyst reports. For example, Ramnath (2002) shows that analysts revise their earnings forecasts when firms in the same industry make earnings announcements, and Hameed et al. (2010) document that analyst forecast revisions move prices of other firms in the industry. Furthermore, Piotroski and Roulstone (2004) argue that because analysts lack access to inside information (relative to institutions and insiders) they are likely to focus their efforts on collecting and interpreting industry- and market-wide information as it relates to the firms they cover, and their tests provide evidence consistent with these arguments.

In this study, we examine how the information produced by analysts when they *initiate* coverage contributes to the mix of firm-specific and industry- and market-wide information available about the firm, as measured by stock return synchronicity (measured as the R^2 value from a regression of firm returns on market and industry returns). We also explore whether the level of existing analyst coverage at the initiation affects the type of information analysts provide. These innovations provide two important contributions to the literature. First, coverage initiations represent a significant change in the information provided by analysts; thus, more new information is contained in initiation reports than in the reports of analysts engaged in continuing coverage. Evidence of this is found in studies that document abnormal returns around coverage initiations relative to other analyst activity (see Peterson 1987; Branson et al. 1998; Irvine 2003). We add to these studies by identifying how the mixture of firm-, industry-, and market-based information is rebalanced around initiations and thus identify compositional changes to the set of publicly available information that is associated with abnormal returns. Additionally, by examining how changes in coverage affect information, we alleviate concerns that analysts simply choose to follow firms that provide a specific mix of information to investors (see Piotroski and Roulstone 2004).[1]

Second, by conditioning our analysis on the existing level of coverage at the time of an initiation, we provide insight into whether analyst incentives to gather firm versus industry and market information depend on competition from other analysts—an issue that prior studies do not explore. This analysis is particularly important because analysts are thought to erode the informational advantage of insiders and institutions (Frankel and Li 2004). We examine whether this erosion is sensitive to the existing level of coverage.

Understanding analyst incentives to cover firms is important when examining how coverage initiations and competition influence the mix of information available about a firm. Analysts initiate coverage to generate lucrative trading commissions, drum-up investment banking business, and to provide information to important institutional clients (see Irvine 2003). Two important considerations affect the type of information they choose to provide: (1) information-gathering and -processing costs and (2) competition among other analysts. For new initiations (defined as initiations for firms without prior analyst coverage), only the former consideration applies. This implies that analysts will choose to gather and disseminate low-cost information because it allows them to follow a larger set of stocks, and covering more stocks enables analysts to increase trading commissions, attract more investment banking business, and increase their compensation (Groysberg et al. 2011).[2] Gathering market and industry information poses the lowest cost because it does not require access to management or detailed analysis of historical financial statements. While market and industry information is less costly to acquire, it may provide fewer benefits to investors in terms of their ability to make capital allocation decisions *vis-à-vis* firm-specific information. Nevertheless, market and industry information can facilitate investors' capital allocation decisions as it provides investors with important information about how a firm's fundamentals vary with the market or its industry. This is especially true in the case of new initiations because no other analyst-provided information exists. For subsequent initiations (defined as initiations for firms with prior analyst coverage), the decision regarding what type of information to provide is less trivial because analysts also need to concern themselves with competition from other analysts. Thus, subsequent analysts face a greater incentive to provide firm-specific information to distinguish themselves from other analysts already covering the firm.

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[1] While using a changes specification alleviates concerns that analysts simply choose to follow synchronous firms, our results are still subject to the concern that initiations are predetermined, i.e., variables that lead to changes in coverage could also precipitate changes in synchronicity. We discuss this concern in Sections III and V.

Based on this reasoning, the central hypothesis of our paper is that the first analyst initiating coverage of a firm provides more market and industry information, while subsequent analysts provide more firm-specific information. Consistent with our expectations, we find that analyst initiations of firms with no prior coverage increase the amount of industry and market information being impounded into price, as evidenced by an increase in synchronicity. Initiations of firms with existing coverage appear to decrease synchronicity, consistent with these initiations increasing the amount of firm-specific information flowing into price. Our results suggest that the type of information analysts provide in their initiation reports depends on whether the firm is being followed by other analysts.

Given that analysts choose to initiate coverage on firms, we must address the possibility that initiations are correlated with other events or characteristics that drive changes in stock return synchronicity. In our main tests we include control variables that may be correlated with both an analyst's decision to initiate coverage and changes in the mix of information regarding a given firm. To further address endogeneity concerns, we search for changes in analyst coverage that are plausibly exogenous to the individual firm. Specifically, we examine instances in which brokerage houses drop coverage of entire industry sectors.[3] We find that these "exogenous drops" in coverage are associated with increases in stock return synchronicity, mirroring the decreases in synchronicity associated with initiations at firms with existing coverage. We also use propensity score matching and find our results are robust to this methodology.

We also investigate whether changes in return synchronicity are the result of analyst initiations inducing co-movement among stocks for non-information reasons (see Barberis et al. 2005). We do so by adding the change in trading-volume synchronicity (defined as the extent to which market- and industry-level trading volume explain firm-level trading volume) to our main regressions and showing that our main results still hold. This suggests that analyst initiations change the mix of information available about the firm rather than simply causing stocks to trade in a more synchronous manner.

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[2] These benefits may come as a result of the analyst "planting the flag" on a stock. Being the first analyst to cover a stock can serve to set the analyst apart from others not only in terms of receiving future brokerage commissions, but also with respect to generating investment banking business. For example, the covered firm may be more likely to develop a professional relationship with the analyst who is the first to provide coverage that may raise the analyst's employer to the top of the list when it comes time for the firm to engage the services of an investment bank.

[3] This methodology is similar in spirit to that in Kelly and Ljungqvist (2012) who examine drops in coverage caused by the closing of brokerage houses.

To sharpen the inferences of our main empirical tests, we also examine alternative measures of the mix of information available regarding a firm. As earnings forecasts are a primary output of analysts' efforts, we examine whether initiations aid in incorporating firm-specific or industry-level earnings surprises into consensus forecasts. Specifically, we show that new initiations are more likely to mitigate the relation between industry earnings innovations and consensus forecast errors, while subsequent initiations are more likely to mitigate the relation between firm-specific earnings innovations and consensus forecast errors. Furthermore, we examine changes in market and industry betas and changes in return kurtosis (Roll 1988). The results with these measures also support our conclusion that the type of information analysts produce at initiation depends critically on the information being provided by other analysts.

The rest of the paper is organized as follows. Section II details prior literature and develops our central hypotheses and Section III discusses the research design. Section IV discusses the findings of our main tests and Section V discusses several robustness checks. Section VI concludes.

II. PRIOR LITERATURE AND HYPOTHESIS DEVELOPMENT

Analysts' Contribution to the Information Environment of the Firm

Piotroski and Roulstone (2004) are among the first to empirically examine the type of information that analysts contribute to the price formation process. They examine the association between stock return synchronicity and the trading activities of analysts, institutions, and insiders. Piotroski and Roulstone (2004) postulate that analysts typically have less access to firm-level information than do insiders and institutions, making them more likely to specialize in obtaining and mapping industry- and market-level information into prices.[4] In their tests, Piotroski and Roulstone (2004) find that analyst coverage is positively related to stock return synchronicity, which is consistent with analysts increasing the amount of market- and industry-level information included in firms' stock prices. The authors argue that the association is a result of analysts facilitating intra-industry information transfers, i.e., analysts use their industry expertise to better interpret and disseminate common information across all firms in an industry.

To provide additional intuition for the Piotroski and Roulstone (2004) results and our main hypothesis, we define industry- and market-level information as encompassing two non-mutually exclusive components: (1) information about the performance of a given industry and the broader macroeconomy and (2) any information that describes how firms' fundamentals covary with market and industry performance. Synchronicity changes whenever new information alters investors' understanding of how the fundamentals of the firm align with the fundamentals of the industry or macroeconomy. This is because the market is better equipped to understand the implications of industry and market news for a firm and incorporate this news into firms' share prices in a timely fashion.

Pushing this argument further, if analysts provide new information that a firm's earnings prospects are heavily tied to the state of the macroeconomy or industry outlook, then investors have increased confidence that upturns (downturns) in market and industry indices signal an increase (decrease) in the value of the firm's future cash flows. Thus, we expect firms' synchronicity to increase, since price movements will exhibit a higher tendency to mirror market and industry indices.

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[4] This assertion is supported by the tendency of analysts to specialize in a given industry (see Liang et al. 2008). Clement (1999) documents a positive association between forecast accuracy and the degree of analyst specialization.

One example of industry information comes from an initiation report on FiberNet Telecom, a telecommunications company, by an analyst at Merriman, Curhan, and Ford. In the report, the analyst describes how industry trends will affect the firm: "We expect FiberNet to benefit from macro industry trends, including firming pricing and increasing demand, as well as an improved operating structure . . . Similar to other data center companies, FiberNet is currently increasing its prices primarily due to the limited space that it and its competitors have" (Synesael 2006, 2, 4). Note that the analyst points out how FiberNet is similar to competitors and, thus, linking its price to the prices of other firms in the industry, which should increase synchronicity. We interpret this report as conveying industry information because it focuses on industry trends as the primary impetus for changes in FiberNet's performance prospects.

While Piotroski and Roulstone (2004) and others (e.g., Chan and Hameed 2006) show that analyst following is positively correlated with the relative amount of market- and industry-level information in prices, Liu (2011) suggests that the information analysts provide is predominantly firm-specific. Liu (2011) predicts that analysts produce more firm-specific information because its investment value is greater than the value of industry-specific information. To test his prediction, Liu (2011) examines short- and long-window market and industry adjusted returns surrounding analyst recommendation revisions and finds that revisions predominantly affect the firm-specific portion of returns.[5] Liu's (2011) arguments and results are intriguing because they suggest that analysts' incentives to gather firm-specific information will depend on analysts' beliefs about what information is already reflected in prices.[6]

We believe the type of firm-specific information referenced in Liu (2011) may include an analyst's analysis and discussion of novel strategies, products, services, and operations unique to the covered firm. One example of this type of information comes from a report by Rodman & Renshaw who initiated subsequent coverage on Conor Medsystems, a manufacturer of "CoStar" medical stents. In the report, the analyst provides a detailed description of Conor's unique product offerings: "Unlike conventional drug-eluting stents, the CoStar has unique laser drilled reservoirs that can be filled with multiple drug entities and then top-coated with a biodegradable polymer . . . We believe the CoStar can gain at least 8–10 percent market share in 2008, based on the strong clinical results so far" (Kalia 2005, 2). We interpret this report as conveying firm-specific information because it focuses on product developments unique to Conor Medsystems as the primary impetus for changes in the firm's fundamentals. This type of firm-specific information will tend to move the firm's price but have little impact on other firms and, thus, will reduce synchronicity.

Analyst Initiations

Our main empirical tests examine how coverage initiations affect stock return synchronicity. Initiations provide us with a powerful setting in which to test how analyst activity contributes to the flow of information because initiations likely contain more new information than continuing coverage reports. Several studies make this conjecture and then go on to show that initiations result in short-window, positive returns (see Peterson 1987; Branson et al. 1998; Irvine 2003; Demiroglu and Ryngaert 2010). Also, Demiroglu and Ryngaert (2010) document that new initiations of neglected stocks (those without any prior coverage) are particularly distinctive events. Finally, Schutte and Unlu (2009) show initiations defined at the firm-year level are associated with a decline in a measure of the noise in security prices. Although the results of these papers suggest that initiation events are notably different than continuing analyst coverage, they do not provide any evidence that the type of information contained in initiation reports is more firm-specific than it is market- and industry-related.

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[5] We note that our findings and the results in Liu (2011) both document that firm returns are predominantly driven by firm-specific information. Our focus is on how the mix of information in returns shifts with analyst activity and how these shifts are affected by the level of analyst coverage.

[6] In determining our research design, we considered using an approach similar to Liu's (2011). However, his methodology relies on estimating market and industry betas prior to recommendation revisions in order to calculate the firm, industry, and market components of returns. Because initiations shift market and industry betas (see Section V), his approach is not well suited for our setting. In addition, short-window returns around initiations reflect not only the information provided by analysts, but also the market's perception of the benefits that accompany new coverage (e.g., increased liquidity as in Irvine [2003]). Furthermore, the market's assessment of these benefits is likely to be different for firms with little or no coverage than it is for firms with a large analyst following. In fact, we know from Irvine (2003) and others that short-window returns surrounding initiations are more positive for initiations with less analyst coverage as new analyst coverage is generally viewed as a favorable event for these firms.

III. SAMPLE AND RESEARCH DESIGN

In this section, we describe our sample of analyst initiations and our synchronicity measure. We also formally specify our empirical tests.

Initiation Sample

We identify analyst initiations following the methodology of Irvine (2003) and Irvine et al. (2007).[7] We collect analyst recommendations on U.S. stocks from the I/B/E/S detail recommendation file. We count a recommendation as an initiation if it is the first time a given broker is associated with a particular firm on the I/B/E/S tape. We also require that it be the first time the analyst has covered the firm. This ensures that recommendations an analyst makes about a firm after the analyst moves from one brokerage house to another are not counted as initiations if the analyst covered the stock before switching employers. We also eliminate any initiations of coverage for IPO firms by deleting initiations that occur within three months of a firm's appearance on CRSP, our source of price and return data.

Occasionally, I/B/E/S adds brokerage firms to its coverage universe. Typically, these additions reflect a broadening of coverage by I/B/E/S rather than a response to the creation of a new brokerage firm. To ensure that we do not count simple database additions as initiations, we begin our sample in 1996 instead of 1994 when I/B/E/S first began gathering recommendations. We do so because coverage increased rapidly in the first several months of the I/B/E/S recommendation file. Furthermore, we eliminate initiations for brokerage firms that have not appeared in the I/B/E/S recommendation detail file for at least six months. After implementing these screens to identify initiations, we are left with 60,021 firm-months with at least one initiation from 1996 to 2006. We also require initiation firms to have the necessary data to compute the variables used in our study. This results in a sample of 39,855 firm-months with at least one initiation.[8] Table 1 shows the number of initiations by year. The number of initiation observations fluctuates over the sample period, with a high in 1998 of 5,069 and a low of 2,827 in 2006.

To determine whether the effects of coverage initiations on synchronicity vary based on the level of existing coverage at the time of the initiation, we measure the level of existing analyst coverage for each of our initiations. Specifically, we distinguish between initiations with no prior coverage (new initiations) and those with prior coverage (subsequent initiations). Initiations that

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[7] Irvine (2003) validates his sample of initiations by searching the Dow Jones News Service (DJNS) for press coverage of the initiations. All but two of the initiations identified by DJNS as initiations are initiations in his sample. Furthermore, the initiation dates, as recorded by DJNS, match the initiation dates on I/B/E/S in all but three cases.

[8] We report the number of initiations at the firm-month level because we examine how synchronicity changes around a given month. The number of unique initiation events in our sample is 44,707. Our sample size is smaller than the sample in Ertimur et al. (2011) because of data restrictions.

TABLE 1

Sample of Analyst Initiations

Panel A: Number of Initiations Each Year

	$INITIATION_{i,t}$	$INITIATION\_NEW_{i,t}$	$INITIATION\_SUB_{i,t}$
1996	3,877	365	3,512
1997	4,068	289	3,779
1998	5,069	236	4,833
1999	4,172	195	3,977
2000	3,943	214	3,729
2001	3,677	139	3,538
2002	3,221	198	3,023
2003	2,887	185	2,702
2004	3,119	175	2,944
2005	2,995	178	2,817
2006	2,827	128	2,699
Total	39,855	2,302	37,553

Panel B: Number of Analysts Initiating Coverage at the Initiation

	$INITIATION_{i,t}$	$INITIATION\_NEW_{i,t}$	$INITIATION\_SUB_{i,t}$
1	35,690	2,120	33,570
2	3,599	149	3,450
3	475	24	451
4	69	6	63
5	18	3	15
6	2	0	2
7	1	0	1
9	1	0	1

Panel C: Recommendation Level

	$INITIATION_{i,t}$	$INITIATION\_NEW_{i,t}$	$INITIATION\_SUB_{i,t}$
1 = Strong Buy	14,009	1,021	12,988
2 = Buy	15,060	932	14,128
3 = Hold	9,966	327	9,639
4 = Sell	499	14	485
5 = Strong Sell	321	8	313

This table presents the number of firm-months with coverage initiations ($INITIATION_{i,t}$) used in our primary tests as well as the breakdown between firm-months with new initiations ($INITIATION\_NEW_{i,t}$) and subsequent initiations ($INITIATION\_SUB_{i,t}$). See Appendix A for detailed variable definitions.

Panel A shows the number of firm-months with initiations in each year.

Panel B shows the number of analysts initiating coverage for each initiation.

Panel C shows the I/B/E/S recommendation associated with each initiation. The recommendation code is an integer between 1 and 5 where 1 = Strong Buy and 5 = Strong Sell.

occur at firms with (no) existing coverage are denoted by the indicator variable ($INITIATION\_NEW_{i,t}$) $INITIATION\_SUB_{i,t}$.

Table 1 shows the number of initiations based on the level of existing coverage; new initiations make up about 5 percent of all initiations. In Table 1, we also document occasions during which multiple analysts initiate coverage on a firm in the same month. While the vast majority of initiations are made by a single analyst, approximately 10 percent of the sample is comprised of initiations by more than one analyst. Finally, Table 1 shows the recommendation level associated with the initiations (1 = Strong Buy, 5 = Strong Sell). Initiation recommendations are overwhelmingly favorable, especially for new initiations. The proportion of strong buy recommendations for new initiations is 44 percent, while subsequent initiations are associated with a strong buy 35 percent of the time.

Synchronicity Literature

Roll (1988) was the first to formalize the notion that stock return synchronicity, the association between a firm's stock returns and market and industry returns, is negatively associated with the amount of firm-specific information being impounded into individual stock prices. Since Roll's (1988) work, several studies have provided empirical support for his interpretation of synchronicity. For example, Durnev et al. (2004) show that lower synchronicity is associated with the efficient allocation of capital within U.S. capital markets. They argue this relation is a result of firm-specific information being impounded into prices, allowing for greater monitoring and reduced information asymmetry between insiders and outsiders. Many papers interpret synchronicity similarly (e.g., Morck et al. 2000; Wurgler 2000; Durnev et al. 2003; Ferreira and Laux 2007). Alternatively, some papers argue and find evidence that low synchronicity or high firm-specific volatility is a measure of noise (e.g., West 1988; Kelly 2007; Teoh et al. 2008). Given the debate over synchronicity as a measure of the mix of information available about a given firm, our empirical analyses are joint tests of how analyst initiations contribute to the information environment of the firm and our interpretation of stock return synchronicity.[9] To support the results using synchronicity we also examine the relation between analyst initiations and three additional measures of information flow: the association between forecast errors and firm- and industry-components of earnings, market and industry betas, and return kurtosis (see Section V).

Measurement of Stock Return Synchronicity

We use a measure of stock return synchronicity that is similar to measures found in the prior literature. However, our empirical focus is different in that we examine changes in synchronicity around an analyst initiation. Given this focus, we measure synchronicity and changes in synchronicity using daily returns over a relatively short time period.

For each calendar month, we estimate a firm-level measure of return synchronicity. Specifically, for each firm-month observation, we regress daily returns on the value-weighted market return and the value-weighted two-digit SIC industry return, or:

$$RET_{i,t} = \alpha + \beta_1 MARET_{i,t} + \beta_2 INDRET_{i,t} + \varepsilon_{i,t}. \tag{1}$$

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[9] We interpret increases in return synchronicity as reflecting a decrease in the fraction of firms' returns that are explained by contemporaneous changes in the market and industry indices. The positive relation between new initiations and synchronicity suggests that analysts tend to provide information about how the fundamentals of the market or industry are likely to covary positively with the fundamentals of the covered firm. An additional and non-mutually exclusive interpretation is that new initiations increase synchronicity by reducing estimation risk associated with market and industry betas. We examine this interpretation in Section V, where we measure changes in market and industry betas around initiations.

The industry return ($INDRET_{i,t}$) for a specific day t, is created using all firms with the same two-digit SIC code, with firm i's daily return omitted. $INDRET_{i,t}$ is the value-weighted average of these firms' day t returns. We estimate this regression using daily returns from the past three months for each firm-month, with a minimum of 50 daily observations. For example, IBM's synchronicity measure for May 2000 is estimated using daily returns from March, April, and May 2000. All stock return data are gathered through CRSP. Following the definition in Morck et al. (2000), we define synchronicity as:

$$SYNCH_{i,t} = \log\left(\frac{R^2}{1-R^2}\right). \tag{2}$$

where R^2 is the coefficient of determination from the estimation of Equation (1).[10] The log transformation of R^2 creates an unbounded continuous variable out of a variable originally bounded by 0 and 1. *SYNCH* is measured for each firm-month in the sample. By construction, high values of *SYNCH* indicate firms whose stock returns are closely tied to and vary strongly with market and industry returns and whose returns reflect relatively less firm-specific information.

Empirical Tests

The Effect of Initiations on Synchronicity

To determine if analyst initiations increase the amount of firm-specific or market- and industry-wide information impounded into prices we estimate the following equation using OLS:

$$\begin{aligned}\Delta SYNCH_{i,t} = {} & \alpha_0 + \beta_1 INITIATION_{i,t} + \beta_2 INITIATION_{i,t} * LNUM_{i,t-1} + \beta_3 LNUM_{i,t-1} \\ & + \beta_4 \Delta LMVE_{i,t} + \beta_5 \Delta INST_{i,t} + \beta_6 \Delta TURN_{i,t} + \beta_7 RET\_12_{i,t} + \beta_8 ISSUE\_12_{i,t} \\ & + \beta_9 REPORT_{i,t-1} + \beta_{10} GUIDE\_6_{i,t} + \beta_{11} LBM_{i,t-1} + \beta_{12} STDROA_{i,t} \\ & + \beta_{13} REG_{i,t} + \sum \alpha_i FIRM_i + \varepsilon_{i,t}.\end{aligned} \tag{3}$$

In all of our analyses, we provide standard errors corrected for cross-sectional and serial correlation by clustering at the industry (two-digit SIC) level. Our OLS estimations of Equation (3) also include firm fixed effects.

We define the variables as follows: $\Delta SYNCH_{i,t}$ is the change in synchronicity, defined as $SYNCH_{i,t+3} - SYNCH_{i,t-1}$.[11] $LNUM_{i,t-1}$ is the natural log of 1 plus the number of analysts covering firm i in month $t-1$. The estimated coefficients on $INITIATION_{i,t}$ and $INITIATION_{i,t} * LNUM_{i,t-1}$ are of primary interest in Equation (3). The coefficient on $INITIATION_{i,t}$ reveals how synchronicity responds to initiations, and the interaction term captures whether the type of information provided in initiation reports depends on the existing level of coverage. We define the remaining variables in Appendix A.

In addition to Equation (3), we introduce an alternative approach to estimating how the level of existing coverage can affect the relation between initiations and changes in synchronicity as follows:

$$\begin{aligned}\Delta SYNCH_{i,t} = {} & \rho_0 + \gamma_1 INITIATION\_NEW_{i,t} + \gamma_2 INITIATION\_SUB_{i,t} + \gamma_3 DROP_{i,t-1} \\ & + \gamma_4 DROP\_ZERO_{i,t-1} + \gamma_5 \Delta LMVE_{i,t} + \gamma_6 \Delta INST_{i,t} + \gamma_7 \Delta TURN_{i,t} \\ & + \gamma_8 RET\_12_{i,t} + \gamma_9 ISSUE\_12_{i,t} + \gamma_{10} REPORT_{i,t-1} + \gamma_{11} GUIDE\_6_{i,t} \\ & + \gamma_{12} LBM_{i,t-1} + \gamma_{13} STDROA_{i,t} + \gamma_{14} REG_{i,t} + \sum \rho_i FIRM_i + \varepsilon_{i,t}.\end{aligned} \tag{4}$$

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[10] We use the adjusted R^2 in all of our empirical tests. We follow the literature and trim negative adjusted R^2 numbers at 0.0001.

[11] We define the change in synchronicity using two, non-overlapping measures of synchronicity. $SYNCH_{i,t+3}$ uses data from the three months following month t while $SYNCH_{i,t-1}$ uses data from the three months preceding month t.

In Equation (4) we define four new variables that take the place of $LNUM_{i,t-1}$ in Equation (3). If analysts contribute relatively more industry- and market-related (firm-specific) information about the firms they cover, then the estimated coefficients on $INITIATION\_NEW_{i,t}$ and $INITIATION\_SUB_{i,t}$ should be positive (negative). For completeness, we also define two variables related to decreases in analyst coverage: $DROP_{i,t}$ is an indicator variable set equal to 1 if the level of analyst coverage declines from month $t-1$ to t. $DROP\_ZERO_{i,t}$ is an indicator variable set equal to 1 if analyst coverage is non-zero in month $t-1$ and becomes 0 in month t. We include these variables to examine whether the loss of coverage provides insight into the type of information provided by analysts.[12]

The remaining variables in Equations (3) and (4) are controls. Fundamentally, variables should be included if they affect the type of information flowing into prices. Because we are not aware of prior studies that examine the determinants of shifts in stock return synchronicity, we base the variables included in our model on intuition and the results of related studies (see Bhushan 1989; O'Brien and Bhushan 1990). Additionally, we attempt to control for variables tied to analyst incentives that may also influence synchronicity. $\Delta LMVE_{i,t}$ is the change in the natural log of market value, defined as $LMVE_{i,t+1} - LMVE_{i,t-1}$. We include $\Delta LMVE_{i,t}$ because changes in size are likely to be associated with various dimensions of the firm's information environment, including media exposure and the overall level of investor interest. $\Delta INST_{i,t}$ is the average value of $INST$ in months $t+1$ to $t+6$, less the average value of $INST$ in months $t-1$ to $t-6$. We include variables related to institutional holdings because of their association with synchronicity, as documented in Piotroski and Roulstone (2004); in addition, analysts' initiations may be correlated with changes in institutional ownership. $TURN_{i,t}$ is the number of shares traded in month t divided by the number of shares outstanding as identified in CRSP. $\Delta TURN_{i,t}$ is the average value of $TURN$ in months $t+1$ to $t+6$, less the average value of $TURN$ in months $t-1$ to $t-6$. As discussed in Section IV, we include share turnover to control for changes in synchronicity that are driven by increased investor awareness that is unrelated to the type of information in analysts' reports. $RET\_12_{i,t}$ is the market-adjusted return on the firm's stock from $t-12$ to $t-1$, as measured by CRSP. We control for past returns as a summary measure of the information being released about the firm over the prior year. Further, analysts prefer to initiate coverage on stocks that have performed well (McNichols and O'Brien 1997). $ISSUE\_12_{i,t}$ is an indicator variable set equal to 1 if the firm issued equity in the past 12 months, 0 otherwise, which we include because issuing firms may be undergoing changes that affect how they relate to other firms in the industry and market. $REPORT_{i,t-1}$ is an indicator variable set equal to 1 if the firm reported earnings in month $t-1$, 0 otherwise. $GUIDE\_6_{i,t}$ is an indicator variable set equal to 1 if the firm issued earnings guidance within the last six months. We gather earnings release dates from Compustat and earnings guidance dates from First Call. We include $REPORT_{i,t-1}$ and $GUIDE\_6_{i,t}$ because they are information events that may affect synchronicity.[13] $LBM_{i,t-1}$ is the natural log of the book-to-market ratio in month $t-1$. $STDROA_{i,t}$ is the standard deviation of return on assets (ROA) measured over the current and previous four quarters. We include these two variables because analysts have been shown to prefer growth firms and firms with less volatile earnings. $REG_{i,t}$ is an indicator variable for financial services or utilities, which we include because firms in regulated industries are subject to common constraints on their operations, making it more likely that their returns move together.

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[12] Dropped coverage may or may not have the same effect on synchronicity as initiations. On the one hand, when an analyst drops coverage there will be a decrease in the amount of information available about a firm, which would suggest that synchronicity changes accordingly. On the other hand, the analyst's recent reports and forecasts are not removed from the information set available to investors, so the corresponding change in synchronicity could happen over a long period of time as the analyst's information becomes stale and less relevant to current circumstances. Ultimately, the issue of whether drops affect synchronicity is an empirical question.

[13] Barron et al. (2002) show that earnings announcements trigger analyst information production. Also, Stickel (1989) finds that analysts supply more forecasts following large, negative earnings surprises.

TABLE 2

Descriptive Statistics for the Sample of Firm-Month Observations Available between Fiscal Years 1996 and 2006

	n	Mean	Std. Dev.	25th Pctl.	Median	75th Pctl.
Synchronicity Variables						
R^2	613,111	0.1105	0.1560	0.0001	0.0383	0.1621
$SYNCH_{i,t}$	613,111	−4.4808	3.4598	−9.2102	−3.32230	−1.6429
$\Delta SYNCH_{i,t}$	613,111	0.0472	3.6050	−1.2132	0.0000	1.3239
Analyst Coverage Variables						
$INITIATION_{i,t}$	613,111	0.0650	0.2465	0.0000	0.0000	0.0000
$INITIATION\_NEW_{i,t}$	613,111	0.0038	0.0612	0.0000	0.0000	0.0000
$INITIATION\_SUB_{i,t}$	613,111	0.0612	0.2398	0.0000	0.0000	0.0000
$DROP_{i,t}$	613,111	0.0763	0.2655	0.0000	0.0000	0.0000
$DROP\_ZERO_{i,t}$	613,111	0.0033	0.0574	0.0000	0.0000	0.0000
$NUM_{i,t}$	613,111	4.3983	5.8718	0.0000	2.0000	6.0000
$LNUM_{i,t}$	613,111	1.1874	0.9963	0.0000	1.0986	1.9459
Other Variables						
$LMVE_{i,t}$	613,111	12.1400	2.0435	10.6609	11.9659	13.4607
$\Delta LMVE_{i,t}$	613,111	0.0022	0.2683	−0.1077	0.0082	0.1210
$INST_{i,t}$	613,111	0.3112	0.3006	0.0232	0.2250	0.5483
$\Delta INST_{i,t}$	613,111	0.0174	0.0878	−0.0072	0.0009	0.0346
$TURN_{i,t}$	613,111	1.2787	2.6440	0.2887	0.6741	1.4557
$\Delta TURN_{i,t}$	613,111	0.0271	2.2525	−0.2153	−0.0010	0.2139
$RET\_12_{i,t}$	613,111	0.0606	0.9188	−0.3694	−0.0749	0.2548
$ISSUE\_12_{i,t}$	613,111	0.2196	0.4140	0.0000	0.0000	0.0000
$REPORT_{i,t-1}$	613,111	0.3191	0.4661	0.0000	0.0000	1.0000
$GUIDE\_6_{i,t}$	613,111	0.2050	0.4037	0.0000	0.0000	0.0000
$BM_{i,t-1}$	613,111	0.6869	0.8141	0.2996	0.5197	0.8274
$LBM_{i,t-1}$	613,111	−0.7327	0.8809	−1.2053	−0.6545	−0.1894
$STDROA_{i,t}$	613,111	0.0254	0.1410	0.0027	0.0087	0.0247
REG_i	613,111	0.0461	0.2098	0.0000	0.0000	0.0000

All variables are as defined in Appendix A.

IV. EMPIRICAL RESULTS

Univariate Statistics

Table 2 presents descriptive statistics for the variables we use in the study. For comparison purposes, we report the statistics for the raw R^2 values although we do not use this measure in any of our empirical tests. The average R^2 value is about 11.05 percent.[14] The average values of $SYNCH_{i,t}$ and $\Delta SYNCH_{i,t}$ are −4.4808 and 0.0472. Evaluated at the mean of R^2, this change in synchronicity represents a 0.47 percentage point increase in R^2 from 11.05 percent to 11.52 percent. Initiations occur in roughly 6 percent of the firm-month observations indicating that they are relatively infrequent events. Finally, a significant portion of our firm-months have no analyst coverage, as reflected in the first quartile of $NUM_{i,t-1}$ being 0.

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[14] Morck et al. (2000) report an average R^2 in the U.S. of approximately two percent. Piotroski and Roulstone (2004) report an R^2 of over 19 percent in their sample. Our estimate falls between these two figures, which is likely due to (1) Piotroski and Roulstone (2004) including both current and lagged market and industry returns as explanatory variables in their synchronicity regressions; and (2) Morck et al. (2000) including only the market return in their regressions.

Table 2 also displays summary statistics for the control variables in the study. The median value of $LMVE_{i,t-1}$ is 11.97, which corresponds to a market value of approximately $157 million. Second, institutions own 31 percent of the average firm's shares. Third, on average, returns are positive in our sample, and only 4.6 percent of the firms in the sample are from regulated industries.

In untabulated results, we find that $\Delta SYNCH_{i,t}$ is not strongly correlated (all correlations with $\Delta SYNCH_{i,t}$ are less than $|0.05|$) with the independent variables included in Equations (3) and (4) or the other variables used in the analysis. Further, a strong positive (negative) relation exists between $LNUM_{i,t-1}$ and $INITIATION\_SUB_{i,t}$ ($INITIATION\_NEW_{i,t}$) because only firms with (without) coverage can have a subsequent (new) initiation.

The Impact of Initiations on Changes in Synchronicity

Table 3 reports the results of estimating Equations (3) and (4). The coefficient on $INITIATION_{i,t}$ reported in column (1) is –0.0484, suggesting that an initiation reduces synchronicity. The results in column (2) indicate that the negative effect of an initiation increases in the level of existing coverage. In fact, the effect of an initiation on changes in synchronicity is positive when prior coverage is 0. The results from estimating Equation (4) corroborate the preceding findings. Column (3) of Table 3 shows the estimated coefficient on $INITIATION\_SUB_{i,t}$ is negative and significant, while the coefficient on $INITIATION\_NEW_{i,t}$ is positive and significant. The absolute magnitude of the coefficient on $INITIATION\_NEW_{i,t}$ is approximately three times that of the coefficient on $INITIATION\_SUB_{i,t}$, suggesting that the absolute magnitudes of the impact on synchronicity is greater for first-time initiations than subsequent initiations. Initiations have significant economic effects with a new initiation producing a 2.38 percentage point shift in the average R^2 value from 11.05 percent to 13.43 percent. Subsequent initiations result in a downward shift in the R^2 value of 0.642 percentage points. Column (4) shows that decreases in coverage do not appear to affect synchronicity.

We interpret the results in Table 3 as indicating first that analysts initiating coverage at firms with no prior coverage provide relatively more industry- and market-related information about these firms as evidenced by the increase in synchronicity around the initiation. This is consistent with analysts gathering information that is relatively inexpensive to acquire but potentially valuable for investors seeking to place a newly covered firm in the context of its market and industry. Second, analysts initiating coverage at firms with existing coverage provide relatively more firm-specific information about the covered firm. These results are consistent with analysts only initiating coverage if they have a relevant piece of firm-specific information to share with investors since industry- and market-related information has already been provided by other analysts. These results suggest that the type of information analysts produce in their initiation reports depends on the information that other analysts already provide.[15]

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[15] We also examine whether the effects of initiations vary across analyst and recommendation characteristics. We identify seven characteristics and interact them separately with $INITIATION\_NEW_{i,t}$ and $INITIATION\_SUB_{i,t}$ in Equation (4). The characteristics are as follows: (1) the level of the I/B/E/S recommendation code (an integer between 1 and 5 where 1 = Strong Buy and 5 = Strong Sell); (2) analyst experience measured as the number of months since the analyst appeared in I/B/E/S; (3) analyst employer resources captured by whether the analyst is employed at a brokerage in the largest size decile of brokerages (measured by number of analysts); (4) analyst specialization in a given industry; (5) whether an analyst initiates coverage on a firm in his industry of specialization; (6) the number of firms already covered by the analyst; (7) all-star analysts, as ranked by *Institutional Investor* magazine. Our untabulated results show little effect from these analyst-characteristic variables. Further, untabulated tests reflect no evidence that multiple initiations have a differential effect on synchronicity. Finally, we examine whether the intensity of coverage in a given industry affects how initiations impact synchronicity and find that initiations appear to have a uniform effect on synchronicity regardless of the intensity of coverage in a particular industry.

TABLE 3

Estimation of the Relation between Changes in Stock Return Synchronicity and Analyst Initiations

$$\begin{aligned}\Delta SYNCH_{i,t} = {} & \alpha_0 + \beta_1 INITIATION_{i,t} + \beta_2 INITIATION_{i,t} {*} LNUM_{i,t-1} + \beta_3 LNUM_{i,t-1} \\ & + \beta_4 \Delta LMVE_{i,t} + \beta_5 \Delta INST_{i,t} + \beta_6 \Delta TURN_{i,t} + \beta_7 RET\_12_{i,t} + \beta_8 ISSUE\_12_{i,t} \\ & + \beta_9 REPORT_{i,t-1} + \beta_{10} GUIDE\_6_{i,t} + \beta_{11} LBM_{i,t-1} + \beta_{12} STDROA_{i,t} \\ & + \beta_{13} REG_{i,t} + \sum \alpha_i FIRM_i + \varepsilon_{i,t}.\end{aligned}$$

$$\begin{aligned}\Delta SYNCH_{i,t} = {} & \rho_0 + \gamma_1 INITIATION\_NEW_{i,t} + \gamma_2 INITIATION\_SUB_{i,t} + \gamma_3 DROP_{i,t-1} \\ & + \gamma_4 DROP\_ZERO_{i,t-1} + \gamma_5 \Delta LMVE_{i,t} + \gamma_6 \Delta INST_{i,t} + \gamma_7 \Delta TURN_{i,t} + \gamma_8 RET\_12_{i,t} \\ & + \gamma_9 ISSUE\_12_{i,t} + \gamma_{10} REPORT_{i,t-1} + \gamma_{11} GUIDE\_6_{i,t} + \gamma_{12} LBM_{i,t-1} \\ & + \gamma_{13} STDROA_{i,t} + \gamma_{14} REG_{i,t} + \sum \rho_i FIRM_i + \varepsilon_{i,t}.\end{aligned}$$

Estimation	(1)	(2)		(3)	(4)
$INITIATION_{i,t}$	−0.0484	0.0656	$INITIATION\_NEW_{i,t}$	0.2221	0.2226
	(−3.49)***	(1.78)*		(3.25)***	(3.25)***
$INITIATION * LNUM_{i,t}$		−0.0610	$INITIATION\_SUB_{i,t}$	−0.0670	−0.0669
		(−3.05)***		(−4.16)***	(−4.05)***
$LNUM_{i,t-1}$	−0.1480	−0.1435	$DROP_{i,t}$		0.0010
	(−13.81)***	(−13.51)***			(0.05)
			$DROP\_ZERO_{i,t}$		0.0450
					(0.51)
$\Delta LMVE_{i,t}$	0.0232	0.0227	$\Delta LMVE_{i,t}$	0.0434	0.0434
	(0.70)	(0.69)		(1.29)	(1.29)
$\Delta INST_{i,t}$	0.5532	0.5489	$\Delta INST_{i,t}$	0.5750	0.5750
	(8.07)***	(8.07)***		(8.02)***	(8.03)***
$\Delta TURN_{i,t}$	−0.0164	−0.0164	$\Delta TURN_{i,t}$	−0.0157	−0.0157
	(−3.58)***	(−3.58)***		(−3.46)***	(−3.45)***
$RET\_12_{i,t}$	0.1374	0.1372	$RET\_12_{i,t}$	0.1441	0.1441
	(14.50)***	(14.53)***		(15.09)***	(15.12)***
$ISSUE\_12_{i,t}$	−0.0938	−0.0939	$ISSUE\_12_{i,t}$	−0.0986	−0.0986
	(−4.70)***	(−4.69)***		(−4.62)***	(−4.61)***
$REPORT_{i,t-1}$	0.0475	0.0474	$REPORT_{i,t-1}$	0.0472	0.0472
	(7.68)***	(7.66)***		(7.58)***	(7.56)***
$GUIDE\_6_{i,t}$	0.0373	0.0372	$GUIDE\_6_{i,t}$	0.0101	0.0101
	(2.07)**	(2.06)**		(0.60)	(0.59)
$LBM_{i,t-1}$	−0.2085	−0.2080	$LBM_{i,t-1}$	−0.1992	−0.1993
	(−14.17)***	(−14.20)***		(−12.55)***	(−12.48)***
$STDROA_{i,t}$	0.0417	0.0418	$STDROA_{i,t}$	0.0474	0.0474
	(2.64)***	(2.61)***		(2.47)***	(2.48)***
$REG_{i,t}$	−0.1246	−0.1248	$REG_{i,t}$	−0.1041	−0.1041
	(−1.84)*	(−1.84)*		(−1.36)	(−1.36)
Intercept	0.0576	0.0530	Intercept	−0.1058	−0.1060
	(2.53)***	(2.32)***		(−6.21)***	(−6.13)***
R^2	0.0104	0.0104	R^2	0.0102	0.0102
n	613,111	613,111	n	613,111	613,111

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

All variables are defined in Appendix A. t-statistics (in parentheses) are based on standard errors that are robust to heteroscedasticity and correlation across observations from the same two-digit SIC industry.

The coefficients on the control variables in Table 3 indicate that increases in the percentage of shares held by institutions, positive past returns, and the release of financial information increase synchronicity, while an increase in share turnover and the issuance of equity decrease synchronicity.

V. ROBUSTNESS CHECKS

This section reports checks on the robustness of the main empirical findings of Section IV.

Endogeneity

Work by McNichols and O'Brien (1997) and the theoretical model of Hayes (1998) provide evidence of endogenous coverage decisions. To the extent that expected changes in return synchronicity endogenously determine analyst coverage decisions, our coefficient estimates may be biased. To mitigate these concerns we examine instances in which a brokerage firm appears to exogenously drop coverage of an entire sector.

While we are unable to rule out the possibility that sector terminations are also endogenous, sector terminations are usually associated with brokerage firms shifting their focus across industries, brokerage closures, or changes in analyst coverage associated with brokerage firm mergers and acquisitions (Hong and Kacperczyk 2010). Using the I/B/E/S Detail Estimates file, we identify sector terminations as instances in which a brokerage firm (1) provides coverage of at least ten firms within a given four-digit SIC code and (2) ceases providing coverage on at least 75 percent of these firms in a given quarter, and (3) does not resume coverage within the next two quarters. This methodology identifies 1,643 firm-quarters of dropped coverage attributed to sector terminations. The resulting variable, $EXOG_{i,q}$, equals 1 in the presence of an exogenous drop in quarter q, and 0 otherwise. We treat all other sector drops failing to meet the above three criteria as endogenous.

Since we use the absence of analysts' forecasts within a two-quarter span as an indication of dropped coverage, drops in analyst coverage are identified at the quarterly level. Thus, to investigate the impact of exogenous changes in analyst coverage on return synchronicity, our main dependent variable is the difference in return synchronicity in the three months following a given quarter versus the three months prior to the quarter. Analogous to our main dependent variable above, we define $\Delta SYNCH_{i,q}$ as $SYNCH_{i,q+1} - SYNCH_{i,q-1}$, where the time subscript denotes the quarter of measurement.

In columns (1) and (2) of Table 4, the coefficient on $EXOG_{i,q}$ is positive and significant suggesting that reductions in analyst coverage increase return synchronicity. These results are consistent with those in Table 3 where subsequent initiations are associated with decreases in return synchronicity. Overall, to the extent that sector drops reflect exogenous changes in analyst coverage, the results of this section provide supporting evidence for our main findings.

As an alternative approach to addressing endogeneity concerns, we employ propensity score matching to match firm-months with initiations to firm-months without initiations that are similar along multiple observable dimensions (Rosenbaum and Rubin 1983). Within each calendar month, we estimate the likelihood of an initiation using a probit regression, where the dependent variable equals 1 when an analyst initiates coverage.[16] We transform the fitted values from estimating this probit regression into a propensity score. The propensity score is the conditional probability of receiving an initiation in a given month conditional on the firm controls used in our initiation model. We pair firms with an initiation with two control firm observations using the nearest neighbor algorithm. Matching each initiation firm with two non-initiation firms provides added power for our tests, yielding a sample of 119,565 firm-months, corresponding to three observations for each of the 39,855 initiations.[17]

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[16] We follow prior research in choosing variables associated with the probability of an initiation (see Liang et al. 2008). Our model of initiations includes firm size, prior analyst following, institutional ownership, trading volume, past returns, book-to-market, earnings variability, and indicators for events such as earnings announcements, management forecasts, and merger activity.

Columns (3) and (4) in Table 4 report the results from re-estimating Equation (3) using the propensity score matched sample. Column (3) results demonstrate that the coefficient on $INITIATION_{i,t}$ is positive and significant while the interaction term has a negative and significant coefficient. To mitigate concerns about heterogeneity that is not captured by our matching algorithm, column (4) includes the main controls used in our primary analyses.[18] We again find that the coefficient on $INITIATION_{i,t} * LNUM_{i,t-1}$ is significantly negative, while the coefficient on $INITIATION_{i,t}$ is significantly positive. These findings mirror those in Table 3, suggesting that new initiations contribute primarily to industry- and market-information while subsequent initiations contribute to firm-specific information. The consistency of our findings within the multivariate, propensity score matching approach mitigates concerns regarding selection based on observables and provides additional support for our main tests.

Trading Co-Movement and the Speed of Information Flow

Barberis et al. (2005, 284) point out that if investors place stocks into specific groups such as industries, and then allocate resources to these groups of stocks instead of individual assets, this process can generate co-movement. In the context of our study, initiations could introduce stocks to investors, causing them to group these stocks with other firms in their respective industries, thereby increasing synchronicity as the stocks trade together. We attempt to rule out this possibility by using synchronicity in *trading volume* to capture the shift in return synchronicity that is driven by the increased awareness of the stock but is unrelated to the information provided by the analyst.[19,20] We re-estimate Equations (3) and (4) after controlling for changes in volume synchronicity (untabulated) and find that the positive (negative) association between new (subsequent) initiations and the change in synchronicity is still present and of similar magnitude to the results presented in Table 3.

Analysts may also impact return synchronicity without affecting the mix of information available about a firm by simply speeding up the flow of information into prices. Implicitly, our methodology for measuring synchronicity assumes that market and industry price movements map into a firm's stock price on a daily basis, but a firm's price may actually take more than a day to synchronize with the market and industry (Barberis et al. 2005). To address this issue we calculate our synchronicity measure using two lags of the industry and market stock return and then re-estimate Equation (4). Our untabulated results show no change in the relation between initiations and changes in synchronicity. New (subsequent) initiations continue to be positively (negatively) related to changes in synchronicity with no significant decreases in the magnitude of coefficients in Table 3.

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[17] Matching to only one other observation yields similar results.

[18] As discussed in Armstrong et al. (2010), the most common way to assess the reasonableness of the propensity score approach is to examine the covariate balance between the treatment group (firms with initiations) and control group (firms without initiations). Covariate balance is not achieved in our case, but this is common in practice (see Ho et al. 2007) and the recommended approach is to estimate a regression of the outcome (change in synchronicity) as a function of the treatment and the control variables used in the propensity score model.

[19] Subsequent initiations could also induce co-movement in trading, but given that our results document a decrease in synchronicity for these initiations and that investors should already be aware of firms with coverage, we believe this is less likely to be the case than for new initiations.

[20] To estimate trading volume synchronicity we regress shocks to daily share turnover, measured as shares traded scaled by shares outstanding, at the firm level on shocks to daily share turnover at the industry- and market-level. Because daily turnover is highly auto-correlated, we create shocks to our turnover measures by regressing daily turnover (at the firm-, industry-, and market-level) on six daily lagged values of turnover. The residuals from these regressions are then used in the volume analog of Equation (1) (Halling et al. 2011).

TABLE 4

Estimation of the Relation between Exogenous Drops in Coverage and Changes in Stock Return Synchronicity and Estimation with Propensity-Score Matched Firms

$$\begin{aligned}\Delta SYNCH_{i,q} = {} & \varphi_0 + \lambda_1 EXOG_{i,q} + \lambda_2 EXOG_{i,q}{*}LNUM_{i,q} + \lambda_3 LNUM_{i,q-1} + \lambda_4 \Delta LMVE_{i,q} \\ & + \lambda_5 \Delta INST_{i,q} + \lambda_6 \Delta TURN_{i,q} + \lambda_7 RET\_12_{i,q} + \lambda_8 ISSUE\_12_{i,q} + \lambda_9 REPORT_{i,t-q} \\ & + \lambda_{10} GUIDE\_6_{i,q} + \lambda_{11} LBM_{i,q-1} + \lambda_{12} STDROA_{i,q} + \lambda_{13} REG_{i,q} + \sum \varphi_i FIRM_i \\ & + \varepsilon_{i,t}.\end{aligned}$$

$$\begin{aligned}\Delta SYNCH_{i,t} = {} & \alpha_0 + \beta_1 INITIATION_{i,t} + \beta_2 INITIATION_{i,t}{*}LNUM_{i,t-1} + \beta_3 LNUM_{i,t-1} \\ & + \beta_4 \Delta LMVE_{i,t} + \beta_5 \Delta INST_{i,t} + \beta_6 \Delta TURN_{i,t} + \beta_7 RET\_12_{i,t} + \beta_8 ISSUE\_12_{i,t} \\ & + \beta_9 REPORT_{i,t-1} + \beta_{10} GUIDE\_6_{i,t} + \beta_{11} LBM_{i,t-1} + \beta_{12} STDROA_{i,t} \\ & + \beta_{13} REG_{i,t} + \sum \alpha_i FIRM_i + \varepsilon_{i,t}.\end{aligned}$$

	Exogenous Drops Analysis			**Propensity Score Analysis**	
Estimation	**(1)**	**(2)**		**(3)**	**(4)**
$EXOG_{i,t}$	0.488	0.527	$INITIATION_{i,t}$	0.406	0.372
	(2.11)***	(2.28)**		(7.02)***	(6.62)**
$EXOG_{i,t} * LNUM_{i,t}$	−0.101	−0.100	$INITIATION * LNUM_{i,t}$	−0.148	−0.140
	(−1.17)	(−0.95)		(−6.25)***	(−5.90)***
$LNUM_{i,t-1}$	−0.0200	−0.039	$LNUM_{i,t-1}$	0.002	0.018
	(−2.74)***	(−4.69)***		(0.15)	(1.27)
$\Delta LMVE_{i,t}$		0.686	$\Delta LMVE_{i,t}$		0.019
		(21.91)***			(0.51)
$\Delta INST_{i,t}$		0.993	$\Delta INST_{i,t}$		1.135
		(10.50)***			(11.07)***
$\Delta TURN_{i,t}$		−0.004	$\Delta TURN_{i,t}$		−0.094
		(−1.33)			(−12.13)***
$RET\_12_{i,t}$		0.151	$RET\_12_{i,t}$		0.198
		(15.33)***			(19.66)***
$ISSUE\_12_{i,t}$		−0.144	$ISSUE\_12_{i,t}$		−0.060
		(−7.23)***			(−2.75)***
$REPORT_{i,t-1}$		0.094	$REPORT_{i,t-1}$		0.019
		(3.36)***			(1.00)
$GUIDE\_6_{i,t}$		−0.007	$GUIDE\_6_{i,t}$		−0.011
		(−0.36)			(−0.59)
$LBM_{i,t-1}$		−0.101	$LBM_{i,t-1}$		−0.137
		(−9.74)***			(−11.08)***
$STDROA_{i,t}$		−0.036	$STDROA_{i,t}$		0.124
		(−0.52)			(3.58)***
$REG_{i,t}$		0.048	$REG_{i,t}$		−0.590
		(1.48)			(2.15)**
Intercept		0.032	Intercept	−0.038	−0.278
		(2.05)**		(−1.19)	(7.83)***
R^2	0.000	0.007	R^2	0.001	0.011
n	204,731	204,731	n	119,565	119,565

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*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

All variables are defined in Appendix A. t-statistics (in parentheses) are based on standard errors that are robust to heteroscedasticity and correlation across observations from the same two-digit SIC industry.

Alternative Measures of Information

Because synchronicity is not a perfect measure of the mix of information available about a specific firm, we use several alternative measures of the available information about a firm to assess the robustness of our main findings.

Analyst Forecast Errors

Earnings forecasts are an important channel through which analysts disseminate news and, thus, serve as an alternative measure of the type of information that analysts provide. In this section, we extend the analyses in Ayers and Freeman (1997) and Piotroski and Roulstone (2004) by examining the link between coverage initiations and the accuracy of analysts' forecasts. We conjecture that newly initiating analysts are more likely to incorporate industry-specific news than firm-specific news into their earnings forecasts and that the opposite holds for subsequent analysts. Thus, using earnings innovations as a proxy for news, we predict that the relation between industry-specific earnings innovations and consensus forecast errors is weaker following new initiations, while the relation between firm-specific earnings innovations and consensus forecast errors is weaker following subsequent initiations.

To investigate the relation between initiations and forecast errors, we measure seasonally adjusted earnings innovations, $\Delta E_{i,q}$, for each firm i and quarter q, where we scale innovations by the beginning of quarter price, $\Delta E_{i,q} = \frac{E_{i,q}-E_{i,q-4}}{P_{i,q}}$, where $E_{i.q}$ equals firm i's earnings per share for fiscal quarter q and $P_{i,q}$ denotes the beginning of quarter price. We next decompose $\Delta E_{i,q}$ into industry- and firm-specific components following Ayers and Freeman (1997). Specifically, we define the industry component of the current earnings innovation as the median earnings innovation of all firms in the same two-digit SIC code as firm i, denoted by $\Delta I_{i,q}$, minus the market earnings innovation, defined as the median value of $\Delta I_{i,q}$ for all industries in quarter q. Finally, we calculate the firm-specific earnings innovation, $\Delta F_{i,q}$, by subtracting $\Delta I_{i,q}$ from the total earnings innovation, $\Delta E_{i,q}$.

For each firm-quarter, we also calculate the consensus analyst forecast error, $FE_{i,q}$, defined as firm i's actual earnings minus the consensus forecast immediately prior to the quarterly earnings announcement, scaled by beginning-of-quarter price. Positive (negative) values of $FE_{i,q}$ correspond to cases where analysts are pessimistic (optimistic) relative to actual earnings. To complement our synchronicity-based tests, we examine the relation between the consensus analyst forecast error, $FE_{i,q}$, and the industry and firm-specific components of the earnings innovation.

We predict that new initiations increase the extent to which industry earnings news is reflected in consensus forecasts, while subsequent initiations increase the extent to which firm-specific earnings news is reflected in consensus forecasts. To test these predictions, we estimate the following regression:

$$\begin{aligned} FE_{i,q} = {} & \alpha_0 + \beta_1 \Delta F_{i,q} + \beta_2 \Delta F_{i,q} * INITIATION\_NEW_{i,q} + \beta_3 \Delta F_{i,q} * INITIATION\_SUB_{i,q} \\ & + \beta_4 \Delta I_{i,q} + \beta_5 \Delta I_{i,q} * INITIATION\_NEW_{i,q} + \beta_6 \Delta I_{i,q} * INITIATION\_SUB_{i,q} \\ & + \beta_7 INITIATION\_NEW_{i,q} + \beta_8 INITIATION\_SUB_{i,q} + \beta_9 LMVE_{i,q} + \beta_{10} LBM_{i,q} + \varepsilon_{i,q}, \end{aligned} \tag{5}$$

where $INITIATION\_NEW_{i,q}$ is a dummy variable that equals 1 when the firm experiences a new initiation in the 60 trading days prior to firm i's quarter q earnings announcement and we define $INITIATION\_SUB_{i,q}$ analogously for subsequent initiations.[21] If newly initiating analysts primarily provide market- and industry-information, then we expect to observe a negative coefficient on the interaction between $\Delta I_{i,q}$ and $INITIATION\_NEW_{i,q}$, consistent with new analysts reducing consensus forecast errors by incorporating the effects of industry earnings innovations into their forecasts. Similarly, if subsequent analysts primarily provide firm-specific information, then we expect to observe a negative coefficient on the interaction between $\Delta F_{i,q}$ and $INITIATION\_SUB_{i,q}$, consistent with subsequent analysts reducing consensus forecast errors by incorporating the effects of firm-specific earnings innovations into their forecasts.

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[21] When $INITIATION\_NEW_{i,q}$ equals 1, the consensus forecast will generally consist of only the forecast from the initiating analyst, although there can be additional forecasts if there are subsequent initiations following the new initiation. In untabulated results, we find qualitatively identical results when $INITIATION\_NEW_{i,q}$ equals 1 and the new initiation is the only forecast or the consensus forecast consists of the new initiation and one or more subsequent initiations.

Analyst forecast errors and earnings innovations are obtained from I/B/E/S and Compustat, respectively. We eliminate firms whose stock price is less than five dollars to avoid scaling issues when measuring analyst forecast errors. The final sample consists of 116,699 firm-quarters spanning 1996–2007. All non-binary variables are standardized at the quarterly level to have a mean of 0 and a standard deviation of 1.

Table 5 presents the results from estimating Equation (5). The results demonstrate that analyst forecast errors are positively related to both contemporaneous innovations in industry and firm-specific earnings innovations, $\Delta I_{i,q}$ and $\Delta F_{i,q}$, respectively. All else equal, greater increases (decreases) in firm or industry-level earnings lead to analysts being overly pessimistic (optimistic) relative to realized earnings. In addition, the negative interaction term between $\Delta F_{i,q}$ and $INITIATION\_SUB_{i,q}$ demonstrates that subsequent initiations attenuate the relation between firm-specific earnings innovations and analyst forecast errors. All else equal, when an analyst initiates coverage on a firm already followed by other analysts, firm-specific earnings news is more likely to be reflected in the consensus forecast. In contrast, the interaction term between $\Delta F_{i,q}$ and $INITIATION\_NEW_{i,q}$ is positive but insignificant. These findings suggest that while subsequent initiations contribute to the incorporation of firm-specific earnings innovations into earnings forecasts, the same is not true of new initiations. The results also demonstrate a negative interaction effect between industry earnings innovations, $\Delta I_{i,q}$, and $INITIATION\_NEW_{i,q}$. The fact that we find a negative and significant interaction effect between industry earnings innovations and $INITIATION\_NEW_{i,q}$, while finding a positive and insignificant interaction effect between firm-specific earnings news and $INITIATION\_NEW_{i,q}$, is consistent with analysts primarily providing industry-earnings news when initiating coverage on firms without existing coverage.

We further document a negative interaction effect between industry earnings innovations and $INITIATION\_SUB_{i,q}$, consistent with subsequent initiations also contributing to the incorporation of industry-specific news into earnings forecasts. An F-test rejects the equality of the $INITIATION\_SUB_{i,q}$ interaction terms, consistent with subsequent initiations playing a larger role in the assimilation of firm-specific earnings news into the consensus forecast relative to the assimilation of industry-specific earnings news. Column (4) of Table 5 demonstrates that these relations are robust to including interaction effects between the components of the earnings innovations and firm size and book-to-market.

Taken together, the results of this section corroborate and extend the main analyses. The results provide additional evidence that the type of information provided by analysts varies depending on the existing level of coverage. Specifically, we show that new initiations are more likely to incorporate industry-earnings innovations into the consensus forecast, while subsequent initiations are more likely to incorporate firm-specific earnings innovations into the consensus forecast. Additionally, by documenting how the type of coverage initiation affects the type of information reflected in analysts' forecasts, the results highlight one of the potential mechanisms through which analysts affect the mix of industry- versus firm-specific information reflected in security prices.

TABLE 5

Estimation of the Relation between Analyst Forecast Errors, Firm and Industry Earnings, and Analyst Initiation

$$\begin{aligned} FE_{i,q} = {} & \alpha_0 + \beta_1 \Delta F_{i,q} + \beta_2 \Delta F_{i,q} * INITIATION\_NEW_{i,q} + \beta_3 \Delta F_{i,q} * INITIATION\_SUB_{i,q} \\ & + \beta_4 \Delta I_{i,q} + \beta_5 \Delta I_{i,q} * INITIATION\_NEW_{i,q} + \beta_6 \Delta I_{i,q} * INITIATION\_SUB_{i,q} \\ & + \beta_7 INITIATION\_NEW_{i,q} + \beta_8 INITIATION\_SUB_{i,q} + \beta_9 LMVE_{i,q} + \beta_{10} LBM_{i,q} + \varepsilon_{i,q}. \end{aligned}$$

Estimation	(1)	(2)	(3)	(4)
$\Delta F_{i,q}$	0.290	0.308	0.308	0.250
	(45.23)***	(45.23)***	(43.07)***	(37.68)***
$\Delta F_{i,q} * INITIATION\_NEW_{i,q}$	0.052		0.041	−0.018
	(0.77)		(0.61)	(−0.28)
$\Delta F_{i,q} * INITIATION\_SUB_{i,q}$		−0.127	−0.126	−0.087
		(−8.67)***	(−8.65)***	(−5.87)***
$\Delta I_{i,q}$	0.052	0.055	0.056	0.055
	(16.29)***	(15.02)***	(15.15)***	(14.73)***
$\Delta I_{i,q} * INITIATION\_NEW_{i,q}$	−0.094		−0.095	−0.116
	(−1.78)*		(−1.81)*	(−2.19)**
$\Delta I_{i,q} * INITIATION\_SUB_{i,q}$		−0.021	−0.021	−0.014
		(−3.19)***	(−3.26)***	(−2.17)**
$INITIATION\_NEW_{i,q}$	0.125	0.121	0.124	0.117
	(2.57)**	(2.61)**	(2.56)**	(2.41)**
$INITIATION\_SUB_{i,q}$	0.058	0.061	0.061	0.059
	(10.07)***	(10.50)***	(10.50)***	(10.11)***
$LMVE_{i,q}$	0.079	0.078	0.078	0.077
	(25.68)***	(25.47)***	(25.47)***	(25.44)***
$LBM_{i,q}$	−0.015	−0.015	−0.015	−0.015
	(−3.28)***	(−3.30)***	(−3.30)***	(−3.22)***
$\Delta F_{i,q} * LMVE_{i,q}$				−0.096
				(13.73)***
$\Delta F_{i,q} * LBM_{i,q}$				0.026
				(5.04)***
$\Delta I_{i,q} * LMVE_{i,q}$				−0.022
				(−6.04)***
$\Delta I_{i,q} * LBM_{i,q}$				0.000
				(0.02)
Intercept	−0.010	−0.010	−0.010	−0.006
	(−3.23)***	(−3.21)***	(−3.21)***	(−1.99)**
R^2	0.098	0.100	0.100	0.100
n	116,699	116,699	116,699	116,699

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

All variables are defined in Appendix A. t-statistics (in parentheses) are based on standard errors that are robust to heteroscedasticity and correlation across observations from the same two-digit SIC industry.

Other Measures of Information

In addition to examining analyst forecast errors, we use the market and industry betas from variations of Equation (1) and return kurtosis, measured over the same time period as return synchronicity, to assess how initiations affect the information available about a firm.

Market and industry betas should capture how a firm's returns move with changes in the market and industry returns. We regress firm returns on market returns ($MKT\_BETA_{i,t}$) and firm returns on industry returns ($IND\_BETA_{i,t}$) to obtain the betas and then replace changes in synchronicity around initiations with changes in these betas around initiations. Table 6 contains the results of estimating Equation (4) where the dependent variable equals the change in betas. The estimated coefficients on $INITIATION\_NEW_{i,t}$ ($INITIATION\_SUB_{i,t}$) are positive (negative) and significant when $\Delta MKT\_BETA_{i,t}$ or $\Delta IND\_BETA_{i,t}$ is the dependent variable.[22] These results are consistent with our primary findings.[23]

Roll (1988) documents that the kurtosis of the return distribution declines significantly when he eliminates daily observations associated with firm-specific news, suggesting that a change in kurtosis may be one way to measure the presence of firm-specific information. Accordingly, we replace the change in synchronicity with the change in kurtosis and re-estimate Equation (4). The results, presented in Table 7, show that subsequent initiations lead to an increase in kurtosis while new initiations have no appreciable effect on kurtosis. Decreases in coverage result in a decrease in kurtosis. Interpreting these results in the light of Roll (1988) suggests that subsequent initiations increase the amount of firm-specific information being impounded in price, while decreases in coverage have the opposite effect.

Other Robustness Checks

We now summarize several other untabulated robustness checks that do not alter the main inferences of our study. First, because analysts may simply choose to cover firms that receive more press coverage, and because press coverage may actually increase the amount of firm-specific news available (instead of analysts increasing a firm's exposure to the press), we include the change in media coverage as a control variable.[24] Second, we replace adjusted R^2 values in the calculation of synchronicity with raw R^2 values to ensure that our modifications to the adjusted R^2 measure do not affect our results. Third, we include fixed effects defined by industry and month instead of firm fixed effects. Fourth, we include a time trend in the regressions. Fifth, we use the Prais-Winsten procedure to correct for autocorrelation in the regression error terms reported in Table 3. Finally, we

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[22] For completeness we also include $DROP_{i,t}$ and $DROP\_ZERO_{i,t}$ in the regressions. We document that when there is a drop in coverage in a given month the market and industry betas increase, which is consistent with the decrease in betas when there is a subsequent initiation. In untabulated results, we also include our volume synchronicity measure as a control variable. The coefficient on $INITIATION\_NEW_{i,t}$ ($INITIATION\_SUB_{i,t}$) remains positive (negative) and significant.

[23] Note that betas and R^2s can change independently of each other. They are only directly related under strict distributional assumptions regarding the variances of the dependent and independent variables. Given this, we have estimated the relation between changes in synchronicity and initiations while controlling for changes in betas. Results (untabulated) indicate that new (subsequent) initiations continue to have a positive (negative) relation with the change in synchronicity. Given that the R^2 of a regression is determined by the regression betas, the variance and covariance of the independent variables, and the variance of the idiosyncratic portion of the dependent variable, and assuming that initiations do not affect the variance and covariance of market and industry returns, our findings suggest that initiations affect a firm's stock return synchronicity through changes in the weight placed on market and industry returns (i.e., changes in betas) as well as changes in the idiosyncratic portion of the firm's returns.

[24] Eugene Soltes of Harvard Business School graciously provided data on media coverage (Soltes 2010).

TABLE 6

Estimation of the Relation between Changes in Betas and Analyst Initiations

$$\begin{aligned}\Delta BETA_{i,t} = {} & \rho_0 + \gamma_1 INITIATION\_NEW_{i,t} + \gamma_2 INITIATION\_SUB_{i,t} + \gamma_3 DROP_{i,t-1} \\ & + \gamma_4 DROP\_ZERO_{i,t-1} + \gamma_5 \Delta LMVE_{i,t} + \gamma_6 \Delta INST_{i,t} + \gamma_7 \Delta TURN_{i,t} + \gamma_8 RET\_12_{i,t} \\ & + \gamma_9 ISSUE\_12_{i,t} + \gamma_{10} REPORT_{i,t-1} + \gamma_{11} GUIDE\_6_{i,t} + \gamma_{12} LBM_{i,t-1} \\ & + \gamma_{13} STDROA_{i,t} + \gamma_{14} REG_{i,t} + \sum \rho_i FIRM_i + \varepsilon_{i,t}.\end{aligned}$$

Estimation	$\Delta MKT\_BETA_{i,t}$	$\Delta MKT\_BETA_{i,t}$	$\Delta IND\_BETA_{i,t}$	$\Delta IND\_BETA_{i,t}$
$INITIATION\_NEW_{i,t}$	0.0635	0.0640	0.0526	0.0526
	(3.43)***	(3.43)***	(4.58)***	(4.55)***
$INITIATION\_SUB_{i,t}$	−0.0124	−0.0111	−0.0134	−0.0130
	(−2.22)**	(−2.02)**	(−4.30)***	(−4.13)***
$DROP_{i,t}$		0.0170		0.0067
		(2.93)***		(1.87)**
$DROP\_ZERO_{i,t}$		0.0025		−0.0260
		(0.10)		(−1.07)
$\Delta LMVE_{i,t}$	0.1167	0.1169	0.0549	0.0550
	(5.63)***	(5.62)***	(4.71)***	(4.72)***
$\Delta INST_{i,t}$	0.1637	0.1641	0.1386	0.1388
	(4.84)***	(4.83)***	(6.25)***	(6.26)***
$\Delta TURN_{i,t}$	0.0118	0.0119	0.0111	0.0111
	(4.67)***	(4.68)***	(5.38)***	(5.39)***
$RET\_12_{i,t}$	0.0461	0.0462	0.0287	0.0287
	(8.94)***	(8.94)***	(9.40)***	(9.41)***
$ISSUE\_12_{i,t}$	−0.0464	−0.0465	−0.0199	−0.0199
	(−4.50)***	(−4.50)***	(−4.76)***	(−4.76)***
$REPORT_{i,t-1}$	0.0058	0.0057	0.0055	0.0055
	(4.14)***	(4.08)***	(3.69)***	(3.67)***
$GUIDE\_6_{i,t}$	0.0076	0.0072	−0.0019	−0.0020
	(2.53)**	(2.38)**	(−0.80)	(−0.87)
$LBM_{i,t-1}$	−0.0399	−0.0401	−0.0259	−0.0260
	(−8.10)***	(−8.00)***	(−11.48)***	(−11.51)***
$STDROA_{i,t}$	−0.0009	−0.0010	0.0035	0.0035
	(−0.10)	(−0.11)	(0.60)	(0.59)
$REG_{i,t}$	0.0442	0.0440	0.0401	0.0400
	(2.00)**	(2.00)**	(1.78)*	(1.78)*
Intercept	−0.0299	−0.0313	−0.0171	−0.0175
	(−8.62)***	(−8.47)***	(−7.81)***	(−8.06)***
R^2	0.0208	0.0208	0.0181	0.0181
n	613,111	613,111	613,111	613,111

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*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

All variables are defined in Appendix A. t-statistics (in parentheses) are based on standard errors that are robust to heteroscedasticity and correlation across observations from the same two-digit SIC industry.

TABLE 7

Estimation of the Relation between Changes in Kurtosis and Analyst Initiations

$$\begin{aligned}\Delta KURTOSIS_{i,t} = {} & \alpha_0 + \beta_1 INITIATION_{i,t} + \beta_2 INITIATION_{i,t}{*}LNUM_{i,t-1} + \beta_3 LNUM_{i,t-1} \\ & + \beta_4 \Delta LMVE_{i,t} + \beta_5 \Delta INST_{i,t} + \beta_6 \Delta TURN_{i,t} + \beta_7 RET\_12_{i,t} + \beta_8 ISSUE\_12_{i,t} \\ & + \beta_9 REPORT_{i,t-1} + \beta_{10} GUIDE\_6_{i,t} + \beta_{11} LBM_{i,t-1} + \beta_{12} STDROA_{i,t} \\ & + \beta_{13} REG_{i,t} + \sum \alpha_i FIRM_i + \varepsilon_{i,t}.\end{aligned}$$

$$\begin{aligned}\Delta KURTOSIS_{i,t} = {} & \rho_0 + \gamma_1 INITIATION\_NEW_{i,t} + \gamma_2 INITIATION\_SUB_{i,t} + \gamma_3 DROP_{i,t-1} \\ & + \gamma_4 DROP\_ZERO_{i,t-1} + \gamma_5 \Delta LMVE_{i,t} + \gamma_6 \Delta INST_{i,t} + \gamma_7 \Delta TURN_{i,t} \\ & + \gamma_8 RET\_12_{i,t} + \gamma_9 ISSUE\_12_{i,t} + \gamma_{10} REPORT_{i,t-1} + \gamma_{11} GUIDE\_6_{i,t} \\ & + \gamma_{12} LBM_{i,t-1} + \gamma_{13} STDROA_{i,t} + \gamma_{14} REG_{i,t} + \sum \rho_i FIRM_i + \varepsilon_{i,t}.\end{aligned}$$

Estimation	(1)		(2)	(3)
$INITIATION_{i,t}$	−0.0350 (−1.28)	$INITIATION\_NEW_{i,t}$	−0.0482 (−1.05)	−0.0495 (−1.09)
$INITIATION * LNUM_{i,t}$	0.0386 (2.37)**	$INITIATION\_SUB_{i,t}$	0.0429 (3.33)***	0.0402 (3.13)***
$LNUM_{i,t-1}$	0.0641 (10.11)***	$DROP_{i,t}$		−0.0363 (−3.43)***
		$DROP\_ZERO_{i,t}$		−0.0288 (−0.62)
$\Delta LMVE_{i,t}$	−0.0844 (−3.49)***	$\Delta LMVE_{i,t}$	−0.0941 (−3.91)***	−0.0944 (−3.93)***
$\Delta INST_{i,t}$	−0.0895 (−1.60)***	$\Delta INST_{i,t}$	−0.1025 (−1.85)*	−0.1033 (−1.87)*
$\Delta TURN_{i,t}$	0.0588 (7.56)***	$\Delta TURN_{i,t}$	0.0586 (7.51)***	0.0585 (7.51)***
$RET\_12_{i,t}$	−0.0205 (−3.76)***	$RET\_12_{i,t}$	−0.0237 (−4.44)***	−0.0239 (−4.47)***
$ISSUE\_12_{i,t}$	−0.0290 (−4.27)***	$ISSUE\_12_{i,t}$	−0.0269 (−3.75)***	−0.0269 (−3.72)***
$REPORT_{i,t-1}$	−0.0316 (−6.76)***	$REPORT_{i,t-1}$	−0.0315 (−6.68)***	−0.0314 (−6.61)***
$GUIDE\_6_{i,t}$	−0.1590 (−9.78)***	$GUIDE\_6_{i,t}$	−0.1466 (−9.69)***	−0.1459 (−9.67)***
$LBM_{i,t-1}$	0.0327 (3.59)***	$LBM_{i,t-1}$	0.0288 (3.13)***	0.0293 (3.17)***
$STDROA_{i,t}$	−0.0609 (−4.76)***	$STDROA_{i,t}$	−0.0635 (−4.19)***	−0.0634 (−4.21)***
$REG_{i,t}$	0.0159 (0.38)	$REG_{i,t}$	0.0064 (0.14)	0.0070 (0.16)
Intercept	0.0337 (3.58)***	Intercept	0.1047 (9.79)***	0.1079 (10.28)***
R^2	0.0135	R^2	0.0134	0.0134
n	608,548	n	608,548	608,548

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

All variables are defined in Appendix A. t-statistics (in parentheses) are based on standard errors that are robust to heteroscedasticity and correlation across observations from the same two-digit SIC industry.

include the fourth lag of the level of synchronicity in our regressions as firms with higher levels of synchronicity may experience higher or lower shifts in synchronicity.

VI. CONCLUSION

In this study, we examine how analyst initiations affect synchronicity in order to determine whether analysts provide market- and industry- or firm-specific information about the firms they cover. We show that initiations increase synchronicity among firms with no existing analyst coverage and interpret this result as evidence that the first analyst report for a firm predominantly provides industry and market information. In contrast, we find that coverage initiations of firms with existing coverage appear to decrease synchronicity. Thus, if other analysts are following the firm, then analysts who initiate coverage appear to provide firm-specific information. Our results contribute to the literature by suggesting that the information analysts provide relies critically on the presence of other analysts. Furthermore, we extend prior studies that examine the relation between returns and analyst initiations by also showing that initiations are important information events.

We solidify our findings by documenting that they are robust to alternative measures of the mix of information available about a firm. For example, we disaggregate earnings news into industry- and firm-specific components and examine each component's relation with consensus forecast errors. Consistent with our synchronicity-based tests, we show that new initiations reduce the sensitivity of consensus forecast errors to industry earnings innovations, while subsequent initiations are more likely to mitigate the relation between consensus forecast errors and firm-specific earnings innovations. These findings document how the type of coverage initiation affects the type of information reflected in analysts' forecasts, thus highlighting a potential channel through which analysts affect the mix of industry- versus firm-specific information reflected in security prices. We also show that our results are robust to efforts to control for the endogenous nature of coverage decisions.

While our results are important for understanding the type of information that analysts produce, we do not take a comprehensive approach to identifying the mechanisms through which analysts communicate this information to investors. For example, in addition to their earnings forecasts, do analysts convey specific components of earnings news through their written assessment of market and industry conditions and the firm's operations? Additionally, do investors learn about industry and market information by observing analysts' coverage decisions? We leave these questions to future research.

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APPENDIX A

VARIABLE DEFINITIONS

Variable		Definition
Synchronicity Variables		
R^2	=	coefficient of determination from the firm-month estimation of the model: $RET_{i,t} = \alpha + \beta_1 MKRET_{i,t} + \beta_2 INDRET_{i,t} + \varepsilon_{i,t}$, where *MKRET* is the value-weighted market return and *INDRET* is the two-digit SIC industry value-weighted return for day t (not including the return of firm i). The model is estimated using daily returns from the past three months for each firm-month with a minimum of 50 daily observations;
$SYNCH_{i,t}$	=	logarithmic transformation of R^2, defined as log $(R^2/(1-R^2))$;
$\Delta SYNCH_{i,t}$	=	change in synchronicity defined as $SYNCH_{i,t+3} - SYNCH_{i,t-1}$;
$MKT\_BETA_{i,t-1}$	=	β_1 from the firm-month estimation of the model: $RET_{i,t} = \alpha + \beta_1 MKRET_{i,t} + \varepsilon_{i,t}$, where *MKRET* is the value-weighted market return. The model is estimated using daily returns from the past three months for each firm-month with a minimum of 50 daily observations;
$\Delta MKT\_BETA_{i,t}$	=	change in the market beta defined as $MKT\_BETA_{i,t+3} - MKT\_BETA_{i,t-1}$;
$IND\_BETA_{i,t-1}$	=	β_1 from the firm-month estimation of the model: $RET_{i,t} = \alpha + \beta_1 INDRET_{i,t} + \varepsilon_{i,t}$, where *INDRET* is the two-digit SIC industry value-weighted return for day t (not including the return of firm i). The model is estimated using daily returns from the past three months for each firm-month with a minimum of 50 daily observations;
$\Delta IND\_BETA_{i,t}$	=	change in the industry beta defined as $IND\_BETA_{i,t+3} - IND\_BETA_{i,t-1}$;
$KURTOSIS_{i,t-1}$	=	average of three monthly values of kurtosis based on a firm's daily returns, i.e., this is the average of kurtosis measured in months $t-1$, $t-2$, and $t-3$; and
$\Delta KURTOSIS_{i,t}$	=	change in kurtosis defined as $KURTOSIS_{i,t+3} - KURTOSIS_{i,t-1}$.
Analyst Coverage Variables		
$INITIATION_{i,t}$	=	indicator variable set to 1 if an analyst initiates coverage on a stock in month t, 0 otherwise;
$INITIATION\_NEW_{i,t}$	=	indicator variable set to 1 if an analyst initiates coverage on a stock with no existing coverage in month t, 0 otherwise;
$INITIATION\_SUB_{i,t}$	=	indicator variable set to 1 if an analyst initiates coverage on a stock with prior coverage in month t, 0 otherwise;
$DROP_{i,t}$	=	indicator variable set to 1 if the level of analyst coverage declines from month $t-1$ to t;
$DROP\_ZERO_{i,t}$	=	indicator variable set to 1 if analyst coverage in month t is 0 and non-0 in month $t-1$;
$EXOG_{i,q}$	=	indicator variable set to 1 if there is an exogenous reduction in coverage in quarter q;
$NUM_{i,t-1}$	=	number of analysts covering firm i in month $t-1$; and
$LNUM_{i,t-1}$	=	natural log of 1 plus the number of analysts covering firm i in month t.
Other Variables		
$LMVE_{i,t}$	=	natural log of market value of equity (in millions) in month t calculated as price times the number of shares outstanding;
$\Delta LMVE_{i,t}$	=	change in the natural log of *MVE* defined as $LMVE_{t+1} - LMVE_{t-1}$;
$INST_{i,t}$	=	number of shares in firm i held by institutions in month t divided by the total number of shares outstanding. We obtain data on institutional holdings from the CDA/Spectrum database;

(continued on next page)

APPENDIX A (continued)

Variable		Definition
$\Delta INST_{i,t}$	=	average value of $INST_{i,t}$ in months $t+1$ to $t+6$ less the average value of $INST_{i,t}$ in months $t-1$ to $t-6$;
$VOL_{i,t-1}$	=	log of the average trading volume in the six months prior to month t, $VOL_{i,t-1}$;
$TURN_{i,t}$	=	number of shares traded in month t divided by total shares outstanding;
$\Delta TURN_{i,t}$	=	average value of $VOLUME_{i,t}$ in months $t+1$ to $t+6$ less the average value of $VOLUME_{i,t}$ in months $t-1$ to $t-6$ scaled by the total number of shares outstanding in month t;
$RET\_12_{i,t}$	=	market-adjusted return on the firm's stock from $t-12$ to $t-1$;
$ISSUE\_12_{i,t}$	=	indicator variable set equal to 1 if the firm issued equity in the past 12 months, 0 otherwise;
$REPORT_{i,t-1}$	=	indicator variable set equal to 1 if the firm reported earnings in month $t-1$, 0 otherwise;
$GUIDE\_6_{i,t}$	=	indicator variable set equal to 1 if the firm issued earnings guidance within the last six months, 0 otherwise;
$BM_{i,t-1}$	=	book-to-market ratio in month $t-1$ defined as book value divided by MVE;
$LBM_{i,t-1}$	=	natural log of the book-to-market ratio in month $t-1$ defined as book value divided by MVE;
REG_i	=	indicator variable set equal to 1 if the firm belongs to a regulated industry, namely financial services or utilities;
$STDROA_{i,t}$	=	standard deviation of return on assets (ROA) measured over current and previous four quarters;
$\Delta E_{i,q}$	=	seasonally adjusted earnings innovation, scaled by price: $\Delta E_{i,q} = \frac{E_{i,q} - E_{i,q-4}}{P_{i,q}}$, where $E_{i,q}$ equals firm i's earnings per share for quarter q and $P_{i,q}$ denotes the beginning of quarter price;
$\Delta I_{i,q}$	=	industry component of the current earnings innovation defined as $\Delta IE_{i,q} - \Delta ME_{i,q}$, where $\Delta IE_{i,q}$ is the median earnings innovation of all firms in the same two-digit SIC code as firm i and $\Delta ME_{i,q}$ is the median value of $\Delta IE_{i,q}$ for all industries in quarter q;
$\Delta F_{i,q}$	=	firm-specific earnings innovation, $\Delta F_{i,q}$ defined as $\Delta E_{i,q} - \Delta IE_{i,q}$; and
$FE_{i,q}$	=	consensus forecast error defined as firm i's actual quarter q earnings minus the consensus forecast immediately prior to the quarterly earnings announcement, scaled by beginning of quarter price.

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Influence Activities and Favoritism in Subjective Performance Evaluation: Evidence from Chinese State-Owned Enterprises

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ABSTRACT: This study addresses the two-way process in which a subordinate and a superior engage in influence activities (bottom-up) and favoritism (top-down) in subjective Performance Evaluation. The research context is the Chinese government's evaluation of Chinese state-owned enterprises (SOEs) by the State-Owned Assets Supervision and Administration Commission of China (SASAC). We analyze archival records of the government's evaluation scores, score adjustments, and evaluation ratings given to 63 SOEs between 2005 and 2007. These analyses are also interpreted based on insights gained from in-depth field interviews with SASAC officials and chief financial officers (CFOs) of SOEs. Results indicate that the political connection of SOE CFOs, the geographic proximity of SOE headquarters to the SASAC central office, and political rank of the firm affect the SASAC's evaluations.

Keywords: *subjective Performance Evaluation; favoritism; influence activities; political connections; geographic proximity; State-Owned Asset Supervision and Administration Council (SASAC); Chinese State-Owned Enterprises (SOEs).*

Data Availability: *Data used in this study cannot be made public due to a confidentiality agreement.*

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I. INTRODUCTION

The purpose of Subjective Performance Evaluation (SPE) is to allow a superior to include information relevant to subordinate's work performance that is not incorporated in objective performance measures (Holmstrom 1979; Prendergast and Topel 1996). Studies by Gibbs et al. (2004) and Nisar (2007) suggest that including subjectivity in Performance Evaluation increases subordinate pay satisfaction, sales productivity, and trust between superiors and subordinates. While SPE allows a superior to include more information in subordinate Performance Evaluation, it also increases the likelihood of influence activities, since it provides more opportunities for subordinates to affect superior evaluation decisions and for superiors to make discretionary judgments by playing favorites (Gibbs et al. 2004).

Empirical studies in this area have emphasized the decision-influencing behavior of subordinates' activities, while focusing less on the role of favoritism on the part of the superiors.[1] This study empirically assesses the two-way relationship in a subjective Performance Evaluation process by determining how subordinates attempt to influence their evaluations and how superiors engage in favoritism with particular subordinates (Prendergast and Topel 1996).

Our research setting is the Chinese government's evaluations of 63 state-owned enterprises (SOEs—the subordinates) by the State-Owned Assets Supervision and Administration Commission of the State Council of China (SASAC—the superior). The study draws on the archival records of the Chinese government's evaluation scores, score adjustments, and evaluation ratings of 63 SOEs between 2005 and 2007, as well as in-depth field interviews with chief financial officers (CFOs) of 6 SOEs. We identify the factors that influence governmental evaluations of SOEs in addition to the financial performance indicators that the SASAC claims to use when evaluating SOEs. To address this research question, we use a database prepared by the SASAC that contains several levels of our dependent variables—the scores, ratings, the assignment of cutoff scores, and the *ex post* adjustments in scores/ratings.

Chinese SOEs play a critical role in the Chinese and world economies. At the end of 2007, the SASAC list of supervised firms contained 152 of the largest firms owned by the Chinese central government, whose asset size totaled 14.9 trillion RMB (US$2.1 trillion), and whose annual earnings totaled 1 trillion RMB (US$140 billion), accounting for 4 percent of China's annual gross domestic product (GDP). As of December 2007, 16 Chinese central government controlled SOEs were on the *Fortune* 500 list. By the end of 2011, this number has increased to 38. Each year the SASAC evaluates the performance of each central government controlled SOE in China, giving it a score based on three metrics: (1) the levels of budgetary targets on key financial indicators; (2) actual performance on the same set of key financial indicators; and (3) the degree of target difficulty as predetermined by the SASAC. Of relevance to our study is that the SASAC engages in *ex post* subjective adjustments of evaluation scores on a case-by-case basis. The SASAC makes personnel and compensation decisions based on these final evaluation scores and ratings.

Evidence from our field interviews illustrates the two-way process in which subordinates (using a bottom-up approach) and superiors (using a top-down approach) can affect subjective Performance Evaluation. From the SOE (subordinate) perspective, Performance Evaluation influences SOE executives' compensation and their political advancement, and executives are motivated to maximize performance on measures in the official evaluation formula. When subjectivity is included in the appraisal process, SOE executives have the opportunity to engage in influence activities with superiors by privately communicating information to the SASAC, engaging in joint social activities with SASAC officials, and attempting to manipulate the SASAC's perceptions of their performance in order to appear more competent (Milgrom and Roberts 1992). We assess the level of influence activities on the part of SOEs by looking at the effects of two factors on the SASAC's evaluations—the political connections of top executives of the SOEs and the geographic proximity between SOE headquarters and the central SASAC office. Further, we study how two other factors related to favoritism affect the SASAC's evaluation scores and ratings of SOEs—the SASAC's determination of whether an SOE is fulfilling strategic government objectives, and the political rank of an SOE.[2]

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[1] Bol et al.'s (2010) study, while not directly related to ours, is the first empirical study to look at how superiors adjust sales targets to promote fairness.

Results suggest that both CFO connections, an executive identity variable that proxies for influence activities, and SOE political rank, a firm identity variable that proxies for government favoritism, affect evaluation scores, ratings, and *ex post* score adjustment. To better understand whether these factors influence how governmental discretion is used to apply favoritism, we further test their role in the target-setting process and cutoff scores assignment. Results indicate that highly ranked SOEs and SOEs that compete in strategic industries obtain fewer points in achieving targets based on common measures, and obtain more points in achieving targets based on firm-specific measures than less highly ranked SOEs or SOEs not in strategic industries. The government assigns cutoff scores to the benefit of high-ranked SOEs, which is consistent with the government playing favorites with a certain set of SOEs.

Our paper proceeds as follows. In Section II we present institutional details relating to the development of the SASAC and how it conducts Performance Evaluations of SOEs. In-depth field interviews conducted with SOE CFOs and high-ranking officials of the SASAC also informed this and the following section. Section III provides the theoretical framework motivating our investigation and presents our research hypotheses. In Section IV we test our hypotheses using archival data and present our results. Section V concludes the paper with a summary, discussion, and suggestions for future research.

II. INSTITUTIONAL BACKGROUND

The state-owned enterprise (SOE) system in China has undergone enormous changes in the last three decades. SOEs were initially part of the Chinese government, but reforms gave them increasing autonomy so that, by the early 1990s, many SOEs had become independent production and management entities. The State-Owned Assets Supervision and Administration Commission (SASAC) was created in November 2002, and charged with establishing a "new state asset management system in which authority, duty, and responsibilities are united, and in which management of assets, personnel, and affairs is unified" (SASAC 2006). The SASAC represents the scaling back of government authority through the creation of an arm's-length regulatory body, and is designed to be a powerful and authoritative board of directors. The roles of the SASAC include monitoring enterprise operations to protect the rights of the government owner; dispatching supervisors to audit and monitor the enterprise; appointing members of boards of directors and establishing procedures for appointing managers; approving major decisions in enterprise operations, including mergers, bankruptcies, and the issuance of new securities; and reporting on enterprise performance and revenues to the appropriate level of government. The SASAC also issues guidelines to direct strategic pathways for the SOEs.

To understand the evaluation timeline and procedures better, we conducted in-depth interviews with two high-ranking officials from the Performance Evaluation bureau of the SASAC and with

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[2] In this research setting, the government evaluator (the SASAC) is the sole decision maker that decides which industries are strategic industries, and assigns political rank to these SOEs. An SOE is not able to influence its own industry or its political rank. These two measures allow us to disentangle governmental favoritism from influence activities.

CFOs of six central government controlled SOEs in July 2009. We use insights gained from these interviews to increase our knowledge of the institutional aspects of the paper and to clarify particular findings in the results section. The interviews included a broad range of companies in different industries, including equipment manufacturing, military supply, metallurgy, commercial trade, petroleum, and a large investment holding company. We conducted three interviews in person at companies in Beijing and three others via conference calls to companies headquartered outside Beijing. We conducted all interviews in Mandarin with two of the co-authors present. We followed a strict interview protocol asking the same set of open-ended questions in the same order across all six firms and obtained insights into whether SPE, SOE influence activities, or governmental favoritism exist in the Performance Evaluation of SOEs managed by the SASAC. More specifically, we were interested in how different aspects of SPE may have affected the SASAC's Performance Evaluation scores and ratings of SOEs, and the potential factors that may have led to more favorable governmental ratings. Each interview lasted approximately three hours. By the sixth interview we did not learn anything that was significantly different from previous interviews and, thus, terminated the interview process.

Five Stages of the SASAC's Performance Evaluation Process

In this section we describe the five stages that the SASAC goes through to determine an SOE's Performance Evaluation score.

Stage 1: Operational Target Setting

In the fourth quarter of each year, the SASAC begins a Performance Evaluation process for the following year by setting performance targets for SOEs. Target setting is a bottom-up process in which the SOEs propose annual operational targets, and the SASAC either approves or adjusts those targets. Setting an operational target that is either lower than last year's actual performance or lower than the average of the last three years precludes an SOE from getting a top rating in the next annual Performance Evaluation. At the end of March, after the SASAC and SOEs have reached consensus regarding targets, an SASAC representative signs a performance responsibility contract with the so-called "Persons in Charge" of each SOE.

Each SOE sets targets based on four measures as shown in Appendix A. The first two measures are mandatory: earnings before tax and extraordinary items (EBT), and return on equity (ROE). Each SOE must also choose two of the following three other measures: inventory turnover, accounts receivable turnover, or sales growth. Details of the scoring are discussed below.

Stage 2: Operational Results Verification

Before the end of April each year, CFOs of the SOEs prepare annual performance reports, which include a budget variance analysis and an operational performance summary based on audited annual financial statements for the previous year. These mandatory reports are submitted to the SASAC and its supervising committee. The SASAC verifies and evaluates each SOE's performance in relation to its annual operational targets. SOEs may voluntarily disclose additional information to the SASAC when they submit the performance reports. For example, if unfavorable variances occur, they can provide a detailed explanation for the variances.

Stage 3: Raw Score Calculation

The SASAC verifies the operational results of the SOEs and calculates their raw evaluation scores. The raw scores range from a minimum of 72 to a maximum of 120 points. The comprehensive score for each SOE's annual Performance Evaluation is the sum of the points awarded for target achievement on each measure, multiplied by the degree of operational difficulty given that an SOE meets all performance targets. When an SOE achieves a specific target measure, points earned on that measure are multiplied by the degree of difficulty parameter, a parameter > 1 for every SOE as shown in Appendix A.

The degree of operational difficulty is a subjectively determined parameter based on assets, revenue, total profit, return on equity, number of employees, and ratio of retired employees to total employees. This parameter varies across years for different firms. The degree of operational difficulty parameter is assigned to each SOE after all SOE audited financial statements have been submitted to the SASAC. The SASAC then compiles a list that ranks all SOEs based on the current-year assets, revenue, total profit, return on equity, number of employees, and ratio of retired employees to total employees. Using this rank, the SASAC then subjectively assigns the degree of difficulty parameter to each SOE.[3]

Stage 4: **Ex Post** *Discretionary Adjustment*

After calculating the raw scores of all SOEs, the SASAC makes *ex post* discretionary adjustments to each SOE's score based on subjective criteria. The SASAC indicates that they deduct punishment points from raw scores if enterprises have severe safety incidents or have been involved in financial fraud or other scandals. Enterprises that have acquired financially distressed enterprises out of political concerns receive bonus points (SASAC 2008). The SASAC makes *ex post* discretionary adjustments to SOE Performance Evaluation scores on a case-by-case basis, and all such adjustments publicly available online.

Stage 5: Ratings Assignment

The Performance Evaluation Bureau prepares a comprehensive working document that includes all SOE fulfillment or non-fulfillment of targets, degrees of difficulty, raw scores, subjective adjustment scores, and final scores sorted from the highest to the lowest final score. Based on this document, SASAC directors determine cutoff scores with which to assign each SOE to one of five classes: A, B, C, D, and E. A score of "C" or above is considered acceptable and SOE executives in D- and E-ranked firms may be asked to step down from their current positions by SASAC officials.

SOE executives are motivated to influence the perceptions of government superiors and engage in private communication with government superiors because evaluation ratings affect their performance-based bonuses and their career advancement.[4]

The SASAC notifies the executive of each SOE of their confirmed evaluation scores and ratings. An executive can appeal the rating to the SASAC. The SASAC reports the Performance Evaluation results to the State Council of China, and makes bonus and personnel decisions based on Performance Evaluation results. Our field interviews suggest that firms care more about the ratings (A–E) than their scores *per se*. All of the CFOs and SASAC officials we interviewed agreed that

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[3] We were unable to obtain further details regarding the specific formula used by the SASAC to calculate the degree of difficulty parameter of individual SOEs.

[4] Performance-based bonuses are influenced by evaluation rank in a piece-linear manner: SOE executives of A-ranked firms can get bonuses of 2 to 3 times their base salaries; SOE executives of B-ranked firms can get bonuses of 1.5 to 2 times their base salaries; SOE executives of C-ranked firms can get bonuses of 1 to 1.5 times their base salaries; SOE executives of D-ranked firms can get bonuses of 0 to 1 times their base salaries; and SOE executives of E-ranked firms get 0 bonuses. In addition, part of the bonuses of SOE executives of D- and E-ranked firms are deducted as punishment, depending on how large the gaps are between their evaluation scores and the minimum scores of C-ranked firms.

SOE executives carefully monitor the ratings assigned to them by the SASAC, in part because of economic concerns, but primarily because of issues of face and honor.

III. THEORETICAL FRAMEWORK

Subjective Performance Evaluation of State-Owned Enterprises (SOEs)

SOEs differ from their private counterparts because of their roles as business entities and agents of the government. As governmental agents, they are responsible for achieving multiple objectives, including creating jobs, promoting industrialization, advancing technologies, defending national security, and subsidizing underdeveloped areas of the country. Since the SOEs work toward multiple goals, the government uses multiple measures to evaluate both the economic efficiency and the social effectiveness of these SOEs. Some of these measures may be contradictory with one another and may be difficult to quantify. Subjective Performance Evaluation is a mechanism used by the Chinese government and individual politicians to focus SOEs on the goals and objectives important to the government and to individual politicians.

Influence Activities and Tactics Used by SOEs to Influence the SASAC

Influence activities occur when agents attempt to affect a superior's decisions for their own private benefit (Milgrom 1988; Milgrom and Roberts 1988, 1992; Meyer et al. 1992). In organizations where individuals have discretion over decisions that have distributional implications, influence activities inevitably arise (Inderst et al. 2005). Since subjective Performance Evaluation directly affects the compensation and promotion of SOE executives, they are motivated to engage in influence activities in the subjective evaluation process. While the SASAC centralizes authority over SOEs, CFOs have private information regarding their own performance, which exacerbates the potential for costly efforts to influence the SASAC evaluation decisions. The costs of engaging in influence activities include wasted subordinate effort and time, inefficient decision-making, and deadweight losses in firm value (Milgrom 1988; MacLeod 2003).

More specifically, influence activities include direct personal appeals, offers of reciprocity, and the use of rational persuasion and consultation (Ciadini 1984; Yukl et al. 1993). Higgins et al. (2003) suggest that these types of ingratiation activities have positive effects on work outcomes. SOE executives may attempt to manage perceptions regarding their own performance to make themselves look more competent in the eyes of supervisors. They may also be motivated to create a generally good impression with supervisors to gain higher evaluation scores (Milgrom and Roberts 1988). The CFOs we interviewed confirmed that SOEs are likely to actively communicate with the SASAC during the Performance Evaluation process. They suggested that such SOE initiated communication should include "an early start," "a timely follow-up," and "a creative mentality."

Our field interviews suggest that SOE executives frequently use two types of influence tactics when communicating with SASAC officials. The first tactic is persuasion, which is the use of logical arguments and factual evidence to persuade others that their proposals or special requests are viable, reasonable, and may aid long-term sustainable growth of their firms (Yukl et al. 1993). One commercial trade company successfully convinced the SASAC to adjust their operational targets during the evaluation period. The company's CFO believed that the persuasion strategy was very effective:

> A 2 percent Renminbi appreciation can immediately cause exchange losses of 40 million U.S. dollars in our company. By no means could we foresee our central government reforming the Renminbi exchange rate system on July 21, 2005. (We were) considering which would be the most effective way to communicate with the SASAC about our difficult situation and to ask them for special treatment. We decided to hold a seminar; we

> invited all commercial trade companies in Beijing to discuss how Renminbi appreciation would influence exchange losses in 2005, and of course, we invited officials from the SASAC to attend our seminar. Using this strategy, we were able to convey our message clearly to the SASAC and successfully convince them to upwardly adjust our profit number by 31 million U.S. dollars in that evaluation period.

The second tactic used by SOEs is consultation. The SOEs proactively consult with SASAC officials and solicit the participation of the SASAC in planning strategy, activity, or change, showing the SASAC their willingness to modify a proposal in response to concerns and suggestions. One CFO remarked:

> It would be way too late if we waited until operational results are submitted to the SASAC to start communicating with the SASAC. We have to strategically set targets to better fulfill our promise to the SASAC as specified in our performance responsibility contract.

Another CFO explained:

> We call SASAC officials who are assigned to calculate the score for our firm once a week during the evaluation period.

Political Connections, Geographic Proximity, and SOE Influence Activities

According to social identity theory (Tajfel and Turner 1986), people are attracted to those who belong to the same social categories (an in-group identity), and this attraction has real economic consequences. For example, research on Performance Evaluation has found that older employees are rated lower on task performance and citizenship behavior by younger supervisors than by older supervisors (Tsui and Egan 1994). People have multiple sources of identity, and those sources vary in their salience depending on the social context.

In our setting, one likely social identity category is the political connections of executives in the SOEs to those members of the SASAC. SOE executives with a prior career history in the government are more familiar with the bureaucratic systems, administrative procedures, and macro-perspectives taken by government officials when making decisions. SOE executives with previous government experience often share similar ideologies and aspirations with SASAC officials, understand the inner workings of the bureaucracies, and can better use the language of the Chinese Communist Party when communicating with SASAC officials. As noted by an SASAC official:

> Former governmental officials think like governmental officials even after joining boards of directors of big SOEs. They tend to approach questions from a macro-economic perspective, and from the industry-wide perspective. They understand to what extent the government wants to regulate the industry and why. This is a unique difference between politically connected SOE executives and internally promoted executives in SOEs.

Another CFO explained:

> The experience of serving as governmental officials facilitates communication of SOE executives with SASAC officials.

Politically connected executives share a higher level of in-group identity with SASAC officials than do less-connected executives. A shared group identity facilitates the influence tactics of persuasion and consultation. In addition, levels of trust and reciprocity are higher and friendships and a sense of loyalty are stronger among in-group members. All of these factors can facilitate successful influence activities.

This discussion is summarized in our first hypothesis:

> **H1 (The Political Connection Hypothesis):** SOEs with executives having stronger political connections with the Chinese government receive more favorable evaluation scores from the SASAC than do SOEs whose executives have weaker political connections with the Chinese government.

Another social identity category is the geographic proximity between superiors and subordinates. Geographic proximity implies common dialects (Farh et al. 1998) and similar alumni networks (Faccio 2006), both of which facilitate social relations among people. More frequent business and social interactions between officials and business executives through leisure and social activities are likely to provide more favorable views of each other and easier communication. A study by Landier et al. (2009) of more than 250 firms with approximately five divisions within each firm showed that business divisions that were closer to headquarters were less likely to face layoffs and to be divested. SASAC officials may feel a higher level of social pressure for giving a low rating to a geographically proximate SOE, since they are more likely to know local individuals, be members of the same regional business community, and observe more directly how an individuals' personal welfare is affected.

Geographic proximity, or "home bias" as coined by Covrig et al. (2007), also implies lower communication costs and information advantages (Schultz 2003), which may influence a variety of economic behaviors. These include holding a higher proportion of local stocks in investment portfolios (Grinblatt and Keloharju 2001), higher returns of fund investors on their investments in local firms (Coval and Moskowitz 2001), more accurate forecasts by analysts about firms in closer proximity to their own brokerage firms (Malloy 2005), and higher acquirer returns for acquisitions within closer geographic proximity (Uysal et al. 2008). Incurring higher information-gathering costs can cause supervisors to be less willing to invest the required time in information collection and it is possible that SOEs with headquarters located closer to the central SASAC office in Beijing will have a greater likelihood of receiving a more accurate adjustment from the SASAC central office. Our interviews indicated how geographic proximity helps communication between SOE executives and SASAC officials. As one CFO observed:

> SOEs located in Beijing enjoy great information sharing and knowledge exchange with SASAC officials. First of all, the SASAC invites CEOs of excellent-performing SOEs to come visit their central office in Beijing and give lectures on various management topics such as Economic Value Added (EVAs), budgetary management, continuous improvements, or balance scorecards. We always attend these lectures.

Another CFO put it this way:

> The most frequent communication between the SASAC and us does not take the form of letters, emails, or telephone calls. We believe walk-in visits show more commitment, and we always bring supporting documentation that explains our operational situation in detail when visiting the SASAC office.

This discussion leads to our second hypothesis:

> **H2 (The Geographic Proximity Hypothesis):** SOEs with headquarters located closer to the SASAC central office receive more favorable evaluation scores from the SASAC than SOEs with headquarters located farther from the SASAC central office.

SPE and Governmental Favoritism

Favoritism occurs when superiors, acting on personal preferences, show partiality to some subordinates over others (Prendergast and Topel 1996). Subjectivity in Performance Evaluation facilitates favoritism because it provides superiors with the discretion to adjust performance ratings based on subjective factors that they observe. Like all Performance Evaluation processes, favoritism can have both positive and negative consequences depending on the superior's motivations. Some superiors may use their power to adjust evaluations based on having better information, but others may simply use their power for personal gain or satisfaction. Favoritism can be demoralizing to subordinates if the perception of credibility and fairness of the Performance Evaluation system is diminished. One way of reducing the possibility of favoritism is to install a system of accountability such that higher-level executives review all Performance Evaluations. This solution does not appear to be viable in our research setting. Officials in the Performance Evaluation Bureau of the SASAC are government bureaucrats for whom seniority plays a key role in compensation and promotion decisions, and these officials are not motivated to invest effort to gather information to make accurate subjective evaluations. Thus, we expect subjectivity to lead to favoritism in the annual Performance Evaluation of SOEs.

Our interviews suggest that SASAC officials want SOEs to perform well, to fulfill social goals, and to perform in a manner that can advance the official's own career. Thus, we expect the government to favorably rate those SOEs that assume more social responsibility than those that do not, a preference that can be revealed in analyzing subjective Performance Evaluation scores assigned by the SASAC. For example, the SASAC may favorably rate those who compete in strategic industries such as the military and petroleum industries and those companies that provide more employment opportunities. Consistent with the social responsibility argument, is this statement from a CEO:

> We believe getting A ratings bolsters employee morale, and distinguishes our firm from average performers. We contribute greatly to our nation's basic industries; we provide a tremendous amount of jobs, especially in minority areas. We deserve to get A's no matter how the SASAC weights economic profits versus social benefits.

A CFO from another large SOE told us that:

> There are 460,000 employees in our firm. We have taken such and such steps in creating job opportunities, and have contributed greatly to the central government's call to reduce unemployment. Of course, the SASAC should favor us and reward us for taking on social responsibility in addition to creating economic value.

These observations lead us to our third hypothesis:

> **H3 (The Social Responsibility Hypothesis):** SOEs that assume more social responsibilities receive more favorable evaluation scores from the SASAC than SOEs that assume fewer social responsibilities.

Politicians act out of self-interest and are likely to intervene in firm activities to fulfill their personal agendas. Typically, politicians need money, information, and votes to advance their careers. To advance their careers, politicians would treat firms with more assets that can afford larger campaign contributions, firms with more employees that provide a larger constituency, and firms located in political centers with politically influential CEOs, that provide richer information content and broader information channels, more favorably. We expect government officials to favorably rate SOEs that are willing to return the favor by helping to advance these officials' careers.[5] SOEs with higher political rank are likely to have more strategic allies and partners in critical political circles than SOEs of lower political rank. As a result, we speculate that the SASAC officials in charge of subjective evaluations will be more likely to evaluate those SOEs with higher political rank more favorably. One CFO noted:

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[5] In China, officials are selected through a top-down process initiated every five years by the Organization Department of the CPC Central Committee. Central government officials are elected, followed by elections of provincial, city, county, and township officials. In this process, catering to the desires of high-ranking politicians may be just as important as fulfilling the will of the populace.

> SASAC officials value networking with SOE managers, especially with those CEOs who have potential to be promoted to provincial directors. For example, Mr. W, the former CEO of CNOOC has now been promoted to secretary of the Hainan Provincial Committee of the Communist Party of China (CPC) Committee.

An SASAC official noted that the SASAC is "unfair" when setting cutoff scores and that the SASAC shows favoritism, especially to firms with higher political rank:

> Let's say that the Performance Evaluation bureau finishes summarizing all final performance scores ranked in order from highest to lowest. The director's office decides that 20 percent of the SOEs can be rated as A-level firms. By mechanical calculation, we know that the critical value should be 132. However, if a vice-ministerial-level petroleum firm gets a score of 131.5, chances are that the SASAC will finally lower the cutoff score to 131 to make room for the petroleum firm on the A-list.

This discussion leads to our fourth hypothesis:

> **H4 (The Political Rank Hypothesis):** SOEs with higher political rank receive more favorable evaluation scores from the SASAC than SOEs with lower political rank.

IV. SAMPLE, ANALYSES, AND RESULTS

Data

The data we use in our tests are derived from the following data sources: (1) a proprietary database of annual Performance Evaluation results assigned to central government controlled SOEs prepared by the SASAC from 2005 to 2007; (2) audited financial reports of 63 SOEs from the initial issuance statements of firm bonds from 2005 to 2007; and (3) complete personnel records of the top management teams of 63 SOEs from the initial issuance of firm bonds or from rating agency reports from 2005 to 2007. We obtained the remaining resumes from websites of individual companies. Our final sample consists of 63 state-owned enterprises and 189 firm-year observations.

Measurement of Variables

SOE Executives' Political Connections

Political connections considered in our study derive from career histories of SOE executives as officials in the Chinese government. The first measure of CEO political connections is *CEOConnection*, which is based on scores assigned to the highest bureaucratic positions held by CEOs before they joined the top management teams of their SOEs. We assign the highest level of political connections to those executives who have held top positions in the central government, followed by individuals in the provincial and the municipal governments. The corresponding *CFOConnection* measure is coded in the same way.[6]

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[6] We improve on previous measures of political connections that treated political connections as a dichotomous variable. We manually collected detailed resume data of top management team members of Chinese SOEs, and coded the political connection measures as the sum of the score on an individual's highest career title earned in his or her previous political career before joining the boards of directors of SOEs and the score on the individual's highest-ranking government position.

Geographic Proximity from SOE Headquarters to the SASAC Central Office

We measured geographic proximities using *Distance*,[7] the log transformation of the exact physical distance between headquarters of SOEs and the SASAC central office. This is consistent with Landier et al.'s (2009) approach that calculates the log transformation of spherical distance as the number of miles between the longitude and latitude of two different locations.

Social Responsibility of SOEs

Social responsibility is proxied by two measures. The first measure is the log transformation of employees (*Employees*) scaled by the total assets of each SOE. This number includes retired employees. The second measure reflects the industry in which individual SOE competes (*StrategicIndustry*). *StrategicIndustry* is coded as 1 in those industries in which the Chinese government seeks to retain greater power, including the military, electric power, petroleum and petrochemical, telecom, coal, civil aviation, and shipping and transportation industries. Panel B of Table 1 shows the distribution of industries in which SOEs compete in our sample. Twenty-seven SOEs are categorized into strategic industry groups and 36 SOEs are categorized into non-strategic industry groups, according to the industry classification standard prepared by the SASAC.

Political Rank of SOEs

Political rank (*SOEPoliticalRank*) means the "bureaucratic rank"[8] of the firm. We use two criteria to code the political rank of individual SOEs: whether a SOE is a "vice-ministerial level" firm[9] or whether the CEO of the SOE is assigned by the Organization Department of the CPC Central Committee. Political rank is coded as 2 for the largest 13 "vice-ministry level" firms whose CEOs are appointed by the Organization Department of the CPC Central Committee. Political rank is coded as 1 for the next tier of 40 firms whose CEOs are appointed by the Organization Department of the CPC Central Committee. Political rank of all other central government controlled SOEs is coded as 0, and the CEOs of these SOEs are appointed by the SASAC, which reports to the State Council and the CPC Central Committee.

Control Variables

Earnings before tax and extraordinary items (*EBT*), return on equity (*ROE*), the inventory turnover rate (*INTurnover*), the accounts receivable turnover rate (*ARTurnover*), and sales growth (*SalesGrowth*) serve as control variables as all SOEs are evaluated based on these measures, and the raw evaluation score of each SOE is the sum of the points granted for achieving targets based on these measures.

We use the degree of operational difficulty (*DegreeofDifficulty*) as a control variable in our empirical tests. The SASAC subjectively assigns to each SOE a parameter called the degree of operational difficulty based on assets, revenue, total profit, return on equity, number of employees, and ratio of retired employees to total employees. The comprehensive score of each SOE is a raw evaluation score multiplied by the degree of operational difficulty parameter.

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[7] As a robustness check, we use a dummy indicator, *Local*, that takes the value of 1 if the headquarters of a SOE is located in Beijing, where the central SASAC office is located. Results remain the same.

[8] SOEs have political rank for two reasons. First, before the reform separating state-owned enterprises from the government, all SOEs used the same coding system of political rank as the one used by the Chinese government. Chinese SOEs would follow the wording of government administrative rank with "section chief," "head of the department," or "head of the bureau," to distinguish the administrative rank of firm employees. Second, some SOEs were directly converted from governmental ministries during reforms that streamlined and merged governmental ministries.

[9] "Vice-ministry level" central government controlled SOEs means that their CEOs have the same political rank as vice-ministers of central government ministries.

We use organizational size as an additional control variable since our field observations suggest that size may be related to higher evaluation scores and ratings.[10] Variable measurements are described in Appendix B.

Summary Statistics

Panel A of Figure 1 shows the population distribution of SASAC evaluation scores for all 152 central government owned SOEs from 2005 to 2007. Among all 152 SOEs, we were able to extract the personnel information and complete financial indicators of 63 SOEs, whose distribution of SASAC evaluation scores is reported in Panel B of Figure 1. Panel C of Figure 1 shows the cutoff scores in the SASAC ratings assignment. Panel D of Figure 1 shows the number of SOEs that were in each class from 2005 to 2007 for the population of all 152 central government controlled SOEs. As shown in Panel D of Figure 1, the percentage of SOEs obtaining a Performance Evaluation of A increased from 18.1 percent in 2005, to 26.3 percent in 2007. The percentage of SOEs obtaining a Performance Evaluation of C decreased from 26.3 percent in 2005, to 18.4 percent in 2007. The sample distribution in Panel E of Figure 1 is consistent with this data pattern, showing that the percentage of SOEs obtaining a Performance Evaluation of A increased from 28.6 percent in 2005 to 46.0 percent in 2007, and that the percentage of SOE obtaining a Performance Evaluation of C decreased from 14.3 percent in 2005 to 3.2 percent in 2007. Thus, it appears that the SASAC has issued more lenient ratings over time.

Table 2 shows the summary statistics for our sample of 63 SOEs from 2005 to 2007. The 25th percentile of SASAC scores is 125.7, which coincides with the cutoff score for B rated firms in 2007. The 75th percentile of SASAC scores is 132.0, which coincides with the cutoff score for A rated firms in 2007.

Empirical Models

Our hypotheses are summarized in Figure 2.

To test whether a positive association exists between the variables of interest and the SASAC evaluation results, we perform a set of regressions in which the dependent variables are the SASAC evaluation results. Evaluation results measures include evaluation scores, evaluation ratings, points earned on common measures, points earned on firm-specific measures, cutoff scores, upward adjustment of raw evaluation scores, and upward adjustment of evaluation ratings. We examine whether our political connection measures, geographic proximity measures, social responsibility measures, and political rank measures load significantly in addition to the control variables when explaining the variation of SASAC evaluation results. The general model specification is as follows:[11]

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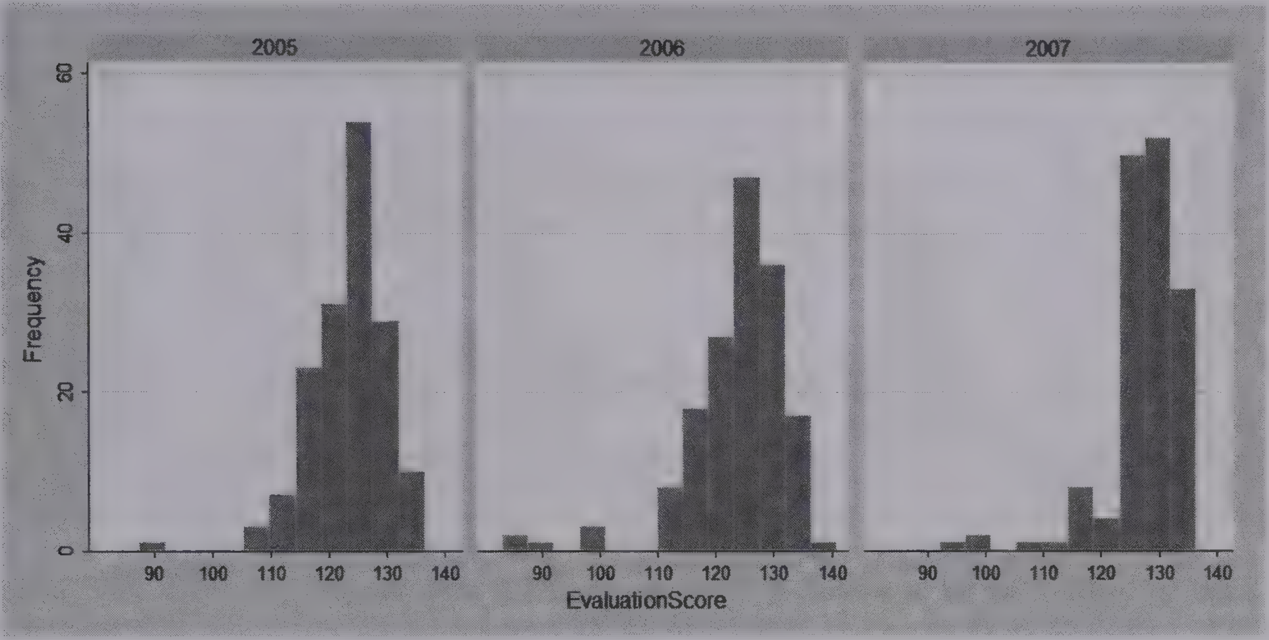
[10] A SASAC official told us that "[t]he SASAC shows favoritism to big firms due to strategic concerns, and having a larger asset base leads to a mechanical advantage when calculating the degree of operational difficulty, and thus large SOEs tend to get A's as long as they do OK jobs." The CFOs of relatively large SOEs mentioned that the SASAC has "done a great job" in giving big firms special opportunities, while the CFOs of relatively small SOEs said that the system is "unfair" knowing that largest firms are more likely to be leniently treated by the SASAC in the Performance Evaluation process. A CFO from a relatively small SOE mentioned that "A-ratings are reserved for those largest SOEs."

[11] The condition number generated from the Collin test of multicollinearity is 36.1684, which suggests the possibility of multicollinearity. The small sample size ($n = 168$) may exacerbate the multicollinearity concern for this study. Thus, we chose to introduce the independent variables one at a time instead of including all independent variables in the same regression. We also investigated whether a single common factor better reflects the underlying processes that created the correlations among our variables of interest. To this end, we performed principal factors extraction with varimax rotation in our research sample. Three factors were extracted with eigenvalues higher than 1. Consistent with our results, the social responsibility measures (*Employees*, *StrategicIndustry*) and SOE political rank measure load on the same factor, which we label Factor_Favoritism. *CFOConnection* and *Distance* load on a common factor, which we label Factor_Influence. *CEOConnection* loads on a factor, which we label Factor_CEO. We replaced the original independent variables with the factor scores in the regression models and the results are consistent with our original analyses.

FIGURE 1
Scores and Ratings Comparison of Sample Central Government Owned SOEs to the Population Central SOEs in 2005–2007[a]

Panel A: Population Distribution of Evaluation Scores of All 152 Central Government Owned SOEs in 2005–2007



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[a] Figure 1 is excerpted and summarized from the *Report Regarding the Annual Performance Evaluation Results* of central government controlled SOEs issued by the Performance Evaluation Bureau of the SASAC in 2005–2007. The SASAC evaluated 160 central government controlled SOEs in 2005 and 2006, and evaluated 152 central government controlled SOEs in 2007.

(continued on next page)

$$\begin{aligned} Evaluation\ Results_{i,t} = {} & \alpha_0 + \beta_1 Variable\ of\ Interest_{i,t} + \beta_2 DegreeofDifficulty_{i,t} + \beta_3 Assets_{i,t} \\ & + \beta_4 EBT_{i,t} + \beta_5 ROE_{i,t} + \beta_6 INTurnover_{i,t} + \beta_7 ARTurnover_{i,t} \\ & + \beta_8 SalesGrowth_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

We use OLS regression models and ordinal logit analyses to test whether our variables of interest lead to more lenient evaluation results. We use the OLS regressions to examine the effect that the variables of interest have on higher evaluation scores. We use the ordinal logit analyses to assess the effect of the variables of interest on the likelihood that SOEs belong to higher rating categories, and the likelihood that the SASAC upwardly adjusts SOEs scores and ratings.

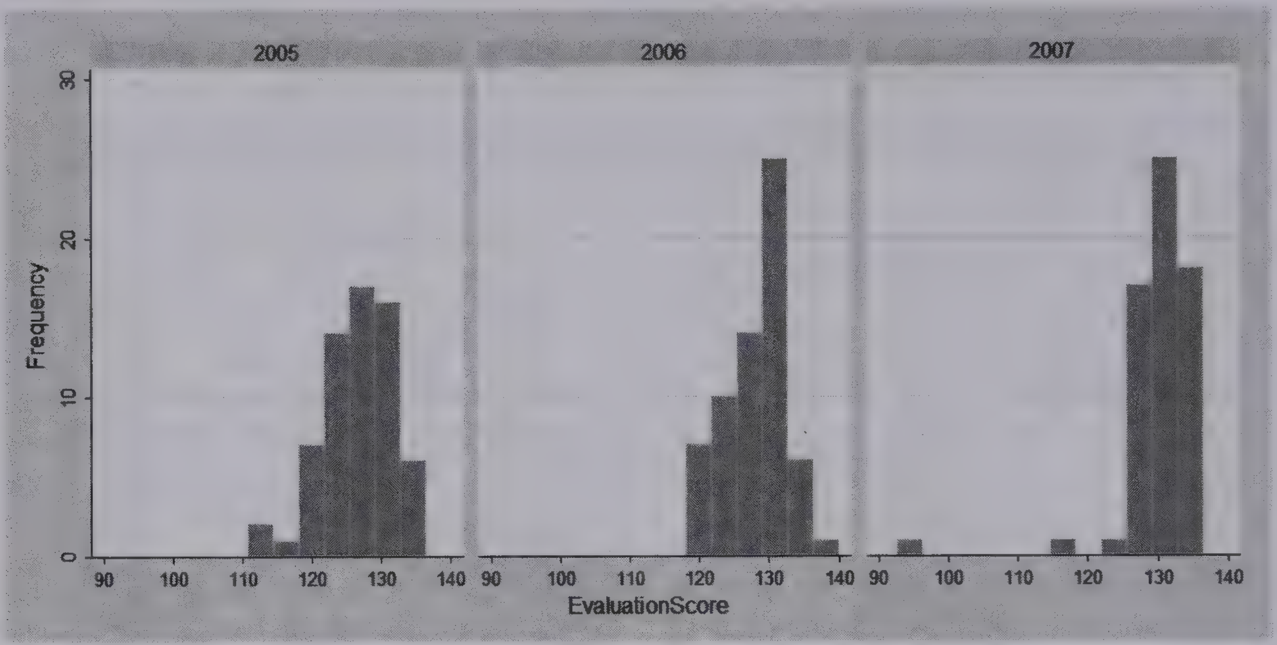
Hypotheses Tests

Test of the Political Connections Hypothesis

Results for Model 1 in Column (1) of Tables 3 and 4 show that the CEO political connection measure (*CEOConnection*) is not significant. Results for Model 2 in Column (2) of Tables 3 and 4

FIGURE 1 (continued)

Panel B: Sample Distribution of Evaluation Scores of 63 out of 152 Central Government Owned SOEs in 2005–2007



Panel C: Cut-off Scores in Rating Assignments Issued to SOEs by the SASAC in 2005–2007

	A	B	C	D	E
2007	>=132	[126,132)	[100,126)	<100	Not Available
2006	>=130	[121,130)	[100,121)	<100	Not Available
2005	>=130	[120.02,130)	[110.32,120.02)	<110.32	Not Available

(continued on next page)

show that SOEs with politically connected CFOs are more likely to get higher evaluation scores (coefficient = 0.363; t-statistic = 2.49) and to get higher evaluation ratings (coefficient = 0.238; t-statistic = 1.99).[12] In terms of percentage change, there is a 27 percent increase in the odds of a SOE getting a higher evaluation rating for a one unit increase in the CFO political connection measure. These results provide partial support for the Political Connections Hypothesis (H1) that SOEs with politically connected executives are more likely to get higher evaluation scores (ratings). This finding is consistent with remarks made by interviewees that CFOs are the major firm representatives in communication with the SASAC, and that CEOs play lesser a role in influencing the SASAC evaluation results. For example, a SASAC official mentioned that CFOs typically take the responsibility for calling, visiting, and following up with the SASAC officials. Key inputs in the SASAC Performance Evaluation process include budgetary targets and financial statement bottom-line items, and CFOs often have specific knowledge about the generation of these numbers. This

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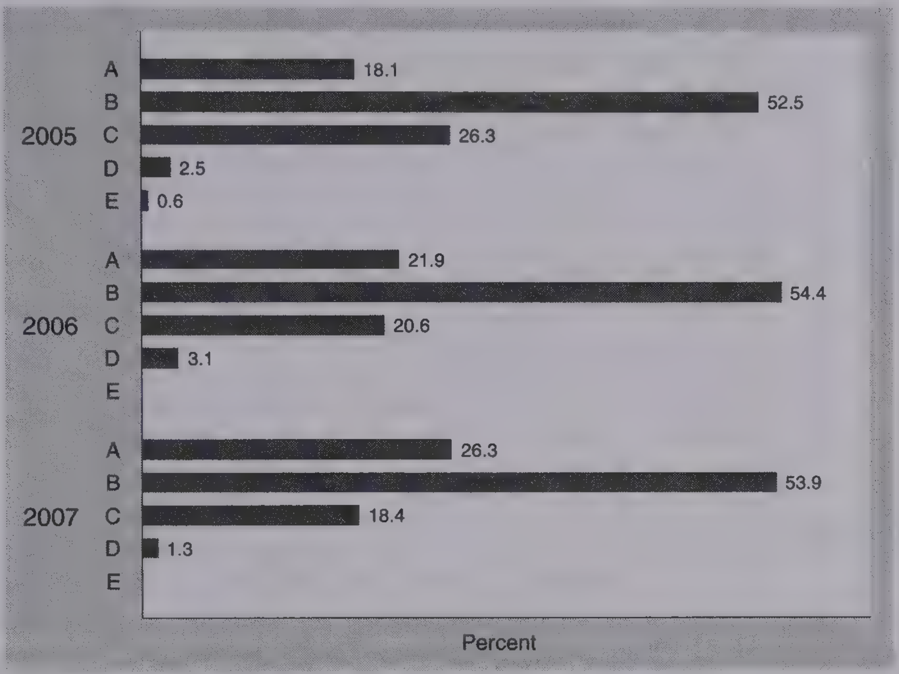
[12] We use the degree of difficulty assigned to SOEs by the SASAC as a control variable because higher degree of operational difficulty parameters mechanically translate into higher evaluation scores/ratings in this research setting. Results do not change if this control is added or dropped.

FIGURE 1 (continued)

Panel D: Population Distribution of Evaluation Ratings of All 152 Central Government Controlled SOEs in 2005–2007[b]



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[b] Percentage of central government controlled SOE sorted into Classes A through E by the SASAC in 2005–2007. Percentages of SOEs that are sorted into a certain rating class in a year are listed on the right-hand side of the bars.

Panel E: Sample Distribution of Evaluation Ratings of 63 out of 152 Central Government Controlled SOEs in 2005–2007

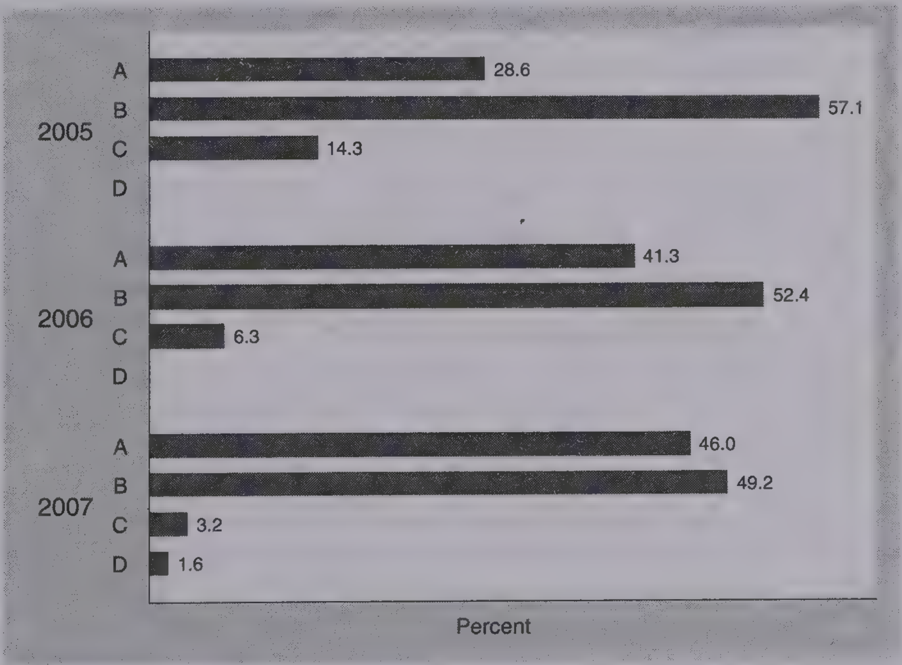

TABLE 1

Sample Statistics of SOEs Characteristics in 2005–2007

Panel A: By Headquarters Location

	% of the Total Sample by Headquarters Location/Industries	Number of SOEs by Headquarters Location/Industries
Beijing	79.37	50
An'Shan	1.59	1
Guangzhou	3.17	2
Pan Zhihua	1.59	1
Shanghai	1.59	1
Shenzhen	4.76	3
Wuhan	6.35	4
Xi'an	1.59	1
Total		63

Panel B: By Industry

	Percentage of the Total Sample by Headquarters Location/Industries	Number of SOEs by Headquarters Location/Industries
Coal	1.59	1
Military	9.52	6
Petroleum	3.17	2
Power	14.29	9
Telecom	6.35	4
Transportation	7.94	5
Strategic Industries	42.86	
Subtotal		27
Electronics	3.17	2
Infrastructure	9.52	6
Investment	4.76	3
Manufacturing	7.94	5
Materials	3.17	2
Metallurgical	7.94	5
Real Estate	3.17	2
Research	1.59	1
Trade	14.29	9
Travels	1.59	1
Non-Strategic Industries	57.14	
Subtotal		36
Total		63

Table 1 shows the distribution of geographic locations of SOE headquarters in our research sample in Panel A and the distribution of industries where SOEs compete in our research sample in Panel B. Columns 1 and 2 report the percentage of SOEs and the number of SOEs. Panel A reports the sample by headquarters location. Panel B reports the sample by industry sector.

TABLE 2
Summary Statistics of the Sample SOEs from 2005 to 2007

Variable	Mean	Std.	Min.	Lower Quartile	Median	Higher Quartile	Max.
EvaluationScore	128.42	5.21	92.60	125.70	129.38	132.03	136.52
EvaluationRating	2.30	0.63	0	2	2	3	3
CEOConnection	1.87	2.31	0	0	0	4	6
CFOConnection	1.06	1.85	0	0	0	2	5
Local	0.79	0.41	0.00	1.00	1.00	1.00	1.00
Distance	3.10	2.20	0.18	1.76	2.42	2.82	7.84
Employees	11.11	1.16	8.11	10.41	11.28	11.8	14.25
StrategicIndustry	0.42	0.5	0	0	0	1	1
SOEPoliticRank	0.67	0.69	0	0	1	1	2
DegreeofDifficulty	1.10	0.02	1.05	1.09	1.10	1.11	1.14
EBT	0.05	0.04	−0.02	0.02	0.03	0.06	0.24
ROE	0.10	0.07	−0.18	0.05	0.09	0.14	0.36
INTurnover	12.84	24.89	0.39	3.61	6.57	13.87	244.71
ARTurnover	18.34	18.97	1.56	7.2	10.28	22.36	126.24
SalesGrowth	0.29	0.22	−0.17	0.16	0.25	0.36	1.06
Assets	6.55	1.19	3.84	5.71	6.64	7.3	9.52

Table 2 describes the summary statistics of the main variables. See Appendix B for variable definitions. *Distance*, *Employees*, and *Assets* are presented after log transformation.

FIGURE 2
Summary of Hypotheses

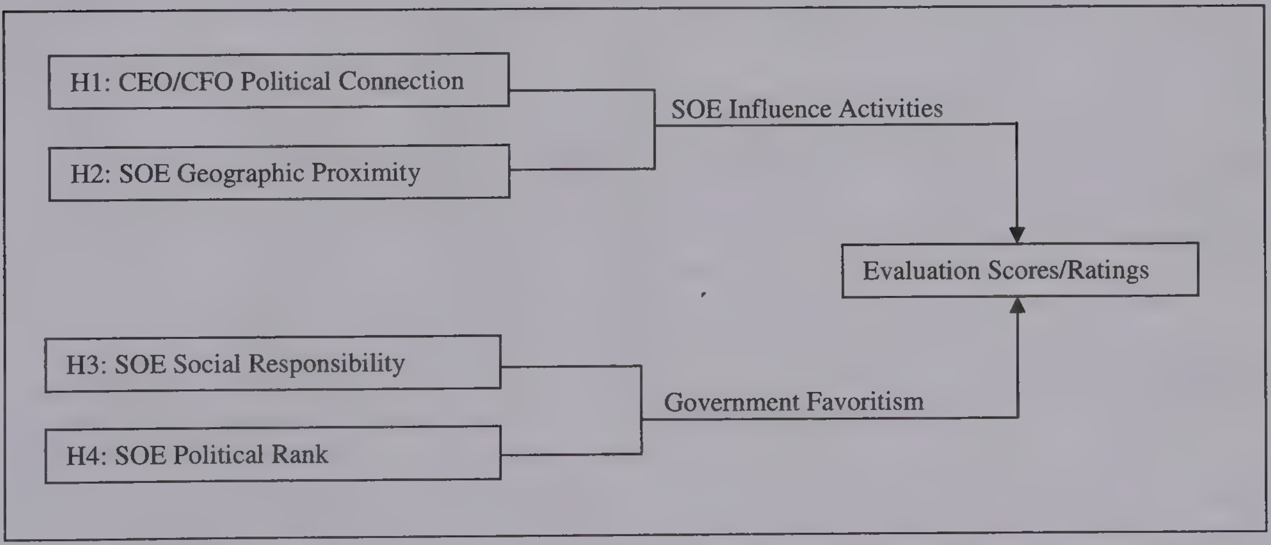
H1: CEO/CFO Political Connection
H2: SOE Geographic Proximity
SOE Influence Activities
Evaluation Scores/Ratings
H3: SOE Social Responsibility
H4: SOE Political Rank
Government Favoritism

Figure 2 illustrates the hypotheses in this study. H1 is the political connections hypothesis: SOEs with executives with stronger political connections are more likely to get higher evaluation scores (ratings). H2 is the geographic proximity hypothesis: SOEs with headquarters located proximate to the SASAC central office are more likely to get higher evaluation scores (ratings). H3 is the social responsibility hypothesis: SOEs that assume more social responsibilities are more likely to get higher evaluation scores (ratings). H4 is the political rank hypothesis: SOEs with higher political ranks are more likely to get higher evaluation scores (ratings).

TABLE 3

Associations between Political Connections, Geographic Proximity, Social Responsibility, SOE Political Rank, and SASAC Evaluation Scores

$$\begin{aligned} EvaluationScore_{i,t} = {} & \alpha_0 + \beta_1 Variable\ of\ Interest_{i,t} + \beta_2 DegreeofDifficulty_{i,t} + \beta_3 Assets_{i,t} \\ & + \beta_4 EBT_{i,t} + \beta_5 ROE_{i,t} + \beta_6 INTurnover_{i,t} + \beta_7 ARTurnover_{i,t} \\ & + \beta_8 SalesGrowth_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

	(1) Evaluation Score	(2) Evaluation Score	(3) Evaluation Score	(4) Evaluation Score	(5) Evaluation Score	(6) Evaluation Score
CEOConnection	0.092					
	(0.69)					
CFOConnection		0.363**				
		(2.49)				
Distance			−0.225*			
			(−1.84)			
Employees				0.043		
				(0.08)		
StrategicIndustry					1.065	
					(1.26)	
SOEPoliticalRank						2.074***
						(4.29)
DegreeofDifficulty	168.6***	164.5***	164.1***	165.3***	172.7***	158.4***
	(3.14)	(3.26)	(3.24)	(2.84)	(3.27)	(3.27)
Assets	0.263	0.195	0.221	0.309	−0.069	−0.444
	(0.39)	(0.30)	(0.35)	(0.46)	(−0.09)	(−0.67)
EBT	29.33***	26.21***	30.29***	28.60**	30.11***	24.62**
	(2.84)	(2.72)	(2.91)	(2.51)	(2.83)	(2.49)
ROE	−1.098	0.269	−0.477	−0.668	−1.008	2.865
	(−0.34)	(0.08)	(−0.15)	(−0.20)	(−0.32)	(0.87)
INTurnover	−0.033***	−0.032***	−0.032***	−0.030***	−0.034***	−0.019***
	(−4.67)	(−4.24)	(−5.09)	(−5.02)	(−5.49)	(−3.08)
ARTurnover	−0.034	−0.030	−0.026	−0.028	−0.028	−0.030
	(−1.37)	(−1.44)	(−1.21)	(−1.17)	(−1.17)	(−1.33)
SalesGrowth	0.164	0.111	−0.320	0.441	0.0734	−0.162
	(0.12)	(0.08)	(−0.24)	(0.36)	(0.06)	(−0.12)
Year Indicator	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.544	0.558	0.551	0.545	0.548	0.577
n	168	168	168	156	168	168

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*, **, *** Indicate two-tailed statistical significance of coefficient estimates at the 0.10, 0.05, and 0.01 levels, respectively.

Table 3 reports the regression results for the effect of executive political connections, geographic proximity, SOE social responsibility, and SOE political rank on SOE evaluation scores. Columns (1)–(6) report results when *CEOConnection*, *CFOConnection*, *Distance* between the SASAC and SOE headquarters, SOE's number of employees, and SOE political rank are used respectively as independent variables. To ensure that our inferences are not artifacts of a few extreme values, all variables are winsorized at the top and bottom 1 percent. Regressions are performed with clustered robust standard errors (Rogers 1993) to control for within firm correlation. t-statistics are in parentheses.

TABLE 4

Associations between Political Connections, Geographic Proximity, Social Responsibility, SOE Political Rank, and SASAC Evaluation Ratings

$$\begin{aligned}EvaluationRating_{i,t} = \alpha_0 &+ \beta_1 Variable\ of\ Interest_{i,t} + \beta_2 DegreeofDifficulty_{i,t} + \beta_3 Assets_{i,t}\\ &+ \beta_4 EBT_{i,t} + \beta_5 ROE_{i,t} + \beta_6 INTurnover_{i,t} + \beta_7 ARTurnover_{i,t}\\ &+ \beta_8 SalesGrowth_{i,t} + \varepsilon_{i,t}.\end{aligned}$$

	(1) Evaluation Rating	(2) Evaluation Rating	(3) Evaluation Rating	(4) Evaluation Rating	(5) Evaluation Rating	(6) Evaluation Rating
CEOConnection	0.003					
	(0.04)					
CFOConnection		0.238**				
		(1.99)				
Distance			−0.170*			
			(−1.84)			
Employees				0.021		
				(0.08)		
StrategicIndustry					1.479***	
					(2.67)	
SOEPoliticalRank						3.596***
						(4.49)
DegreeofDifficulty	40.58*	42.60**	42.73**	35.88	56.21**	46.39**
	(1.84)	(1.99)	(2.01)	(1.61)	(2.49)	(2.02)
Assets	1.442***	1.391***	1.369***	1.432***	0.893**	0.501
	(4.04)	(3.93)	(3.94)	(3.68)	(2.22)	(1.24)
EBT	20.76***	16.09**	22.46***	22.36***	21.62***	15.76*
	(2.62)	(2.00)	(2.80)	(2.71)	(2.66)	(1.79)
ROE	−1.957	−1.098	−1.758	−1.393	−1.816	3.386
	(−0.72)	(−0.39)	(−0.64)	(−0.51)	(−0.65)	(0.99)
INTurnover	−0.024***	−0.023***	−0.024***	−0.023***	−0.028***	−0.009
	(−2.90)	(−2.83)	(−3.01)	(−2.87)	(−3.39)	(−0.80)
ARTurnover	0.006	0.007	0.012	0.006	0.014	0.006
	(0.43)	(0.46)	(0.78)	(0.40)	(0.88)	(0.35)
SalesGrowth	0.476	0.256	0.185	0.525	0.399	0.192
	(0.55)	(0.29)	(0.21)	(0.58)	(0.45)	(0.18)
Year Indicator	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.388	0.401	0.399	0.390	0.412	0.525
n	168	168	168	156	168	168

*, **, *** Indicate two-tailed statistical significance of coefficient estimates at the 0.10, 0.05, and 0.01 levels, respectively.

Table 4 reports the regression results for the effect of executive political connections, geographic proximity, SOE social responsibility, and SOE political rank on SOE evaluation ratings. Columns (1)–(6) report results when *CEOConnection*, *CFOConnection*, *Distance* between the SASAC and SOE headquarters, SOE's number of employees, and SOE political rank are used respectively as independent variables. To ensure that our inferences are not artifacts of a few extreme values, all variables are winsorized at the top and bottom 1 percent. Regressions are performed with clustered robust standard errors (Rogers 1993) to control for within firm correlation. t-statistics are in parentheses.

result suggests that CFOs with strong political connections can successfully influence the SASAC's assignment of evaluation scores to their own advantage.

Test of the Geographic Proximity Hypothesis

Results for Model 3 in Column (3) of Tables 3 and 4 report that SOEs with headquarters located closer to the SASAC central office are more likely to get higher evaluation scores (ratings) with marginal significance ($p < 0.10$). These results are consistent with the prediction of the Geographic Proximity Hypothesis (H2), that SOEs with headquarters located closer to the SASAC central office are more successful in influencing the SASAC's decision in granting higher evaluation scores (ratings).

Test of the Social Responsibility Hypothesis

Results for Model 4 in Column (4) of Tables 3 and 4 show that the number of employees is not significantly related to evaluation scores or evaluation ratings. These results are not consistent with the Social Responsibility Hypothesis (H3). Results for Model 5 in Column (5) of Tables 3 and 4 suggest that firms competing in strategic industries do not receive higher evaluation scores for competing in strategic industries, as shown in Model 5, Table 3, but are more likely to belong to higher rating categories (coefficient = 1.479; t-statistic = 2.67) as shown in Model 5, Table 4. These results are consistent with the Chinese government not rewarding those SOEs that provide more employment opportunities with higher evaluation scores or ratings, but assigning higher evaluation ratings to SOEs in strategic industries.

Test of the Political Rank Hypothesis

Results for Model 6 shown in Column (6) of Table 3 show that SOEs with higher political rank are more likely to get higher evaluation scores (coefficient = 2.074; t-statistic = 4.29). Results for Model 6 in Column (6) of Table 4 show that SOEs with higher political rank are more likely to get higher evaluation ratings (coefficient = 3.596; t-statistic = 4.49). The results suggest that SOEs with higher political rank enjoy favorable treatment by obtaining higher degrees of difficulty, and can exert incremental influence in bargaining for higher evaluation scores after the parameter of degree of difficulty is set. In terms of economic significance, a one-unit increase in SOE political rank measure is associated with an increase of 2.0 points in the total evaluation score and a 3.6 increase in the log odds of being in a higher rating category. The results support the Political Rank Hypothesis, indicating that the SASAC favorably rates SOEs that help fulfill government officials' personal agendas.

Additional Analyses

In this section we conduct some additional analyses related to the how influence activities and favoritism could influence the target-setting process, the cutoff score assignment, and the *ex post* adjustments in evaluation scores and ratings.

The Association between Variables of Interest and the Target-Setting Process

Favoritism and influence costs can potentially occur at all five stages of the evaluation. In particular, it is possible that these variables affect the setting of the target in the first place. The target-setting process is a bottom-up process in which the SOEs propose annual operational targets and the SASAC either approves or adjusts those targets. Since the SASAC uses the difference between targeted amounts and actual budgeted amounts as a main parameter in score calculation, firms try to bargain for lower targets in the performance responsibility contract; a lower target assigned would mechanically lead to higher points earned.

To investigate the possibility that influence activities and government favoritism affect the target-setting process, we obtain detailed data regarding individual SOEs' points earned for achieving profit targets, points earned for achieving ROE targets, and points earned for achieving firm-specific targets. Profit targets and ROE targets, are common measures used by the SASAC to evaluate SOEs, while firm-specific targets may vary across firms. Previous research has found that only common measures affect superiors' holistic evaluations when both common measures and firm-specific measures are used in the Performance Evaluation process (Lipe and Salterio 2000). This occurs because common measures facilitate cross-sectional comparison and benchmarking. Superiors derive utility from power by favoring preferred subordinates (Prendergast and Topel 1996). At the same time, the psychological costs of confrontation inevitably arise when superiors are questioned by less-favored employees about the fairness of the evaluation system (Harris 1994; Bol et al. 2010). In our research setting, if the SASAC assigns a lower ROE common measure target to favored SOEs, then less-favored peer SOEs may question the fairness in target setting. One way for superiors to retain power and reduce the fairness concern is by setting lower targets for favored subordinates on specific measures but not on common measures. If this argument holds, then we expect to see that the variables of interest should load significantly on points earned by achieving firm-specific targets, and should not be significant when explaining the variation in points earned by achieving profit targets and points earned by achieving ROE targets.

The results in Panel A of Table 5 suggest that SOEs competing in strategic industries and having high political rank obtain fewer points on achieving targets based on common measures. On the other hand, the results in Panel B of Table 5 show that SOEs competing in strategic industries and having high political rank obtain more points on achieving targets based on firm-specific measures. This finding supports our conjecture that government favoritism leads to more slack in the target-setting process, but only in the target setting for firm-specific measures. Results are consistent with it being more difficult to manipulate the points earned on common measures rather than the points earned on SOE-specific measures. Common measures are comparable across different SOEs, making it more difficult (easier) for the government evaluator to favor a SOE by assigning lower targets on common (firm-specific) measures. Results also show that firms with high political rank are not necessarily those firms that are more profitable; rather, the data suggest that high-ranked firms have lower profitability compared to those SOEs with lower political rank. These results help to rule out the alternative explanation that the highly ranked SOEs get higher ratings because of their superior ability and performance.

The Association between Variables of Interest and Cutoff Score Assignment

We include SASAC scores as an additional control variable to examine whether the variables of interest influence the SASAC's decision of setting the cutoff scores to the advantage of individual SOEs, holding the evaluation scores constant. As shown in Model 2 in Column (2) of Table 6, the CFO political connections measure leads to a higher evaluation rating after controlling for the evaluation score (coefficient = 0.235; t-statistic = 1.82), suggesting that connected CFOs are better at bargaining with the SASAC for more advantageous cutoff scores for their own firms. SOEs whose headquarters are located closer to the central SASAC office get higher SASAC evaluation ratings, but this relation does not hold when SASAC evaluation scores are controlled for, implying that the geographic proximity does not contribute to effective manipulation of the SASAC's assignment of cutoff scores to the advantage of individual firms. Firms that compete in strategic industries receive higher SASAC evaluation ratings, and this relationship holds when evaluation scores are controlled for (coefficient = 1.390, t-statistic = 2.79), indicating that SOEs operating in

TABLE 5

Associations between Political Connections, Geographic Proximity, Social Responsibility, SOE Political Rank, and Points Earned Based on Common/Specific Measures (Target-Setting Process)

$$Points\ Earned\ Based\ on\ Common/Specific\ Measures_{i,t} = \alpha_0 + \beta_1 Variable\ of\ Interest_{i,t} + \beta_2 EBT_{i,t} + \beta_3 ROE_{i,t} + \beta_4 INTurnover_{i,t} + \beta_5 ARTurnover_{i,t} + \beta_6 SalesGrowth_{i,t} + \varepsilon_{i,t}.$$

Panel A: Points Earned Based on Common Measures (*ROE* and *EBT*)

	(1) Points Earned on Common Measures	(2) Points Earned on Common Measures	(3) Points Earned on Common Measures	(4) Points Earned on Common Measures	(5) Points Earned on Common Measures	(6) Points Earned on Common Measures
CEOConnection	−0.0204					
	(−0.12)					
CFOConnection		−0.114				
		(−0.40)				
Distance			0.106			
			(0.68)			
Employees				−0.654		
				(−1.37)		
StrategicIndustry					−2.889***	
					(−2.83)	
SOEPoliticalRank						−2.767***
						(−3.84)
EBT	25.20**	26.39**	25.34**	22.83*	28.29**	40.58***
	(2.13)	(2.11)	(2.20)	(1.89)	(2.57)	(3.29)
ROE	14.21**	13.79**	13.97**	13.33*	12.45*	8.168
	(2.18)	(2.00)	(2.09)	(1.90)	(2.00)	(1.42)
INTurnover	−0.002	−0.002	−0.002	0.004	0.012	−0.008
	(−0.25)	(−0.26)	(−0.19)	(0.37)	(0.85)	(−0.84)

(continued on next page)

TABLE 5 (continued)

	(1) Points Earned on Common Measures	(2) Points Earned on Common Measures	(3) Points Earned on Common Measures	(4) Points Earned on Common Measures	(5) Points Earned on Common Measures	(6) Points Earned on Common Measures
ARTurnover	0.015	0.014	0.013	0.028	0.010	0.021
	(0.85)	(0.77)	(0.69)	(1.25)	(0.52)	(0.94)
SalesGrowth	−0.818	−0.824	−0.619	−0.599	0.124	0.0768
	(−0.45)	(−0.45)	(−0.33)	(−0.33)	(0.07)	(0.05)
Year Indicator	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.165	0.167	0.168	0.185	0.283	0.370
n	113	113	113	105	113	113

Panel B: Points Earned Based on Firm-Specific Measures

	(1) Points Earned on Specific Measures	(2) Points Earned on Specific Measures	(3) Points Earned on Specific Measures	(4) Points Earned on Specific Measures	(5) Points Earned on Specific Measures	(6) Points Earned on Specific Measures
CEOConnection	−0.112					
	(−0.43)					
CFOConnection		0.208				
		(0.47)				
Distance			−0.256			
			(−1.14)			
Employees				1.645**		
				(2.28)		
StrategicIndustry					4.710***	
					(3.08)	
SOEPoliticalRank						5.456***
						(5.16)
EBT	−7.093	−10.42	−8.588	−5.573	−13.27	−39.24***
	(−0.61)	(−0.78)	(−0.77)	(−0.47)	(−1.10)	(−2.93)

(continued on next page)

TABLE 5 (continued)

	(1) Points Earned on Specific Measures	(2) Points Earned on Specific Measures	(3) Points Earned on Specific Measures	(4) Points Earned on Specific Measures	(5) Points Earned on Specific Measures	(6) Points Earned on Specific Measures
ROE	−28.74***	−28.33***	−28.29***	−28.50***	−26.56***	−16.97**
	(−2.92)	(−2.67)	(−2.83)	(−2.90)	(−3.13)	(−2.52)
INTurnover	−0.020	−0.022	−0.023	−0.035**	−0.044*	−0.009
	(−1.53)	(−1.47)	(−1.62)	(−2.09)	(−1.81)	(−0.51)
ARTurnover	−0.039	−0.039	−0.035	−0.064*	−0.029	−0.046
	(−1.64)	(−1.57)	(−1.47)	(−2.00)	(−1.19)	(−1.48)
SalesGrowth	1.718	1.619	1.091	1.233	0.155	0.060
	(0.75)	(0.69)	(0.45)	(0.59)	(0.07)	(0.04)
Year Indicator	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.156	0.159	0.164	0.254	0.308	0.550
n	106	106	106	98	106	106

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*, **, *** Indicate two-tailed statistical significance of coefficient estimates at the 0.10, 0.05, and 0.01 levels, respectively.

Panel A of Table 5 reports the regression results for the effect of executive political connections, geographic proximity, SOE social responsibility, and SOE political rank on points earned based on common measures including *EBT* and *ROE*. Panel B of Table 5 reports the regression results for the effect of executive political connections, geographic proximity, SOE social responsibility, and SOE political rank on points earned based on firm-specific measures. Columns (1)–(6) report results when *CEOConnection*, *CFOConnection*, *Distance* between the SASAC and SOE headquarters, SOE's number of employees, and SOE political rank are used respectively as independent variables. To ensure that our inferences are not artifacts of a few extreme values, all variables are winsorized at the top and bottom 1 percent. Regressions are performed with clustered robust standard errors (Rogers 1993) to control for within firm correlation. t-statistics are in parentheses.

TABLE 6

Associations between Political Connections, Geographic Proximity, Social Responsibility, SOE Political Rank, and Cutoff Scores

$$\begin{aligned} EvaluationRating_{i,t} = {} & \alpha_0 + \beta_1 CEOConnection_{i,t} + \beta_2 CFOConnection_{i,t} + \beta_3 Local_{i,t} \\ & + \beta_4 Distance_{i,t} + \beta_5 Employees_{i,t} + \beta_6 StrategicIndustry_{i,t} \\ & + \beta_7 SOEPoliticalRank_{i,t} + \beta_8 EvaluationScore_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

	(1) Evaluation Rating	(2) Evaluation Rating	(3) Evaluation Rating	(4) Evaluation Rating	(5) Evaluation Rating	(6) Evaluation Rating
CEOConnection	0.0597					
	(0.60)					
CFOConnection		0.235*				
		(1.82)				
Distance			−0.108			
			(−1.04)			
Employees				0.139		
				(0.55)		
StrategicIndustry					1.390**	
					(2.79)	
SOEPoliticalRank						2.724***
						(4.13)
EvaluationScore	1.025***	1.009***	1.022***	0.975***	0.976***	0.897***
	(7.50)	(7.26)	(7.53)	(6.78)	(7.26)	(6.55)
Pseudo R^2	0.630	0.640	0.634	0.631	0.654	0.710
n	186	186	189	174	189	189

*, **, *** Indicate two-tailed statistical significance of coefficient estimates at the 0.10, 0.05, and 0.01 levels, respectively.

Table 6 reports the regression results for the effect of executive political connections, geographic proximity, SOE social responsibility, and SOE political rank on evaluation ratings after controlling for evaluation scores. To ensure that our inferences are not artifacts of a few extreme values, all variables are winsorized at the top and bottom 1 percent. Regressions are performed with clustered robust standard errors (Rogers 1993) to control for within firm correlation. t-statistics are in parentheses.

strategic industries may effectively influencing the SASAC's decision when setting cutoff scores to their own advantage. Political rank of SOEs (coefficient = 2.724, t-statistic = 4.13) remains positively and significantly related to higher evaluation ratings, suggesting that these firms can get more favorable ratings by influencing the determination of cutoff scores.

To address the question of how political connections and geographic proximity influence the assignment of cutoff scores, we ran a t-test between two subsamples of SOEs: the "just-above" SOEs and the "just-below" SOEs. A firm is included in the "just-above" subsample if it is among the five firms that received evaluation scores that are just higher than the cutoff score for a specific rating category. A firm is included in the "just-below" subsample if it is among the five firms that received evaluation scores that are just lower than the cutoff score for a specific rating category. Results (not tabulated) show that "just-above" SOEs have significantly higher level of political connections than "just-below" SOEs, indicating that political connections influence the SASAC's decision in assigning cutoff scores. An ordinal logit analysis (not tabulated) suggests that SOE political rank leads to a higher chance that an SOE belongs to the "just-above" cutoff score group.

Ex Post *Discretionary Adjustments in Evaluation Scores and Evaluation Ratings*

We investigate whether our variables of interest influence the *ex post* discretionary adjustment in evaluation scores and evaluation ratings after controlling for financial performance indicators. Recall from Section II that punishment points are deducted from raw scores if SOEs have severe safety-related incidents or if they are involved in financial fraud or scandals. Further, bonus points are awarded to those SOEs that have acquired financially distressed SOEs out of political concern. Since we do not have objective measurements of safety-related incidents, financial frauds or financial scandals, and political mergers and acquisitions, we only control for financial performance indicators. This limits the inferences we can draw from the results reported in Table 7 and Table 8.

Results for Model 2 shown in Column (2) of Tables 7 and 8 show that SOEs with strongly connected CFOs are more likely to receive upward score adjustments from the SASAC but not upward ratings adjustments. Results for Model 6 in Column (6) of Tables 7 and 8 suggest that SOEs with higher political rank are more likely to receive upward ratings adjustments but not upward scores adjustments.

Results for Model 4 detailed in Column (4) of Table 7 and Model 4 shown in Column (4) of Table 8 suggest that firms with more employees are less likely to get upward adjustment in scores and ratings. This observed negative association between employee number and *ex post* score/rating adjustment may occur because SOEs with more employees are harder to manage and more likely to evince safety-related incidents and financial fraud. When safety-related incidents and financial fraud do occur, SOEs with more employees receive more public attention and more media coverage, and it is more difficult for the SASAC to cover up these events in the Performance Evaluation process. Further, we use the domestic-listing status and cross-listing status as instrumental variables to proxy for organizational salience. We use the operational scope and the firm internal Herfindahl indices as instrumental variables to proxy for organizational complexity. Results (untabulated) show that even after controlling for these instrumental variables, the measure of employee number remains negatively and significantly related to ratings adjustments, possibly indicating that the Chinese government is rewarding SOEs for reducing extra manpower, but not for creating redundant job opportunities.

The Association between Variables of Interest and Future Financial Performance of SOEs

An alternative explanation that may account for the positive association between variables of interest and evaluation results is that political connections are unique resources, and that politically connected executives bring real benefits to these SOEs. Stronger political connections may reflect more formal and informal social ties and a higher level of legitimacy, capability, and social status. It is possible that the SASAC has specific knowledge regarding SOEs' political connections and makes reasonable adjustments by giving bonus points to those companies that make best use of their political connections in creating firm value. A lead-lag performance analysis (untabulated) shows that executive political connections and firm political rank are significantly and positively related to future evaluation scores, but are not significantly related to contemporaneous or future sales growth, ROE, and EVA. Thus, our data do not support the argument that political connections create value for SOEs or that the SASAC takes into consideration the real benefit of political connections of SOEs when granting evaluation scores and assigning evaluation ratings.

TABLE 7

Associations between Political Connections, Geographic Proximity, Social Responsibility, SOE Political Rank, and Upward Adjustments in Evaluation Scores

$$\begin{aligned} DScoreAdj_{i,t} = {} & \alpha_0 + \beta_1 Variable\ of\ Interest_{i,t} + \beta_2 DegreeofDifficulty_{i,t} + \beta_3 Assets_{i,t} \\ & + \beta_4 EBT_{i,t} + \beta_5 ROE_{i,t} + \beta_6 INTurnover_{i,t} + \beta_7 ARTurnover_{i,t} \\ & + \beta_8 SalesGrowth_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

	(1) *DScoreAdj*	(2) *DScoreAdj*	(3) *DScoreAdj*	(4) *DScoreAdj*	(5) *DScoreAdj*	(6) *DScoreAdj*
CEOConnection	0.0133					
	(0.14)					
CFOConnection		0.416***				
		(3.18)				
Distance			0.003			
			(0.03)			
Employees				−0.566*		
				(−1.84)		
StrategicIndustry					−0.492	
					(−0.85)	
SOEPoliticalRank						0.640
						(1.50)
DegreeofDifficulty	−7.124	−6.051	−7.948	0.141	−12.51	−9.419
	(−0.32)	(−0.28)	(−0.37)	(0.01)	(−0.57)	(−0.44)
Assets	−0.257	−0.421	−0.241	0.134	−0.045	−0.484
	(−0.77)	(−1.26)	(−0.75)	(0.36)	(−0.12)	(−1.36)
EBT	−1.734	−3.968	−1.655	−5.264	−1.724	−3.453
	(−0.26)	(−0.55)	(−0.25)	(−0.76)	(−0.26)	(−0.51)
ROE	3.737	5.526*	3.770	3.424	3.797	4.997
	(1.25)	(1.72)	(1.26)	(1.15)	(1.27)	(1.61)
INTurnover	0.001	0.000	0.001	0.002	0.002	0.005
	(0.14)	(0.02)	(0.16)	(0.30)	(0.27)	(0.61)
ARTurnover	−0.012	−0.011	−0.012	−0.007	−0.014	−0.011
	(−0.89)	(−0.78)	(−0.87)	(−0.54)	(−1.03)	(−0.84)
SalesGrowth	−1.427*	−1.572*	−1.417	−1.328	−1.389	−1.535*
	(−1.68)	(−1.79)	(−1.63)	(−1.52)	(−1.62)	(−1.82)
Year Indicator	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.062	0.120	0.062	0.078	0.066	0.073
n	168	168	168	156	168	168

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*, **, *** Indicate two-tailed statistical significance of coefficient estimates at the 0.10, 0.05, and 0.01 levels, respectively.

Table 7 reports the regression results for the effect of executive political connections, geographic proximity, SOE social responsibility, and SOE political rank on upward adjustments in evaluation scores. Columns (1)–(6) report results when *CEOConnection*, *CFOConnection*, *Distance* between the SASAC and SOE headquarters, SOE's number of employees, and SOE political rank are used respectively as independent variables. To ensure that our inferences are not artifacts of a few extreme values, all variables are winsorized at the top and bottom 1 percent. Regressions are performed with clustered robust standard errors (Rogers 1993) to control for within firm correlation. t-statistics are in parentheses.

TABLE 8

Associations between Political Connections, Geographic Proximity, Social Responsibility, SOE Political Rank, and Upward Adjustments in Evaluation Ratings

$$\begin{aligned} DRatingAdj_{i,t} = {} & \alpha_0 + \beta_1 Variable\ of\ Interest_{i,t} + \beta_2 DegreeofDifficulty_{i,t} + \beta_3 Assets_{i,t} \\ & + \beta_4 EBT_{i,t} + \beta_5 ROE_{i,t} + \beta_6 INTurnover_{i,t} + \beta_7 ARTurnover_{i,t} \\ & + \beta_8 SalesGrowth_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

	(1) *DRatingAdj*	(2) *DRatingAdj*	(3) *DRatingAdj*	(4) *DRatingAdj*	(5) *DRatingAdj*	(6) *DRatingAdj*
CEOConnection	0.254					
	(1.08)					
CFOConnection		0.251				
		(1.02)				
Distance			−0.141			
			(−0.72)			
Employees				−1.481**		
				(−2.27)		
StrategicIndustry					1.711	
					(1.55)	
SOEPoliticalRank						3.122***
						(2.79)
DegreeofDifficulty	−15.95	−24.65	−27.26	−8.367	−8.709	−12.50
	(−0.38)	(−0.59)	(−0.66)	(−0.19)	(−0.19)	(−0.32)
Assets	−0.226	−0.070	−0.079	0.729	−0.730	−1.524*
	(−0.34)	(−0.11)	(−0.12)	(1.00)	(−0.90)	(−1.77)
EBT	3.682	1.680	5.833	−4.865	5.134	−7.963
	(0.23)	(0.10)	(0.38)	(−0.30)	(0.32)	(−0.45)
ROE	2.824	5.770	4.160	4.289	5.182	11.09*
	(0.57)	(1.10)	(0.88)	(0.84)	(1.03)	(1.71)
INTurnover	−0.004	−0.002	−0.001	0.004	−0.005	0.016
	(−0.23)	(−0.08)	(−0.08)	(0.29)	(−0.35)	(0.68)
ARTurnover	0.049	0.049	0.051*	0.070*	0.053*	0.062*
	(1.44)	(1.56)	(1.66)	(1.95)	(1.69)	(1.80)
SalesGrowth	−2.138*	−2.116	−2.253*	−1.480	−2.472*	−3.074**
	(−1.66)	(−1.62)	(−1.72)	(−1.04)	(−1.89)	(−2.21)
Year Indicator	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.138	0.136	0.127	0.198	0.153	0.266
n	168	168	168	156	168	168

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*, **, *** Indicate two-tailed statistical significance of coefficient estimates at the 0.10, 0.05, and 0.01 level, respectively.

Table 8 reports the regression results for the effect of executive political connections, geographic proximity, SOE social responsibility, and SOE political rank on upward adjustments in evaluation ratings. Columns (1)–(6) report results when *CEOConnection*, *CFOConnection*, *Distance* between the SASAC and SOE headquarters, SOE's number of employees, and SOE political rank are used respectively as independent variables. To ensure that our inferences are not artifacts of a few extreme values, all variables are winsorized at the top, and bottom 1 percent. Regressions are performed with clustered robust standard errors (Rogers 1993) to control for within firm correlation. t-statistics are in parentheses.

V. CONCLUSION AND DISCUSSION

The main objective of this study is to investigate the two-way process in which a subordinate and a superior engage in influence activities and favoritism, respectively, in subjective Performance Evaluation. To study this process we employ a database that contains information of economic importance from China, one of the world's largest economies. Our study suggests that the use of subjectivity in Performance Evaluation leads to influence activities, consistent with evaluators hesitating to use subjective evaluation even when objective measures are less than perfect. Furthermore, our interviews suggest that social interaction and unofficial communication between superiors and subordinates influence the level and direction of the subjective adjustment of Performance Evaluation scores. These results enrich our understanding of the complexity of social behavior in the Performance Evaluation process beyond the simple dichotomy of using objective versus subjective measures. Insights from the field also allow us to examine subjectivity in more detail. Subjectivity manifests itself in bargaining for lower operational targets, bargaining for upward adjustment of actual performance because of uncontrollable factors, bargaining for upward adjustment of raw scores, and by affecting the SASAC's determination of cutoff scores of ratings assignments.

We use social identity theory to identify factors that we expect to contribute to the effectiveness of influence activities such as persuasion and consultation in the subjective evaluation setting. Having CFOs with career histories as governmental officials and locating headquarters in geographical proximity to the evaluator (the SASAC office) facilitate the communication between the SOEs and the SASAC and, thus, lead to higher evaluation scores and ratings. This result contributes to the influence literature by demonstrating how subordinates' political connections and geographic proximity contributes to their SOEs' ability to influence the evaluation outcomes. We investigate the firm characteristics that lead to favorable governmental treatments in evaluation scores/ratings. Results indicate that the political rank of SOEs is positively and significantly related to the evaluation scores/ratings.

This study also addresses a puzzling question in the literature regarding why SEOs that receive more favorable governmental treatment perform worse than their counterparts in the private sector. Prior research has focused on the relationship between state ownership and firm value (Sapienza 2004; Dinc 2005), but has rarely discussed the mechanism used by the government that could distract SOEs from making value-maximizing decisions. Our empirical findings suggest that SOEs must balance multiple goals and make trade-offs between social responsibility and economic benefits, which leads to the use of multiple-measure Performance Evaluation when SOEs are evaluated. These social objective measures are hard to quantify and may conflict with one another. Subjectivity is then employed to strengthen incentives by incorporating additional information not included in objective measures. Given that the performance appraisal results influence SOE executives' compensation, promotion, and political career advancement, there are clear incentives for SOE executives to maximize measures officially included in the evaluation formula. However, when subjectivity is included in the appraisal process and when governmental officials are granted discretion in adjusting the evaluation scores/ratings, governmental evaluators can provide more favorable evaluation scores to those SOEs that comply. In addition, officials can treat larger SOEs with politically influential executives more favorably and, in return, hope for reciprocity from these executives who may later wish to advance their careers. Facing unfair treatment under subjective evaluation supervised by the government officials, SOE executives may divert their time and effort from maximizing firm value to pleasing government officials and, in the process, forgo business opportunities.

Our study has some limitations that may lead to future research. First, we follow Ittner et al. (2003) and Hoppe and Moers (2011) in trying to separate different types of subjectivity. We also show that subjectivity is manifested in several forms and is embedded in the Performance Evaluation system and in the target-setting process. Even though we distinguish our study by examining both influence activities and favoritism simultaneously, the correlation among the empirical proxies is likely to limit the study's ability to isolate these two constructs. Second, while our database derives from the Chinese government's evaluation of SOEs, future studies could be extended to the private sector in situations where the firm headquarters evaluates the performance of multiple business divisions.

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APPENDIX A

SASAC Performance Evaluation System[a]

Measures	Base Points for Achieving Target	Range of Points	Points Granted	Degree of Difficulty (DoD)	Scores
Earnings before Tax and Extraordinary Items[b]	30	[24, 36]	P_EBT	[1.01, 1.14]	S_EBT = P_EBT × DoD when the preset target on EBT is achieved. Otherwise, the SOE just gets the points with no multiplier effect.
Return on Equity[c]	40	[32, 48]	P_ROE	[1.01, 1.14]	S_ROE = P_ROE × DoD when the preset target on ROE is achieved. Otherwise, the SOE just gets the points with no multiplier effect.
Inventory Turnover	15	[12, 18]	P_INTurn	[1.01, 1.14]	S_INTurn = P_ INTurn × DoD when the preset target on inventory turnover is achieved. Otherwise, the SOE just gets the points with no multiplier effect.
Accounts Receivable Turnover	15	[12, 18]	P_ARTurn	[1.01, 1.14]	S_ARTurn = P_ ARTurn × DoD when the preset target on accounts receivable turnover is achieved. Otherwise, the SOE just gets the points with no multiplier effect.
Sales Growth	15	[12, 18]	P_Growth	[1.01, 1.14]	S_Growth = P_Growth × DoD when the preset target on sales growth is achieved. Otherwise, the SOE just gets the points with no multiplier effect.
Subtotal: Objective	100	80–120			
Occurrence of Safety Incidents	0	$(-\infty, 0]$	P_Incidents		S_Incidents = P_S Incidents
Occurrence of Financial Frauds and Scandals	0	$(-\infty, 0]$	P_Frauds		S_Frauds = P_Frauds
Acquisition of Financially Distressed Firms Out of Political Concern	0	$[0, +\infty)$	P_Acquisition		S_Acquisition = P_Acquisition
Subtotal: Adjustments	0	$(-\infty, +\infty)$			
EvaluationScore	*Scores Based on Objective Measures + Scores Based on Discretionary Adjustments*				

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[a] Appendix A is excerpted and summarized from the *Report Regarding the Annual Performance Evaluation* of central government controlled SOEs issued by the Performance Evaluation Bureau of the SASAC in 2003.

[b] An SOE can get one additional point when exceeding the preset EBT target by 3 percent.

[c] Additional points granted for exceeding the ROE target depend on the level of the preset target for ROE. When the preset target is lower than the historical record and the industry record, an SOE can get one additional point when exceeding the preset target by 4 percent. When the target is higher than or equal to the historical record or the industry record, an SOE can get one additional point when exceeding the preset target by 3 percent.

APPENDIX B
VARIABLE DEFINITIONS

Dependent Variables

$EvaluationScore_{it}$ = comprehensive Performance Evaluation score prepared by the SASAC assigned to SOE *i* in fiscal year *t*;

$EvaluationRating_{it}$ = rating assigned by the SASAC to SOE *i*. Evaluation ratings are 3 when SOEs are rated A, 2 when they are rated B, 1 when they are rated C, and 0 when they are rated D or E;

$Cutoff_{it}$ = dummy indicator that SOE *i* belongs to the "just-above" group of SOEs. An SOE is included in the "just-above" group if it is among the five firms that received evaluation scores that are just higher than the cutoff scores for the specific rating;

$DScoreAdj_{it}$ = *ex post* discretionary adjustment in evaluation scores. *DScoreAdj* is 2 if an SOE's final evaluation score is higher than the raw score, 1 if an SOE's final evaluation score equals the raw score, and 0 if an SOE's final evaluation score is lower than the raw score; and

$DRatingAdj_{it}$ = *ex post* discretionary adjustment in evaluation ratings. *DRatingAdj* is 2 if the SASAC adjust upward an SOE's rating based on discretionary adjustment in evaluation scores, 1 if the SASAC does not adjust an SOE's rating, and 0 if the SASAC adjust downward an SOE's rating.

Control Variables

$DegreeofDifficulty_{it}$ = degree of operational difficulties assigned by the SASAC to SOE *i*. This parameter, when multiplied by points earned, converts scores on separate financial performance measures of an individual SOE into its final comprehensive evaluation score;

$Assets_{it}$ = natural log of total assets of SOE *i* at the end of year *t*;

EBT_{it} = earnings before tax and extraordinary items of SOE *i* in year *t* scaled by total assets of SOE *i* in year *t*;

ROE_{it} = return on equity of SOE *i* in year *t*;

$INTurnover_{it}$ = operational cost divided by average inventory of SOE *i* in year *t*;

$ARTurnover_{it}$ = operational revenue divided by average accounts receivable of SOE *i* in year *t*; and

$SalesGrowth_{it} = (Sales_t - Sales_{t-1})$ divided by $Sales_{t-1}$ of SOE *i* in year *t*.

Political Connections

$CEOConnection_{it}$ = highest bureaucracy position a CEO occupied before joining an SOE top management team. The position is manually coded into two dimensions of categorical measures: the rank of the bureau in which the CEO had served, and the rank of the political title the CEO held. The ranking of the bureau is sorted into three categories: central government (= 3), provincial government (= 2), and municipal government (= 1). The ranking of the political title is also sorted into three levels, ministerial/provincial level (= 3), board/regional level (= 2), and unit level (= 1); and

$CFOConnection_{it}$ = coded as above for *CEOConnection*.

Geographic Proximity

$Distance_{it}$ = log transformation of distance between SOE headquarters and SASAC central office.

Social Responsibility

$Employees_{it}$ = natural log of the number of employee of SOE *i* scaled by *Assets* in fiscal year *t*; and

$StrategicIndustry_{it}$ = dummy of industry in which SOE *i* competes in year *t*. *StrategicIndustry* is coded as 1 when an SOE competes in industries in which the government wants to retain absolute control power, including the military, electric power, petroleum and petrochemicals, telecom, coal, civil aviation, and shipping and transportation; *StrategicIndustry* is coded as 0 when an SOE competes in industries not mentioned above.

Political Rank

$SOEPoliticalRank_{it}$ = political rank of the SOE *i* is located in fiscal year *t*. The SASAC sorts Chinese SOEs into three levels of administrative rank. *SOEPoliticalRank* is coded as 2 if an SOE is sorted into the highest administrative rank, coded as 1 if an SOE is sorted into the second highest administrative rank, and coded as 0 if an SOE is sorted into the third highest administrative rank.

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Unintended Consequences of LIFO Repeal: The Case of the Oil Industry

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ABSTRACT: This study examines the effect on firm value of repealing the last-in, first-out (LIFO) inventory method for tax purposes. Our model extends prior literature by determining quantities and prices in equilibrium, rather than specifying them exogenously. We find that LIFO repeal could increase the future after-tax cash flows of firms that had used LIFO, because the higher tax costs associated with FIFO result in lower equilibrium quantities and higher equilibrium output prices, which increase pretax cash flows. We illustrate our model by examining inventory methods used by firms in the oil industry.

Keywords: *LIFO; FIFO; tax incidence.*

I. INTRODUCTION

We examine the effects of repealing the last-in, first-out (LIFO) inventory method on firm production decisions, output prices, and firm after-tax profits. This is an important topic because repeal of LIFO, either directly or indirectly, as a consequence of adopting International Financial Reporting Standards (IFRS), is being considered by U.S. policymakers. Although our model applies to any industry, we discuss the implications of our model for the oil industry because (1) almost all firms in the industry use LIFO for their U.S. operations, and (2) demand for the oil industry's products is inelastic. Our model implies that LIFO repeal would cause the after-tax profits of firms in the oil industry to increase because (1) the higher marginal cost would reduce production and raise prices, and (2) inelastic demand implies that the higher output price would more than offset the higher tax cost associated with the first-in-first-out (FIFO) inventory method.[1]

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[1] In the absence of a transitional rule, repeal of LIFO would also cause firms to repay prior-year accumulated tax savings. Although our study does not address this effect, we discuss some implications in Section IV.

For nearly 20 years, Kang's (1993) "real value" model has influenced accounting research on the link between LIFO and firm value. The real value model implies that the value of the firm in the absence of inflation is the same as the value of the firm in the presence of inflation if the firm uses LIFO. LIFO provides a nominal tax gain, but not a real inflation-adjusted gain. This suggests that LIFO repeal would decrease the value of a firm that used LIFO. A critical assumption underlying the real value model is that neither inflation nor inventory method choice affects firms' production decisions or output prices. In this study, we relax that assumption, deriving equilibrium production decisions and prices instead of specifying them exogenously. We find that, unlike the results from the real value model, inflation and inventory choice can affect production decisions and firm value.

Our approach applies insights from the industrial organization literature regarding the effect of cost increases on production decisions and profits under Cournot competition.[2] In particular, an increase in costs, such as income taxes, that affects all firms in an industry induces all firms to reduce output, which, in turn, increases the equilibrium output price. Depending on the slope of an industry's marginal revenue curve, decreasing industry output quantity can either increase or decrease the profits and, hence, the value of the firms in the industry. If the industry marginal revenue curve is downward sloping, a cost increase causes the value of firms to decrease, because the higher selling price is not enough to offset the lost revenue from selling fewer units. In contrast, if the industry marginal revenue curve is upward sloping, a decrease in industry output results in an increase in industry total revenue, and this increase more than offsets the higher cost. Therefore, a cost increase causes the value of all firms in the industry to increase.

Nelson (1957) and Meyer (1967) identify situations in which increases in costs can lead to increased industry profits. The idea of increasing marginal revenue at the industry level may, at first, seem unrealistic. However, as Formby et al. (1982, 303) point out, "the conditions for a positively sloping marginal revenue curve are much less stringent than is generally recognized. Simple transformations of any well-behaved convex demand function can easily result in a demand for which marginal revenue is positively sloping. For this reason, positively sloping marginal revenue functions must be considered whenever convex demand functions are analyzed." In other words, the only restriction on increasing marginal revenue is that the demand curve must be convex.

An excise tax on inputs, such as the tax on the sale of domestically produced coal, is one example of a cost that affects all firms in an industry. Katz and Rosen (1985) and Seade (1985) show circumstances under which an increase in a tax can increase after-tax industry profits. Our study extends this idea to the effects of a firm's inventory method used for income tax purposes. When costs are increasing due to inflation, the use of FIFO instead of LIFO by all firms in an industry is economically equivalent to an excise tax on inputs imposed on all firms. Therefore, if the industry marginal revenue curve is increasing, all firms in an industry would have greater after-tax cash flows by using FIFO instead of LIFO. However, unlike an excise tax, firms can choose an inventory cost flow assumption (LIFO) that avoids the tax increase. Every firm would prefer for other firms to use FIFO while it chooses LIFO, getting both the tax benefits of LIFO for itself while also benefiting from the reduced quantities and higher prices associated with every other firm choosing FIFO. This can occur, for example, if a non-U.S. firm that is not permitted to use LIFO under home country tax rules competes in the same market with a U.S. firm that is permitted to use LIFO. Therefore, each firm has an incentive to choose LIFO, even in a situation in which every firm would be better off if every firm were to choose FIFO.

One way to have all firms use FIFO when doing so would increase the value of every firm is to no longer allow the use of LIFO for tax purposes in the U.S. This suggests that LIFO repeal would increase firm value if the industry's marginal revenue curve is upward sloping and all firms had adopted LIFO.

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[2] Under Cournot competition, firms in an industry simultaneously make production decisions to maximize their own profits, under the assumptions that they know (1) the production decisions of their competitors, and (2) the industry demand curve.

By allowing inventory choice to affect production decisions and output prices, our model is in the spirit of Ball (1972, 25), who states:

> Many . . . accounting changes are associated with changes in other variables which affect cash flows. For example, unexpected increases in factor prices could trigger accounting changes to the LIFO method of inventory valuation, which in turn could reduce tax payments and perhaps increase stock market prices . . . Under the theory of perfect competition, we could even argue that product prices adjust until aftertax profits return to their former equilibrium.

Although the effect of LIFO on firm value has long been of interest to accounting researchers, the topic has recently taken on new importance for two reasons. First, the Obama administration's 2012 budget proposal contained a provision to eliminate LIFO for tax purposes, a proposal that was strongly opposed by a "LIFO coalition" of business interests (see http://www.savelifo.org). Second, the United States is currently moving toward harmonizing U.S. GAAP with International Financial Reporting Standards, under which LIFO is not an acceptable method. Because firms in the U.S. that use LIFO for tax purposes must also use LIFO for financial reporting purposes (Internal Revenue Code [IRC] §472(c)), adopting IFRS would eliminate LIFO for any firm required to use IFRS unless the conformity requirement in §472(c) were repealed. Thus, understanding the effects of LIFO on firm value is important in the context of both taxation and financial reporting standard setting.

In our model, firms operate in an imperfectly competitive environment. Each firm knows its industry's demand curve and input costs, and makes a Cournot conjecture regarding the production decisions of the other firms in the industry. Each firm in an industry chooses output quantity to maximize firm value by choosing its production quantity to equate the firm's after-tax marginal revenue and after-tax marginal cost, assuming that all other firms will set production levels in this same way.

We first consider the effects of LIFO repeal by comparing firm values when all firms use LIFO to firm values when all firms use FIFO. We find that all firms are worth more if all firms use FIFO than if all firms use LIFO if and only if the industry's marginal revenue curve is upward sloping. We illustrate this result in the special case of a demand function with constant own price elasticity, which allows us to relate our result to demand elasticity rather than to the slope of the marginal revenue curve. In this case, LIFO repeal would increase firm value when all firms use LIFO if and only if the demand function is inelastic. Next, we consider the general case in which some firms use LIFO and some firms use FIFO. In that case, LIFO firms benefit less from repeal, because the effect of LIFO repeal on output price is attenuated by the presence of FIFO firms. On the other hand, FIFO firms unequivocally benefit from LIFO repeal as long as some firms in the industry use LIFO.

We then apply our model to the oil industry, in which LIFO is widely used. Our model implies that LIFO repeal would induce lower output and higher prices of products such as gasoline. Because demand for gasoline is inelastic (Espey 1996), oil firms would earn higher after-tax profits in the U.S. as a consequence of LIFO repeal. We emphasize that this is an equilibrium outcome that the firms could not have achieved on their own, because each U.S. firm would maximize its own after-tax profits by using LIFO if all other firms choose FIFO.

Our paper highlights how financial reporting, taxation, and managerial accounting can each play a role in a firm's optimal inventory accounting method decision. Our model also highlights a rare instance in which a firm's financial reporting decision can change the firm's real production decisions through LIFO's effect on the marginal cost of production. Our main contribution is to show that LIFO repeal could increase the value of firms that had used LIFO because the higher tax costs associated with FIFO are mitigated and, in some cases, more than offset by the increase in pretax cash flows which occur due to lower equilibrium quantities and higher equilibrium output prices.[3]

Section II presents our model and shows how LIFO repeal would affect the value of LIFO firms if all firms use LIFO. In Section III, we show the effects of LIFO repeal on firm value if some firms use FIFO. Section IV shows how the inventory method choices by oil industry firms are consistent with our model, and examines the effects of LIFO repeal in that industry. Section V concludes.

II. A MODEL OF LIFO REPEAL

Economic Setting

We consider an industry with N identical firms making a single identical product. Both the cost and price associated with any particular output volume of this product increase each period at the inflation rate π. This is the type of cost increase assumed in Kang's (1993) real value model. On any date j, $j \in \{0, 1, 2, \ldots\}$, the cost of the product is $c(1+\pi)^j$ per unit. Because our analysis focuses on cost increases, we assume that nominal prices are weakly increasing (i.e., $\pi \geq 0$).

Each firm i produces q_i units of the product on each date j, and holds this same number of units in inventory between the date the units are produced (date j) and the subsequent period when the units are sold (date j+1). In other words, the q_i units sold by firm i on date j+1 were produced on date j and held in inventory between the two dates.

All products sold in an industry sell for the same price, P, which is a function of the aggregate quantity produced by the industry (Q) in the prior period, so price is a function of quantity, $P(Q)$. We assume that $P'(Q) < 0$ (i.e., demand is downward sloping). Each firm i pays income tax at rate τ on taxable income when the inventory is sold, where taxable income is equal to revenue, $q_i P(Q)$, less cost of goods sold. Recall that the firm holds q_i units of inventory between the date the units are produced and the date they are sold. If firm i uses the FIFO inventory method, its deduction for cost of goods sold on date j+1 is $c(1+\pi)^j$ per unit sold; if firm i uses the LIFO inventory method, its deduction for cost of goods sold on date j+1 is $c(1+\pi)^{j+1}$ per unit sold. We represent the deduction for cost of goods sold as $q_i c(1+\pi)^j(1+\lambda\pi)$, where $\lambda = 1$ if the firm uses LIFO, and $\lambda = 0$ if the firm uses FIFO. The real cost of capital for each firm is r, so the nominal discount factor is $(1+r)(1+\pi)$.

The present value of after-tax cash flows for the *industry* is maximized if all firms use LIFO and the quantity produced by the industry equates industry after-tax marginal revenue and after-tax marginal cost. However, if each firm strives to maximize the present value of its own after-tax cash flows, it will choose LIFO and a higher level of production than 1/Nth of optimal industry production. The reason is that one of the costs of firm i's higher production is a lower output price, and this cost is borne equally by all the firms in the industry. Therefore, if each firm strives to maximize its own profits, industry output will exceed the level at which industry-wide profits are maximized.

Because the firms are identical, we consider the case in which all firms in the industry use the same inventory method. To ensure that the second-order condition for firm profit maximization will be satisfied, we also assume that N is sufficiently large,[4] i.e.:

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[3] Our results could also be applied to empirical studies of the valuation effect of LIFO adoption (e.g., Hand 1993), although this would require knowing the own price demand elasticities of the sample industries.

[4] This is the stability assumption made in Seade (1980).

$$N > -\frac{QP''(Q) + P'(Q)}{P'(Q)}. \tag{1}$$

When making the production decision for firm i at date j, the manager makes a conjecture regarding competitor firms' production decisions. The manager expects the selling price on date j+1 to be $P(Q)(1+\pi)^{j+1}$, $Q = q_i + \bar{q}(N-1)$, where $\bar{q}$ is the Cournot conjecture that the manager of firm i makes regarding its competitors' production decisions on date j.[5] Although firm i only chooses its own output, in a symmetric Cournot equilibrium, the industry produces $Q = q_iN$ units. Each manager chooses the firm's production quantity and inventory method in a non-cooperative fashion, maximizing the firm's own after-tax profit.

Equilibrium

We characterize firm value in two cases: (1) all firms use LIFO, and (2) all firms use FIFO. Firm i's profit maximization problem when producing on date j and selling on date j+1, framed in terms of the present value of future cash flows as of date j, is:

$$\max_{q_i}\left\{ q_i\left[\frac{P[q_i + \bar{q}(N-1)](1-\tau)(1+\pi)^{j+1}}{(1+r)(1+\pi)} - c(1+\pi)^j + \frac{\tau c(1+\pi)^j(1+\lambda\pi)}{(1+r)(1+\pi)}\right]\right\}. \tag{2}$$

Differentiating Expression (2) with respect to q_i and simplifying yields the first-order condition for profit maximization. Substituting q_i for $\bar{q}$ in the first-order condition reflects the manager's expectation that the production of the other firms in the industry will equal the production of firm i, resulting in $Q = Nq_i$. The equilibrium production quantity (q_i^*) for firm i on date j is the production that solves Equation (3):

$$q_i^* P'(Q)(1-\tau)(1+\pi) + P(Q)(1-\tau)(1+\pi) - c(1+\pi)(1+r) + \tau c(1+\lambda\pi) = 0. \tag{3}$$

The second-order condition for a maximum is satisfied because of Equation (1).

Equation (3) reveals that in an industry in which all firms use LIFO, inflation has no effect on output quantity because all of the $(1+\pi)$ terms factor out of Equation (3) when $\lambda = 1$. Therefore, using LIFO eliminates any effect that inflation would have on production decisions.

The solution to Expression (2) is the present value of the future cash flow resulting from the profit-maximizing production at time j. If we denote this solution to Expression (2) as Π, then the value (V) of the firm on date zero is:

$$V = \sum_{j=0}^{\infty} \frac{\Pi}{(1+r)^j(1+\pi)^j}. \tag{4}$$

If the firm uses LIFO ($\lambda = 1$), this expression simplifies to:

$$V_L = \frac{q_i^*\left[\Big(P(Q) - c\Big)(1-\tau) - rc\right]}{r}. \tag{5}$$

Because there is no π term in Equation (5), and because inflation does not affect the value of q_i^* for a LIFO firm, inflation has no effect on the value of the LIFO firm. If the firm uses FIFO ($\lambda = 0$), Equation (4) simplifies to:

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[5] Because all firms in an industry are identical, the manager assumes that each firm's production decision will be the same.

$$V_F = \frac{q_i^*\left[\left(P(Q) - c\right)(1-\tau) - rc\right]}{r} - \frac{\tau c \pi q_i}{r(1+\pi)}. \tag{6}$$

We show how inflation affects firm value in our model when all firms use the same inventory method in Proposition 1.

Proposition 1: Comparing the value of a firm when inflation is positive to the value of the same firm when inflation is zero:

(a) If all firms in an industry use LIFO, the value of the firm is unchanged;
(b) If all firms in an industry use FIFO, the value of the firms will be higher if and only if $QP''(Q) + 2P'(Q) > 0$.

The proof is in Appendix A.

Proposition 1(a) is the same result as in Kang's (1993) "real value" model. Under these assumptions, electing LIFO does not change the value of a firm relative to what it was under FIFO with no inflation, because production decisions do not change. Proposition 1(b) reflects a known, albeit paradoxical, result from the industrial economics literature—under Cournot competition, an increase in costs, including tax costs, can increase the profits of all firms in an industry if the industry's marginal revenue curve is upward sloping. The intuition for this result is that if firms use FIFO, the increase in tax costs induces each firm to reduce its output, which, in turn, increases the equilibrium price. Because firms that strive to maximize their own profits produce at a higher level than the level at which industry profits are maximized, requiring all firms to use FIFO shifts output closer to the level that maximizes industry profits. If the condition $QP''(Q) + 2P'(Q) > 0$ is satisfied, the effect of reduced output on pretax profits more than offsets the higher tax costs associated with FIFO and, thus, all firms in the industry do better if all firms use FIFO than if all firms use LIFO.[6] We emphasize that it is not in the interest of any one firm to adopt FIFO unilaterally, because that firm would bear all of the increased tax costs, whereas the benefit from increased output price would be shared by all firms in the industry. Therefore, the increase in firm value associated with industry-wide adoption of FIFO cannot be achieved via unilateral action by one firm.

The term $[2P'(Q) + QP''(Q)]$ in Proposition 1(b) is the derivative of *industry* marginal revenue (*MR*), where $MR(Q) = QP'(Q) + P(Q)$ and $MR'(Q) = QP''(Q) + 2P'(Q)$. As long as the industry demand curve is convex, the slope of the industry marginal revenue curve could be positive or negative. If industry marginal revenue is decreasing, inflation decreases the value of a FIFO firm. In contrast, if industry marginal revenue is increasing, inflation increases the value of a FIFO firm.

Industry marginal revenue could behave in various ways. In the case of a linear demand curve, industry marginal revenue is decreasing in Q. In the case of a demand curve with constant price elasticity of demand, industry marginal revenue is positive and decreasing in Q if demand is elastic and is negative, and increasing in Q if demand is inelastic. Firm value is increasing if all firms adopt FIFO when demand is inelastic, because negative marginal revenue implies that a decrease in industry output results in an increase in industry total revenue, such that the higher total revenue more than offsets the higher tax cost from FIFO.[7] Formby et al. (1982, 303) point out that "simple transformations of any well-behaved convex demand function can easily result in a demand for which marginal revenue is positively sloping."

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[6] Although industry after-tax profits and tax revenues increase, consumer surplus decreases by an even greater amount. This occurs because consumers pay higher prices and there is a decrease in consumer surplus associated with the reduction in quantity.

[7] The demand for the product is elastic if the percentage change in price is less than the percentage change in quantity; the demand for the product is inelastic if the percentage change in price is greater than the percentage change in quantity.

The implication of Proposition 1 is that LIFO repeal could increase or decrease firm value. When $\pi = 0$, LIFO and FIFO firms have the same value. For higher values of π, the value of LIFO firms does not change; the value of FIFO firms could increase or decrease, depending on the slope of the industry marginal revenue curve. Therefore, if the industry marginal revenue curve is increasing, LIFO repeal would increase firm value.

Example

We illustrate Proposition 1 using as an example a demand function that exhibits constant price elasticity of demand. The selling price on date j+1 is:

$$P(Q) = \frac{d(1+\pi)^{j+1}}{Q^{1/\varepsilon}}, \tag{7}$$

where $d > 0$ and $\varepsilon > 0$ are exogenous demand parameters. Expressing quantity as a function of price yields:

$$Q(P) = \left[\frac{d(1+\pi)^{j+1}}{P}\right]^{\varepsilon}. \tag{8}$$

The price elasticity of demand, $E(Q)$, is:

$$E(Q) \equiv \frac{\partial Q}{\partial P}\frac{P}{Q} = -\varepsilon, \tag{9}$$

which is constant for this demand function. If $\varepsilon > 1$, the demand for the product is elastic; i.e., the percentage change in price is less than the percentage change in quantity. If $\varepsilon < 1$, the demand for the product is inelastic; i.e., the percentage change in price is greater than the percentage change in quantity.

Using Expression (2), the manager of firm i chooses q_i on date j to solve:

$$\max_{q_i}\left\{ q_i\left[\frac{P[q_i+\bar{q}(N-1)](1-\tau)}{(1+r)(1+\pi)} - c(1+\pi)^j + \frac{\tau c(1+\pi)^j(1+\lambda\pi)}{(1+r)(1+\pi)}\right]\right\}. \tag{10}$$

To solve for the equilibrium quantities under LIFO and FIFO, differentiate Expression (10) to find the first-order condition for each firm. As each firm faces the same production decision problem, and all firms in an industry are identical, we substitute $q_i = \bar{q}$ into the first-order condition to find the equilibrium production quantity for firm i on date j. When all firms in the industry use LIFO, the optimal production quantity is:

$$q^*_{iL} = \frac{1}{N}\left[\frac{(1-\tau)d(\varepsilon N-1)}{c\varepsilon N(1+r-\tau)}\right]^{\varepsilon}. \tag{11}$$

The second-order condition is satisfied because the condition in Equation (1) implies that $N > \frac{1}{\varepsilon}$. The optimal production quantity for a LIFO firm is not affected by the inflation rate π.

When all firms in the industry use FIFO, the corresponding equilibrium quantity is:

$$q^*_{iF} = \frac{1}{N}\left[\frac{(1-\tau)d(1+\pi)(\varepsilon N-1)}{c\varepsilon N\{(1+r)(1+\pi)-\tau\}}\right]^{\varepsilon}. \tag{12}$$

Note that when $\pi = 0$, the optimal output quantity is the same under both LIFO and FIFO.

The cash outflow per unit from production on date j is the same for firms using either LIFO or FIFO, and is equal to $c(1+\pi)^j$, so total cash outflow for a firm using LIFO is:

$$q_{iL}^*[c(1+\pi)^j]. \tag{13}$$

For a firm using LIFO, the after-tax cash inflow on date $j+1$ from selling products that were produced on date j is:

$$q_{iL}^*\left[\frac{(1-\tau)d(1+\pi)^{j+1}}{(Nq_{iL}^*)^{1/\varepsilon}} + \tau c(1+\pi)^{j+1}\right]. \tag{14}$$

Using a discount factor of $(1+r)(1+\pi)$, the value of the firm on date zero when all firms in the industry use LIFO is:

$$V_L = \sum_{j=0}^{\infty} q_i^*\left[\frac{(1-\tau)d}{(Nq_{iL}^*)^{1/\varepsilon}(1+r)^{j+1}} + \frac{\tau c}{(1+r)^{j+1}} - \frac{c}{(1+r)^j}\right]. = \frac{c(1+r-\tau)\left[\frac{d(\varepsilon N-1)(1-\tau)}{c\varepsilon N(1+r-\tau)}\right]^{\varepsilon}}{N(\varepsilon N-1)r}. \tag{15}$$

The value of the LIFO firm is not affected by the inflation rate π.

For a firm using FIFO, the after-tax cash inflow on date $j+1$ from selling products that were produced on date j is:

$$q_{iF}^*\left[\frac{(1-\tau)d(1+\pi)^{j+1}}{(Nq_{iF}^*)^{1/\varepsilon}} + \tau c(1+\pi)^{j}\right]. \tag{16}$$

The only difference between Expressions (16) and (14) is the value of the tax savings, $\tau c(1+\pi)^j$, rather than $\tau c(1+\pi)^{j+1}$. Using a discount factor of $(1+r)(1+\pi)$, the value of the firm on date zero when all firms in the industry use FIFO is:

$$\begin{aligned} V_F &= \sum_{j=0}^{\infty} q_{iF}^*\left[\frac{(1-\tau)d}{(Nq_{iF}^*)^{1/\varepsilon}(1+r)^{j+1}} + \frac{\tau c}{(1+r)^{j+1}(1+\pi)} - \frac{c}{(1+r)^j}\right] \\ &= \frac{c\{(1+r)(1+\pi)-\tau\}\left[\frac{d(1+\pi)(\varepsilon N-1)(1-\tau)}{c\varepsilon N\{(1+r)(1+\pi)-\tau\}}\right]^{\varepsilon}}{N(\varepsilon N-1)(1+\pi)r}. \end{aligned} \tag{17}$$

If inflation is zero ($\pi = 0$), the value of the FIFO firm in Equation (17) is the same as the value of the LIFO firm in Equation (15).[8] However, with positive inflation ($\pi > 0$), the value of the FIFO firm relative to the value of the LIFO firm is given by the ratio V_F/V_L:

$$\frac{V_F}{V_L} = \left[\frac{(1+\pi)(1+r-\tau)}{(1+\pi)(1+r)-\tau}\right]^{\varepsilon-1}. \tag{18}$$

The term in brackets is less than 1, so the ratio itself is less than 1 if $\varepsilon > 1$, and greater than 1 if $\varepsilon < 1$. Therefore, assuming positive inflation, if the industry has a demand curve that exhibits constant price elasticity of demand, and demand is elastic, firm value is higher under LIFO, the same result that Kang (1993) and Hand (1993) report. However, contrary to the results from Kang's (1993) model, when we allow inflation to affect output quantities, firm value is higher under FIFO than LIFO when demand is inelastic.[9]

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[8] Equations (15) and (17) are equivalent to Equations (5) and (6) for the demand curve given by Equation (8).

[9] Firm value is unaffected by inventory method choice when $\varepsilon = 1$.

Using this particular demand curve, in which demand exhibits constant price elasticity, inelastic demand ($\varepsilon < 1$) is a sufficient condition to arrive at the general result from Proposition 1(b) that $\frac{\partial V_F}{\partial \pi} > 0$; i.e., inflation increases the value of a FIFO firm if and only if $2P'(Q) + QP''(Q) > 0$. We explore the intuition behind this condition by examining the properties of the industry revenue curve, $R(Q) = QP(Q)$. Taking the derivative yields the marginal revenue for the industry:

$$MR(Q) = QP'(Q) + P(Q). \tag{19}$$

Rearranging terms and using Equation (9) yields:

$$MR(Q) = P(Q)\left[1 + \frac{1}{E(Q)}\right], \tag{20}$$

where $E(Q) < -1$ when demand is elastic, and $E(Q) > -1$ when demand is inelastic. Therefore, $MR(Q)$ is negative if demand is inelastic and positive if demand is elastic. Differentiating Equation (20) with respect to Q and noting that $E(Q)$ is equal to $-\varepsilon$, a constant, in our example yields:

$$MR'(Q) = P'(Q)\left[1 - \frac{1}{\varepsilon}\right]. \tag{21}$$

Therefore, in our example, the slope of the industry marginal revenue curve is positive when demand is inelastic ($\varepsilon < 1$), and is negative when demand is elastic ($\varepsilon > 1$). This means that for firms in industries with inelastic demand, a mandated switch to FIFO increases after-tax industry profits due to a reduction in industry production that yields an increase in price and an increase in total industry revenue.

III. EFFECTS OF LIFO REPEAL IF SOME FIRMS USE FIFO

In this section, we illustrate the case in which some firms choose LIFO and others choose FIFO. We assume that different firms make different inventory method choices for reasons external to the model, such as facing different costs of adopting LIFO or different preferences regarding the use of LIFO for financial reporting purposes. In this section, we consider the effects of LIFO repeal on firm value when, prior to repeal, M of the firms in an industry use LIFO and the remaining N-M firms use FIFO.

As demonstrated in Section II, the effect of LIFO repeal on the value of a LIFO firm when all firms use LIFO is a combination of two effects; the tax effect reduces firm value, but the effect of reduced supply on output price increases firm value. Which effect dominates depends on the slope of industry marginal revenue. When some firms use FIFO, the tax effect of LIFO repeal remains for the LIFO firms, but the effect on price is attenuated because the FIFO firms were already producing at a reduced level. Therefore, the effect of LIFO repeal on the value of LIFO firms is less favorable when some firms already use FIFO. In contrast, FIFO firms always benefit from LIFO repeal; their tax treatment does not change, but they benefit from the increased output price that occurs when LIFO firms must switch to FIFO.

We illustrate these effects for an example case in which the industry faces a linear demand function. The selling price on date j+1 is:

$$P(Q) = (1 + \pi)^{j+1}(d - aQ), \tag{22}$$

where $d > c(1 + r)$ and $a > 0$ are exogenous demand parameters.[10]

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[10] The industry marginal revenue curve is always downward sloping when demand is linear.

A firm that uses LIFO chooses q_L on date j to solve:

$$\max_{q_L}\left\{ q_L \left[\begin{array}{l} \dfrac{(1+\pi)^{j+1}\left(d - a[q_L - (M-1)\bar{q}_L - (N-M)\bar{q}_F]\right)(1-\tau)}{(1+r)(1+\pi)} \\ -c(1+\pi)^j + \dfrac{\tau c(1+\pi)^{j+1}}{(1+r)(1+\pi)} \end{array} \right] \right\}. \tag{23}$$

A firm that uses FIFO chooses q_F on date j to solve:

$$\max_{q_F}\left\{ q_F \left[\begin{array}{l} \dfrac{(1+\pi)^{j+1}\left(d - a[q_F - M\bar{q}_L - (N-M-1)\bar{q}_F]\right)(1-\tau)}{(1+r)(1+\pi)} \\ -c(1+\pi)^j + \dfrac{\tau c(1+\pi)^{j}}{(1+r)(1+\pi)} \end{array} \right] \right\}. \tag{24}$$

To solve for the equilibrium quantities, we differentiate Expressions (23) and (24) to arrive at the first-order condition for each firm, then substitute q_F for $\bar{q}_F$ and q_L for $\bar{q}_L$ into the first-order conditions to find the equilibrium production quantity for each type of firm on date j. Firms that use LIFO choose:

$$q_L^* = \frac{d - c(1+r-\tau)}{a(1+N)} + \frac{c\tau\pi(N-M)}{a(1+N)(1+\pi)}. \tag{25}$$

Firms that use FIFO choose:

$$q_F^* = \frac{d - c(1+r-\tau)}{a(1+N)} - \frac{c\tau\pi(1+M)}{a(1+N)(1+\pi)}. \tag{26}$$

Substituting the quantities from Equations (25) and (26) into Expressions (23) and (24) yields the present value on date j of after-tax cash flows associated with production on date j and sales on date $j+1$:

$$\Pi_L = \frac{(1+\pi)^{j-2}\{[d - c(1+r-\tau)](1+\pi) + c\tau\pi(N-M)\}^2}{a(1+N)^2(1+r)}; \tag{27}$$

$$\Pi_F = \frac{(1+\pi)^{j-2}\{[d - c(1+r-\tau)](1+\pi) - c\tau\pi(1+M)\}^2}{a(1+N)^2(1+r)}. \tag{28}$$

Therefore, the values of the respective firms prior to LIFO repeal are:

$$V_L = \sum_{j=0}^{\infty} \frac{\Pi_L}{(1+r)^j(1+\pi)^j} = \frac{\{[d - c(1+r-\tau)](1+\pi) + c\tau\pi(N-M)\}^2}{r(1+\pi)^2 a(1+N)^2}, \tag{29}$$

and:

$$V_F = \sum_{j=0}^{\infty} \frac{\Pi_F}{(1+r)^j(1+\pi)^j} = \frac{\{[d - c(1+r-\tau)](1+\pi) - c\tau\pi(1+M)\}^2}{r(1+\pi)^2 a(1+N)^2}. \tag{30}$$

After LIFO repeal, all N firms use FIFO and, thus, all have value V:

$$V = \frac{\{[d - c(1+r-\tau)](1+\pi) - c\tau\pi(1+N)\}^2}{r(1+\pi)^2 a(1+N)^2}. \tag{31}$$

Because *N-M* firms use FIFO, Equation (29) implies that V_L is increasing in the number of FIFO firms and, thus, the effect of LIFO repeal on the value of LIFO firms is less favorable as the number of FIFO firms increases. In contrast, comparing Equations (30) and (31) shows that FIFO firms benefit from LIFO repeal unless all firms in the industry already use FIFO.

IV. INVENTORY CHOICE IN THE OIL INDUSTRY

We illustrate the implications of our model of LIFO repeal by examining the oil industry, SIC 2911 Petroleum Refining. We choose the oil industry for several reasons. First, LIFO is widely used by corporations that supply the U.S. market for refined petroleum products. Second, economists have studied the demand for the outputs of this industry, including gasoline, extensively. They estimate demand to be inelastic with respect to price, with an upward sloping marginal revenue curve at the industry level. Third, repeal of LIFO for oil companies only was proposed in 2006 to pay for a reduction in the federal excise tax on gasoline. The Wyden-Gregg tax reform bill proposed in 2010 also proposed LIFO repeal for oil companies. The Obama administration's 2012 budget proposal also included repeal of LIFO for all firms. About 65 percent of the U.S. market for refined petroleum products is provided by five U.S. firms—ConocoPhillips, ExxonMobil, Valero Energy, Chevron, and Marathon Oil (Molavi 2011). The first Compustat inventory method code listed for each of these five firms for 2009 is LIFO.[11] For 2009, Compustat lists 15 firms incorporated in the U.S. with SIC code 2911 (Petroleum Refining), and for 14 of these firms the first inventory method code listed is LIFO. The exception, Murphy Oil Corporation, had $335 million of inventory valued at LIFO in 2009, out of total inventory of $734 million. There was a $551 million reserve associated with its LIFO inventory, however, suggesting that the reason LIFO is not shown as the first inventory method code for Murphy Oil is the low historical cost of its LIFO inventory.

Roughly 15–20 percent of the U.S. market is represented by U.S. subsidiaries of foreign corporations; for example, Shell Oil Company is owned by Royal Dutch Shell. Although Royal Dutch Shell uses International Financial Reporting Standards, which do not allow LIFO, Revenue Ruling 78-246 allows U.S. subsidiaries of foreign corporations to use LIFO even though the consolidated financial statements use FIFO. Turgeon et al. (2009) report that many U.S. subsidiaries with foreign parents use LIFO without violating the conformity requirement. We conclude that nearly all firms in the Petroleum Refining industry use LIFO with respect to their U.S. operations.

Consider what would happen to firms in the oil industry if LIFO were repealed for U.S. tax purposes. Assuming positive inflation, U.S. firms or U.S. subsidiaries of foreign firms currently using LIFO would face an increase in their marginal costs, which would induce them to reduce output, which, in turn, would increase the sales price of their products. Because products such as gasoline exhibit inelastic demand, the increase in after-tax income due to the higher sales price would exceed the decrease in after-tax income due to lower sales volume and higher tax costs. Therefore, our model predicts that LIFO repeal would increase the future after-tax cash flows of firms in the oil industry. We emphasize that absent collusion, the same firms cannot achieve this outcome on their own by voluntarily switching to FIFO, because each firm would maximize its own after-tax profits by using LIFO, even though all firms in the industry are better off if they all use FIFO.

We conclude this section by estimating the increase in the value of U.S. oil companies if LIFO were to be repealed, and comparing it to the potential tax liability on the firms' existing LIFO reserves. Using the long-run price elasticity of demand for gasoline of –0.58 (Espey 1996), the average inflation rate since 1971 of 4.4 percent, a cost of capital of 10 percent, and a tax rate of 35 percent, Equation (18) implies that the value of the oil industry's assets would increase by about 0.82 percent if LIFO were repealed and the repeal affected the value of all of the industry's assets. This is an upper bound on the increase in value because these are multinational firms; sales in foreign jurisdictions would not be affected by LIFO repeal.[12] The 15 U.S. companies in SIC 2911 had a market value of assets, computed as the market value of equity plus the book value of liabilities, of about $1.2 trillion.

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[11] According to the Compustat manual, the order in which inventory method codes are listed reflects the relative amounts of a firm's inventory valued under each method, with the first method listed reflecting the method used for the largest portion of the firm's inventory.

The LIFO reserves of these 15 companies in 2010 were about $54.4 billion. Whether and how this amount would be included in taxable income is an open question. The Obama administration's proposal is to recognize the LIFO reserve as taxable income ratably over ten years. Using a discount rate of 10 percent and a tax rate of 35 percent, the present value of tax on the oil industry's LIFO reserves is about $13 billion, or about 1 percent of the industry's assets.

V. CONCLUSION

Our study extends Kang's (1993) "real value" model of the effect of LIFO on firm value by allowing an industry's output quantity and product price to be determined endogenously, so that both are affected by inventory method choices made by firms in the industry. With positive inflation, inventory method choice for tax purposes affects optimal output quantities, setting marginal cost equal to marginal revenue, because the additional tax cost under FIFO affects a firm's marginal cost.

The ability to use LIFO for U.S. tax purposes could be eliminated in the future, either by Congress changing the tax law or by U.S. standard setters adopting IFRS. Our results imply that the effect of LIFO repeal on firm value could have unexpected results. We find that for some industries, the value of LIFO firms would increase due to LIFO repeal, because the repeal would force firms to use FIFO in industries in which all firms would be worth more if all firms chose FIFO, but each firm individually has an incentive to use LIFO. In the case of an industry facing a demand curve with constant price elasticity, LIFO repeal would increase firm value if demand is inelastic.

We illustrate our model by examining the oil industry, in which LIFO is widely used by both U.S. firms and U.S. subsidiaries of foreign firms. Products produced by the oil industry, such as gasoline, have inelastic demand. We find that LIFO repeal would have the unintended consequence of increasing the after-tax profits of firms in the oil industry by raising marginal cost, such that the reduction in output and the corresponding increase in output price would outweigh the higher tax cost associated with FIFO.

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[12] For example, only 40 percent of ExxonMobil's petroleum sales (as measured by barrels) were in the U.S. in 2010.

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APPENDIX A

PROOF OF PROPOSITION 1

(a) From Equation (3), we know that q_i^* is unaffected by π when all firms choose LIFO. All other terms in Equation (5) are exogenous constants, so firm value is not affected by π when all firms choose LIFO.

(b) To see how inflation affects the value of a FIFO firm, we first differentiate Equation (6) with respect to the inflation parameter π:

$$\frac{\partial V_F}{\partial \pi} = \frac{q_i[P'(Q)N(1-\tau)]}{r}\frac{\partial q_i}{\partial \pi} + \frac{[P(Q)-c](1-\tau)-rc}{r}\frac{\partial q_i}{\partial \pi} - \frac{q_i \tau c}{r(1+\pi)^2} + \frac{\tau c \pi}{r(1+\pi)}\frac{\partial q_i}{\partial \pi}. \quad \text{(A1)}$$

To determine $\frac{\partial q_i}{\partial \pi}$, we represent the first-order condition in Equation (3) as $F(q_i, \pi) = 0$, substitute $Q = Nq_i$, then implicitly differentiate F to yield:

$$F_{q_i} = (1-\tau)(1+\pi)[q_i P''(Q)N + P'(Q) + NP'(Q)]; \quad \text{(A2)}$$

$$F_{\pi} = q_i P'(Q)(1-\tau) + P(Q)(1-\tau) - c(1+r); \quad \text{(A3)}$$

$$\frac{\partial q_i}{\partial \pi} = -\frac{F_\pi}{F_{q_i}} = -\frac{q_i P'(Q)(1-\tau) + P(Q)(1-\tau) - c(1+r)}{(1+\pi)(1-\tau)[QP''(Q) + (1+N)P'(Q)]}. \quad \text{(A4)}$$

Substituting the value of $\frac{\partial q_i}{\partial \pi}$ from Equation (A4) into Equation (A1), using the result from Equation (3) that:

$$c = \frac{(1+\pi)(1-\tau)[P(Q) + q_i P'(Q)]}{(1+\pi)(1+r) - \tau(1+\lambda\pi)},$$

and simplifying yields:

$$\frac{\partial V_F}{\partial \pi} = -\frac{Q\tau(1-\tau)[NP(Q) + QP'(Q)][2P'(Q) + QP''(Q)]}{rN^2(1+\pi)[(1+\pi)(1+r) - \tau][(1+N)P'(Q) + QP''(Q)]}. \quad \text{(A5)}$$

The effect of inflation on the value of a FIFO firm depends on the sign of Equation (A5). To determine the sign of Equation (A5), we first consider the term $[NP(Q) + QP'(Q)]$ in the numerator. Firm i's revenue (R) is equal to $q_i P(Q)$, and marginal revenue ($\partial R/\partial q_i$) is equal to $q_i P'(Q) + P(Q)$. Multiplying marginal revenue by N results in $Nq_i P'(Q) + NP(Q)$, which is equal to $QP'(Q) + NP(Q)$ because $Q = Nq_i$. Therefore, $[NP(Q) + QP'(Q)]$ is the product of N and the marginal revenue of firm i, which is positive because each firm equates its own marginal revenue with its own marginal cost. The term $[(1+N)P'(Q)+QP''(Q)]$ in the denominator of Equation (A5) is strictly negative because of Equation (1). Therefore, the sign of Equation (A5) is the same as the sign of the term $2P'(Q)+QP''(Q)$.

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Do IRS Audits Deter Corporate Tax Avoidance?

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ABSTRACT: We extend research on the determinants of corporate tax avoidance to include the role of Internal Revenue Service (IRS) monitoring. Our evidence from large samples implies that U.S. public firms undertake less aggressive tax positions when tax enforcement is stricter. Reflecting its first-order economic impact on firms, our coefficient estimates imply that raising the probability of an IRS audit from 19 percent (the 25th percentile in our data) to 37 percent (the 75th percentile) increases their cash effective tax rates, on average, by nearly two percentage points, which amounts to a 7 percent increase in cash effective tax rates. These results are robust to controlling for firm size and time, which determine our primary proxy for IRS enforcement, in different ways; specifying several alternative dependent and test variables; and confronting potential endogeneity with instrumental variables and panel data estimations, among other techniques.

Keywords: *tax enforcement; tax compliance; IRS audits; taxes.*

JEL Classifications: *M40; G34; G32; H25.*

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I. INTRODUCTION

Recent evidence implies that natural byproducts of tough corporate tax enforcement include lower managerial diversion (e.g., Desai and Dharmapala 2006, 2009; Desai et al. 2007), improved earnings quality (e.g., Hanlon et al. 2011), and valuable cross-monitoring evident in debt (e.g., Guedhami and Pittman 2008) and equity (e.g., El Ghoul et al. 2011) pricing. However, extant research neglects to examine the more primitive issue of whether tax enforcement disciplines firms by constraining their tax avoidance. We bridge this gap by providing evidence on whether public firms undertake less aggressive tax positions when the expected likelihood of an Internal Revenue Service (IRS) audit is higher.

Firms are understandably eager to invest in tax planning to lower their taxes since this benefits shareholders as the residual claimants (e.g., Mills 1996; Mills et al. 1998). However, firms also consider the costs stemming from aggressive tax avoidance strategies, including the economically material fines, interest, and penalties that the IRS can impose for under-reporting (Wilson 2009). In this paper, we evaluate whether tax avoidance subsides when corporate tax enforcement is better. Although this prediction may initially appear intuitive, there are several reasons to suspect that corporate tax avoidance is insensitive to IRS oversight. In particular, managers may not perceive IRS audits as sufficiently costly (Hanlon et al. 2007), or may have little incentive to reduce their cost (Slemrod 2004; Graham et al. 2005; Armstrong et al. 2012). Also, irrespective of the severity of IRS enforcement, firms may refrain from drastically lowering their taxes in order to avoid potential political costs stemming from being labeled as tax aggressive (e.g., Hanlon and Slemrod 2009; Mills et al. 2010).

Moreover, firms may behave like wealthy individuals by increasing tax avoidance when IRS monitoring is stricter to ensure that their after-audit tax liability remains stable (Slemrod et al. 2001). For example, a recent report by PricewaterhouseCoopers (2004, 6) stresses that: "Tax compliance risk also includes the risks arising from. . .enquiries on, or the audit of, submitted tax returns . . . by fiscal authorities . . . [T]he final agreement of a tax return often ends in a 'horse trade' between the taxpayer and the relevant revenue authority; it may make sense to have a number of aggressive positions in the return so that there is something to give as part of any negotiations." In other words, companies may undertake more aggressive positions to provide some negotiating room when they perceive that an IRS audit is more likely.

Similarly, the IRS may share this interpretation of its bargaining process with firms. Slemrod (2007, 32) recounts that: "IRS Commissioner Mark Everson (2005) testified to the President's Advisory Panel on Tax Reform that the IRS had 'a reputation for trading [penalties] away,' so that it was 'always in the interest of the noncompliant taxpayer to take an aggressive position with the Service.'" Further, if managers believe that the IRS benchmarks the firm's tax position this year to its tax position last year, then managers may surmise that deviating from avoidance strategies implemented previously may attract more intense IRS scrutiny this year.

Moreover, since corporate tax departments sometimes operate as profit centers with their directors having incentive to increase current year profits or decrease GAAP effective tax rates (Martucci 2001; Hollingsworth 2002; Robinson et al. 2010), they may be reluctant to pursue other favorable tax outcomes, such as attempting to reduce the frequency of future audits (Armstrong et al. 2012; Slemrod 2004). Adding tension to our analysis, managers may also discount the cost of an IRS audit when they consider their tax position to be highly defensible. For example, Hanlon et al. (2007) document that of the firms audited in their sample, 45 percent had no proposed tax deficiency and of those firms with a proposed deficiency identified by the IRS, only 60 percent of these amounts were later paid, suggesting that even conditional upon occurring, these audits are sometimes not very costly to firms. In short, it remains an empirical question as to whether strict IRS monitoring deters corporate tax avoidance.

To address our research question, we primarily gauge corporate tax avoidance with firms' cash effective tax rates, which is the amount of cash taxes paid by the firm scaled by its pretax income. We follow Guedhami and Pittman (2008) and Hanlon et al. (2011) by relying on data from the Transactional Records Clearinghouse (TRAC)—a nonpartisan research watchdog affiliated with Syracuse University that publicly releases statistics on the performance of many federal agencies according to the government's own real data—to measure on-the-ground IRS enforcement. This data source suits our focus on how the agency actually exercises its authority to enforce tax compliance.

Given our interest in capturing the *ex ante* threat of an IRS audit according to managers' perceptions rather than its actual incidence, we mainly specify corporate tax enforcement using a TRAC report on time-varying audit probabilities across the eight nominal asset levels that the IRS use in aggregating its auditing data.[1] We also conduct a survey of managers, which validates our primary test variable in that a substantial number of managers use historical data provided by the tax authority (which is the source of the TRAC data) to gauge tax enforcement. Our analysis spans the earliest (1992) to the latest (2008) year that IRS audit rate data are available. Analyzing this long timeframe is constructive for identification since corporate tax enforcement was ascending at some points during this period and descending at others. However, we caution that this time-series variation is imperfect for our purposes given that IRS enforcement was generally declining during the 17 years under study, with the rise in enforcement predominantly occurring within a short period late in the 1992–2008 timeframe. More positively, the source of variation in IRS audit rates that reflect firm size, time, and interactions between them—factors generally considered removed from management choice—is likely exogenous.[2]

We find that closer IRS monitoring limits corporate tax avoidance. Since IRS audit rates are aggregated by firm size and time, we triangulate our results to help dispel concern that the impact of either of these variables is spuriously behind our evidence. However, we continue to observe an inverse relation between IRS enforcement and tax avoidance when we re-specify the controls for both size and time in several ways. Economically, our coefficient estimates translate into firms' cash effective tax rates rising by, on average, almost two percentage points (a 7 percent increase in relative terms) when the probability that the firm will be subject to an IRS audit increases from 19 percent (the 25th percentile in our data) to 37 percent (the 75th percentile).

In supplementary analysis, we isolate whether the role that active IRS monitoring plays in constraining aggressive tax planning hinges on the quality of firm-level governance. This helps triangulate our main result and complements recent evidence on the interplay between governance and taxation (e.g., Desai and Dharmapala 2006). We examine whether governance characteristics—specifically, the equity stakes held by institutional investors, Gompers et al.'s (2003) index, and external auditor choice—shape the link between corporate tax enforcement and avoidance.

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[1] These data are graphically displayed, and the asset-size categories are listed, in Panel A of Figure 1. Our specification assumes that managers form rational expectations about the probability that their firm will be audited with actual IRS audit rates representing unbiased estimates of their unobservable perceptions. Although we initially assume that any errors that managers make in predicting future IRS audit rates are unsystematic (i.e., any deviations from perfect foresight are random), we later relax this assumption by analyzing whether the impact of lagged IRS audit rates on corporate tax avoidance is sensitive to gauging expectations with historical rates. We also examine whether our core evidence holds when we measure IRS monitoring with lead audit rates to reflect that it can take the agency more than a year to finish its investigation. Data constraints force extant cross-country research to measure corporate tax enforcement with indices that are constant over time; e.g., Dyck and Zingales (2004), Haw et al. (2004), Morck and Yeung (2005), and Desai et al. (2007).

[2] However, firms can pursue corporate policies such as acquisitions, divestitures, and share repurchases that materially affect their size. None of our core results are sensitive to confronting this potential source of endogeneity with, for example, instrumental variables and panel data estimations.

Prior research implies that self-dealing managers can exploit complex tax planning, under the pretext that lowering taxes benefits shareholders, to hide their diversionary activities by suppressing information essential for monitoring (e.g., Desai and Dharmapala 2006; Graham and Tucker 2006; Desai et al. 2007). It follows that IRS monitoring will matter more when the firm's own governance structures are lax. In evidence consistent with this perspective, we find that the impact of IRS audit rates on corporate tax avoidance is larger in poorly governed firms. Collectively, our research suggests that IRS monitoring looms large in preventing corporate tax avoidance, particularly when firm-level governance is weaker.

By documenting that tougher IRS monitoring deters firms from pursuing aggressive tax strategies, we contribute to extant research by responding to calls for evidence that helps to explain corporate tax avoidance (Shackelford and Shevlin 2001; Graham 2008; Desai and Dharmapala 2010). Our analysis also provides empirical support for theory aimed at understanding the intersection between corporate tax behavior and tax enforcement (i.e., Crocker and Slemrod 2005; Shackelford et al. 2008).[3] We also complement recent evidence that the IRS plays a valuable (nontax) external monitoring role by focusing on *managers'* rather than investors' perceptions. Although it would be premature at this early stage to propose policy prescriptions, such as recommending that the government should devote more resources to corporate tax enforcement, we help fill another void by providing some insight on the tax revenue implications of IRS monitoring (Hanlon and Heitzman 2010).

The rest of this paper is organized as follows. Section II motivates the testable prediction. Section III outlines our research design. Section IV covers our empirical results. Section V concludes.

II. MOTIVATION

In this section, we outline prior research to develop the intuition that strict IRS monitoring lowers corporate tax avoidance. There is considerable theory and empirical evidence that tough IRS enforcement improves *individual* tax compliance (e.g., Allingham and Sandmo 1972; Alm et al. 1992; and Slemrod et al. 2001). However, Cowell (2004) and Kopczuk and Slemrod (2006) stress that extant empirical research on this issue for firms remains scarce.[4]

Dyreng et al. (2008) show that effective tax rates (ETRs) are a choice variable, suggesting that firms can strategically avoid taxes over the long run. Similarly, Mills (1996) and Mills et al. (1998) report that firms that spend more on tax planning enjoy lower tax burdens. At the executive level, prior research documents that annual bonuses (Hanlon et al. 2007), equity-based incentives (Desai and Dharmapala 2006), and the compensation paid to CFOs and CEOs (Rego and Wilson 2010) affect corporate tax aggressiveness. In focusing on activities within corporate tax departments, more recent evidence supports that tax director incentives lead to lower GAAP ETRs, unlike their minimal impact on firms' cash ETRs (Armstrong et al. 2011; Robinson et al. 2010). Moreover, Dyreng et al. (2010) find that individual managers' characteristics affect corporate tax planning. In contrast, we focus on the other side of the equation by providing evidence on whether the government expending more resources on enforcement constrains firms—and their managers—from aggressively reducing taxes. In short, we extend research on the determinants of corporate tax avoidance to include IRS monitoring.

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[3] Although there is extensive evidence on individuals' responses to the severity of tax enforcement, prior research seldom examines its importance to *corporate* tax avoidance. However, there are some exceptions. Most notably, Rice (1992) relies on IRS data to provide evidence that income tax compliance is higher for public firms and firms belonging to regulated industries, and lower for firms located in tax havens. Corroborating that newly public firms experience a permanent increase in tax pressure, Pagano et al. (1998) estimate that these firms pay 2 percent more in taxes the year after their initial public offering, which they attribute to the resulting greater accounting transparency and closer scrutiny from tax authorities constraining firms from eluding taxes.

[4] While prior research supports that rigorous IRS monitoring improves individual tax compliance, this evidence does not necessarily imply that a similar link prevails between IRS enforcement and the firm. Although Slemrod et al. (2001) find that individuals pay more in taxes when they face higher audit probabilities, this result does not hold for their subsample of wealthy individuals, who actually decrease their level of tax compliance given a high probability of audit. Since sophisticated firms may behave more like wealthy individuals than like the average individual, the general finding of a positive relation between tax compliance and IRS auditing may not extend to firms. Accordingly, analyzing firm reactions to variations in IRS monitoring is important in its own right.

In analyzing confidential corporate tax returns, Mills and Sansing (2000) find that the probability that the government will audit a transaction is higher for firms that generate book-tax differences. Prior research (e.g., Mills 1998) and federal government publications (e.g., U.S. Department of the Treasury 1999) suggest that firms can deflect IRS attention by narrowing the gap between their financial reporting and taxable incomes. IRS guidelines specifically instruct their agents to investigate when book income seriously diverges from taxable income, reinforcing both tax advisors' (Cloyd 1995) and corporate managers' (Cloyd et al. 1996) perceptions that conformity leads to lower tax audit costs.

Relevant to our research question, there is some indirect evidence that the threat of IRS monitoring affects firms' financial reporting decisions. In closely dissecting 27 firms sanctioned by the SEC for inflating earnings that were eventually restated, Erickson et al. (2004) estimate that these firms resorted to deliberately overpaying their taxes by 11 cents, on average, to legitimize each dollar of fraudulently exaggerated earnings. Similarly, in large-sample analysis, Lennox et al. (2010) find evidence that the likelihood that firms perpetrate accounting fraud is decreasing in their tax avoidance. Erickson et al. (2004, 388) explain that their results might reflect that managers committing accounting fraud "may willingly have their firms pay taxes on the earnings overstatements to avoid raising the suspicion of savvy investors, the Securities and Exchange Commission (SEC), or the Internal Revenue Service (IRS)."

However, there is evidence running in the opposite direction that casts doubt on the role that the IRS plays in external monitoring. For example, it is somewhat hard to accept that the IRS operates as an information intermediary in the capital markets when it did not identify any of the accounting frauds under study in Erickson et al. (2004), which is consistent with Dyck et al.'s (2010) evidence from a more comprehensive sample. Moreover, Phillips et al. (2003) and Frank et al. (2009) find that financial reporting aggressiveness tends to accompany tax aggressiveness, implying that firms concurrently manage book income upward and taxable income downward. The mixed prior evidence on the impact of tax enforcement on the corporate financial reporting process helps to motivate our analysis on whether firms exploit lax IRS oversight to aggressively lower their taxes.

Firms weigh the marginal benefits and costs stemming from their tax-planning strategies to ensure that they remain optimally aggressive (Scholes et al. 2008). We contribute to extant research by analyzing whether firms consider the severity of IRS monitoring when choosing their level of tax avoidance. An IRS audit can potentially impose nontrivial costs in the form of proposed audit adjustments, fines, penalties, and interest. For example, IRS audit adjustments levied on firms with assets exceeding $250 million (which represent a majority of our sample) totaled $17.9 billion in 2007, indicating that the average large firm had $25.7 million in proposed IRS audit adjustments in that year (IRS 2007). Wilson (2009) estimates that firms had median savings of $66.5 million from their tax shelters, an amount almost eclipsed by the $58 million in interest and penalties that the IRS assessed on these tax shelter deficiencies. It follows that firms should rationally consider IRS audit coverage rates when developing their tax avoidance position evident in their effective tax rates, which capture variation in corporate tax aggressiveness (Armstrong et al. 2012; Dyreng et al. 2010; Robinson et al. 2010).

However, a necessary condition to support that firms weigh IRS monitoring in selecting their tax avoidance level is their familiarity with the agency's enforcement practices. Firms can obtain information about IRS coverage rates in various ways: (1) through budget reports that indicate shifts in IRS funding; (2) news about structural changes in the IRS; (3) hiring former IRS employees; (4) leadership changes at the IRS; (5) changes in financial accounting standards; (6) trends in government revenue; (7) maintaining contact with former employees who currently work at the IRS; (8) formal and informal meetings with IRS officials and employees; (9) talking with peer firms undergoing audits; and (10) accessing historical annual and monthly audit coverage data released by the IRS or organizations that monitor the IRS.[5]

The tax avoidance strategies that firms adopt will likely reflect their understanding of trends in IRS enforcement activities. By optimizing their tax strategy to avoid enforcement actions, firms can lower the costs associated with fines, penalties, and interest that they may incur if caught under-reporting. Additionally, firms can avoid the negative publicity and costly litigation that may ensue from an IRS audit. We examine whether firms experiencing tighter IRS enforcement practice less tax avoidance to enable them to reduce both their probability of getting selected for audit and the costs of that audit if it does occur.

Although it may appear intuitive at first glance that strict IRS monitoring constrains corporate tax avoidance, there are several arguments that call into question whether we will observe this relation. Apart from the reasons already outlined, firms may hesitate to pursue aggressive tax strategies—regardless of the level of enforcement that the IRS imposes—when they are sensitive to the negative publicity that this may attract. Hanlon and Slemrod (2009) explain that firms may deliberately reduce their tax avoidance in order to avoid suffering the political costs that can come with being labeled tax aggressive. For example, Citizens for Tax Justice released a series of high-profile studies demonstrating that many large, profitable U.S. firms pay hardly any taxes. Similarly, Mills et al. (2010) find evidence implying that political costs constrain tax aggressiveness evident in federal contractors limiting their tax avoidance behavior.

We surveyed members of the Tax Executive Institute (TEI) for their insights on the link between corporate tax enforcement and avoidance. We administered this survey (see Appendix A) as a supplement to an ongoing survey by TEI of its membership working for firms with multinational operations and received responses from 50 tax directors (these questions were added near the end of the survey period).[6] Participants were asked, "From your experience, does a company's assessment of a higher probability of tax authority audit lead the company to..." The majority of respondents (59.1 percent) replied "take a less aggressive tax position due to the risk of being challenged," which is consistent with our prediction. However, the rest (40.9 percent) replied that it would either "have no effect on the tax position taken," or "take a more aggressive tax position for expected bargaining purposes." In short, although our sample is small, a large fraction of respondents indicate that their companies do not become less tax aggressive when enforcement is stricter. This reinforces the view that considerable uncertainty surrounds whether corporate tax avoidance varies systematically with IRS monitoring.

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[5] (1) The IRS's proposed and final budgets are matters of public record. As an example, the IRS once commented on the importance of financial resources: "The main reason for the large decline in these closures [tax return audit closures] is the overall drop in examination resources" (Internal Revenue Service 2003). (2) For example, the establishment of the Large and Medium Sized Business (LMSB) division of the IRS or the passage of the Internal Revenue Service Restructuring and Reform Act of 1998 affected IRS corporate audits. (3) In one example, PricewaterhouseCoopers (PwC) issued a press release in 2006 announcing the appointment of a new managing director in their Washington National Tax Service, a 35-year veteran of the IRS's LMSB division (PwC 2006). (4) Mark Everson replacing Charles Rossotti as IRS Commissioner in May 2003 precipitated a major shift in the vision of enforcement at the IRS. "Almost from the first day of Everson's five-year term, his official statements have reflected the belief that tougher enforcement was required to recover the 'many billions of dollars of lost tax revenues'" (TRAC 2005). (5) Blouin et al. (2010, 2–3) highlight that "On May 9, 2007, KPMG surveyed approximately 4,000 webcast participants with the question 'Is FIN 48 likely to increase audits by tax enforcers?' Eighty-nine percent thought it was at least likely." (6) IRS audits are often assumed to increase as government revenues fall; e.g., Associated Press (2009). (7) For example, a former tax director of a *Fortune* 100 firm and former President of the Tax Executives Institute (TEI) was made the director of the IRS's LMSB division (TEI 2003). (8) In the minutes of a formal meeting between the IRS LMSB division and the TEI, it was asserted that communication between the IRS and TEI happens not only through formal meetings, but also through "the many informal contacts with TEI officers and staff and LMSB officials' participation in chapter, regional, and Institute meetings" (TEI 2007). (9) Entities such as the Tax Executives Institute and the Manufacturers Alliance for Productivity and Innovation facilitate such exchanges. (10) The IRS was ordered by the court to release audit coverage data on a monthly basis (*Sue B. Long v. United States IRS*, April 4, 2003).

Further, public enforcement agencies are known to have limited information on both general and firm-specific conditions as well as auditing resources (e.g., Roe and Jackman 2009). Indeed, Hanlon and Heitzman (2010, 12) stress that "It is somewhat hard to believe that financial markets perceive the IRS as competent enough to monitor given the plethora of stories about how the agency has few resources and little talent to match that of corporate tax departments." After merging formerly undisclosed IRS operational audits and appeals data with confidential corporate tax return data, Hanlon et al. (2007) document that the largest firms in their sample with assets exceeding $5 billion had the highest tax deficiency rate at 74 percent. In other words, large firms subject to closer IRS monitoring exhibit more tax noncompliance, implying that IRS scrutiny may be less important. Since institutional realities call into question whether lower corporate tax avoidance accompanies stricter tax enforcement, our analysis helps to empirically resolve whether firms subject to tighter IRS oversight react by undertaking less aggressive tax positions. Consequently, we examine the prediction (stated in the alternative) that corporate tax avoidance subsides when the IRS imposes tougher monitoring:

H1: Firms' tax avoidance, *ceteris paribus*, will decrease with the probability of an IRS audit.

III. RESEARCH DESIGN

Regression Equation

We test our prediction that corporate tax avoidance falls with stricter IRS monitoring by estimating several versions of the following model using ordinary least squares (we suppress subscripts for simplicity):

$$CASH\ ETR = \beta_0 + \beta_1 AUDIT\ PROBABILITY + \acute{\beta}_2 X + \beta_3 INDUSTRY + \beta_4 YEAR + v. \qquad (1)$$

The dependent variable, *CASH ETR*, is cash taxes paid divided by pretax book income after removing the effects of special items (Dyreng et al. 2010). This measure reflects both permanent and temporary book-tax differences, and avoids overstating the current tax expense that arises from the tax benefits on employee stock options. We follow recent research by constraining *CASH ETR* to range between 0 and 1 (Chen et al. 2010; Lisowsky et al. 2011; Dyreng et al. 2008). Since we require all firms to have positive pretax income, any negative values for *CASH ETR* occur when the firm has negative tax expense (presumably a refund).[7]

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[6] Our survey results may suffer from a nonresponse bias. This is especially important because our participants came only from respondents who replied after a second notice from TEI. Using Wallace and Mellor's (1988) method, we detect no major statistical difference in the characteristics of the respondent firms in terms of size, industry, or geography relative to the earlier responders. In a second test, we find that our sample of firms is generally comparable in size and industry to the general population of TEI member firms, except that we have a higher concentration of manufacturing firms. Although we could not identify any obvious bias stemming from this difference, we cannot dismiss that nonresponse bias of unknown severity may afflict our results.

Extant research differs concerning the proper aggregation length of cash ETRs. While Dyreng et al. (2008) suggest that aggregating the measure over several years smoothes transient shocks to the measure, Dyreng et al. (2010) use a one-year measure of *CASH ETR*. We asked TEI survey respondents how long different tax-planning positions/transactions would take to be adjusted if there was a change in assessed tax risk. Survey participants responded that 12 percent of tax positions could be changed within a month, 39.6 percent within six months, 69.2 percent within one year, 91.25 percent within two-to-three years, and 100 percent within three-to-five years, helping to justify using a one-year *CASH ETR* in our setting. However, we later consider whether our core evidence on the prediction in H1 is sensitive to quantifying corporate tax avoidance with a three-year cash ETR.

We rely on a TRAC (2009) report to specify our primary test variable, *AUDIT PROBABILITY*, which proxies for the likelihood that the firm will be subject to an IRS audit. Specifically, we code *AUDIT PROBABILITY* as the number of corporate tax return audits completed in the IRS's fiscal year *t* for an IRS asset size group a, divided by the number of corporate tax returns received in the previous calendar year for the same IRS asset size group a.[8] *AUDIT PROBABILITY* (its numerator) is graphed in Figure 1, Panel A (Panel B). To justify that *AUDIT PROBABILITY* reflects managers' unobservable perceptions, we appeal to rational expectations by assuming that actual IRS audit rates amount to unbiased estimates of their expectations.

In order to obtain some insight to validate whether firms follow IRS monitoring trends, we surveyed members of the Tax Executive Institute (Appendix A). Seventy-two percent of respondents suggested that they assess audit probabilities when making corporate tax decisions. Given that firms apparently track IRS enforcement activities, we next consider the empirical relevance of our proxy for the agency's monitoring. Of survey participants, 93.1 percent reported relying on discussions with a tax advisor to gauge the audit likelihood. Other popular information sources according to the survey were "discussions with peer institutions" (51.7 percent), "discussions with an auditor" (37.9 percent), and "past audit rates disclosed by the tax authority"

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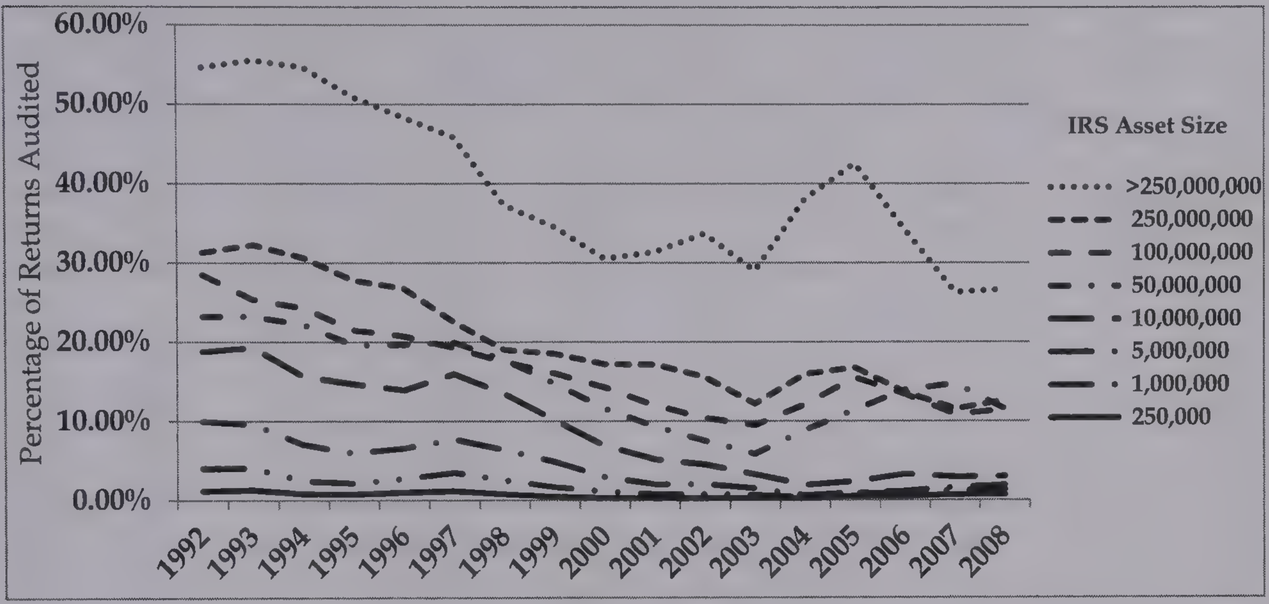
[7] There are multiple measures of tax avoidance/evasion, all of which capture some component of the tax avoidance spectrum (Hanlon and Heitzman 2010; Lisowsky et al. 2011). We primarily focus on a one-year *CASH ETR* in the analysis because it is a broad measure of tax avoidance. We do not have strong priors about which section of the tax avoidance spectrum stricter IRS tax enforcement will affect, and certain other measures are inappropriate for our research question. Book-tax differences (BTDs), for example, assume that book income is not affected by our variable of interest, which seems difficult to justify in our setting (Hanlon et al. 2011). Using BTDs assumes that firms practice nonconforming tax avoidance. Besides since firms participating in tax shelters tend to exhibit large BTDs (Wilson 2009), extensive evidence implies that these amounts reflect earnings management activities and general business conditions; e.g., Mills and Newberry (2001), Philips et al. (2003), Lev and Nissim (2004), and Hanlon (2005). Second, many papers rely on predictive models of tax shelters to identify aggressive tax behavior, especially as measured by the Lisowsky (2010) and Wilson (2009) models. These models are inappropriate for our research question given that they construct the model using tax shelters (listed transactions) that were caught by the IRS (disclosed to the IRS). We do not expect firms to vary the number or type of transactions that they specifically disclose to the IRS in fear of those transactions being audited by the IRS. However, a limitation inherent in gauging tax avoidance with ETRs is that this necessitates excluding firms with negative pretax income, meaning that we cannot generalize our inferences to unprofitable firms. Further, since the TRAC statistics on audit coverage are aggregated across all firms, including those incurring losses, this sample restriction injects noise that almost certainly works against our tests rejecting our null hypothesis.

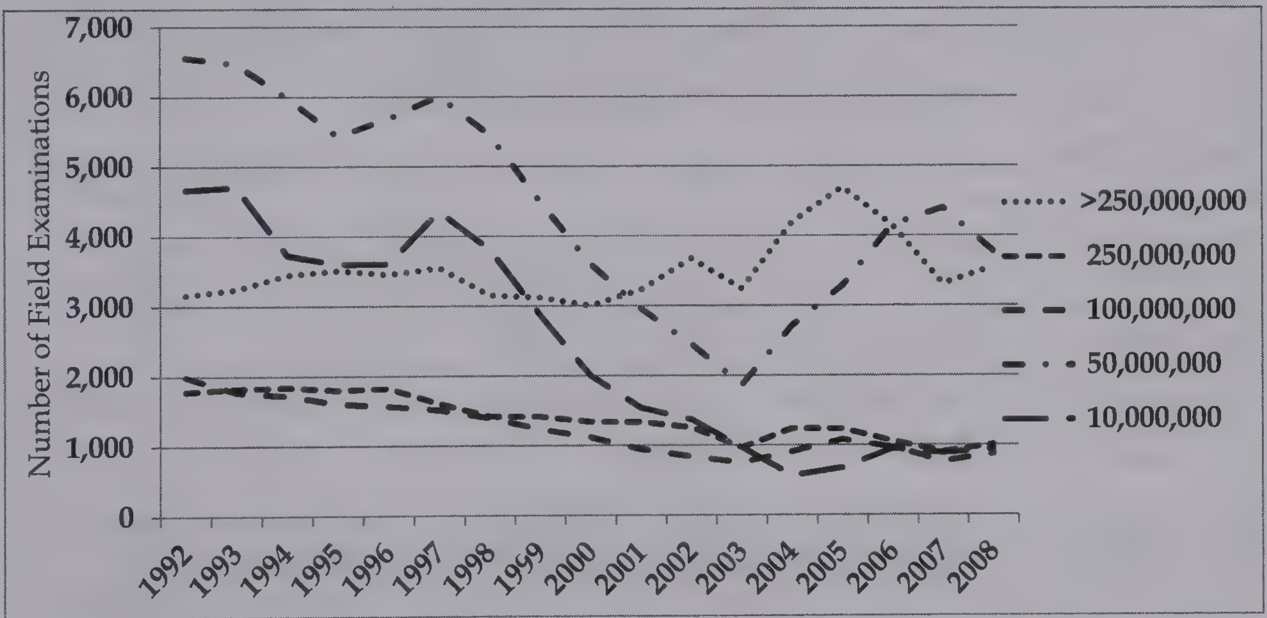
[8] While *AUDIT PROBABILITY* varies only by time and size, this is a function of the IRS aggregating the way they report audit coverage statistics, rather than as a result of the underlying process that determines which firms get audited. The true selection process for IRS audits, which is unknown to managers (and researchers), likely contains firm size as only one of several parameters. These data are an aggregation of data the IRS discloses in its annual Databook, which has been posted to the website of the IRS's Statistics of Income Division.

FIGURE 1
IRS Audit Coverage

Panel A: IRS Audit Coverage for Corporations, by Asset Size, 1992–2008



Panel B: Number of Corporate Audits Completed in IRS Fiscal Year, by Asset Size, 1992–2008



These graphs display the audit probability (Panel A) and the numerator (Panel B) of audit probability, for firms in specific IRS asset classes using data obtained from the Transactional Records Access Clearinghouse. In Panel A, each line represents the proportion of corporate tax audits completed in the given IRS fiscal year *t* for that asset class, divided by the number of corporate tax returns in IRS calendar year *t*–1 for that asset class. In Panel B, the lines represent the number of face-to-face IRS audits conducted each year in each IRS asset size pool. Panel B excludes asset sizes below $10,000,000 in assets, as including them makes the variance in the larger (and more relevant) asset size class groups almost indiscernible.

TABLE 1

Sample Selection

Panel A: Number of Observations for Each IRS Asset Level from Compustat Sample

Year	Less than $250,000	$250,000–$1 Million	$1–$5 Million	$5–$10 Million	$10–$50 Million	$50–$100 Million	$100–$250 Million	$250 Million or More	Total
1992	5	24	148	199	840	507	593	1449	3765
1993	5	14	146	182	895	541	675	1558	4016
1994	3	14	122	190	922	604	769	1783	4407
1995	1	14	107	173	900	611	761	1876	4443
1996	1	12	102	172	987	645	852	2078	4849
1997	1	8	86	143	842	607	874	2145	4706
1998	2	8	63	117	629	537	776	2107	4239
1999	1	7	61	101	564	438	665	2095	3932
2000	3	3	72	92	453	337	565	2035	3560
2001	1	7	58	82	373	286	479	1845	3131
2002	2	5	58	84	359	274	480	1931	3193
2003	1	9	51	79	349	251	481	2058	3279
2004	4	5	36	65	370	269	552	2777	4078
2005	0	9	34	56	329	242	510	2843	4023
2006	2	8	26	45	293	212	485	2873	3944
2007	2	5	24	40	261	189	436	2654	3611
2008	0	3	18	32	227	179	386	2289	3134
Total	34	155	1212	1852	9593	6729	10339	36396	66310

Panel B: Number of Observations Surviving Each Sample Screen

Sample Selection	Firm-Year Observations
Firms with data on assets	173,231
Firms incorporated in the U.S.	143,151
Firms located in a U.S. state	140,489
Firms with positive net income less special items	87,961
Firms with the data required for all control variables	66,310

This table reports the distribution of the sample by calendar year and asset level in Panel A and the screening process applied to assemble the sample in Panel B.

(31 percent). These results suggest that nearly a third of firms rely on historical rates, which is the construct that our primary test variable, *AUDIT PROBABILITY*, is intended to measure.[9]

Table 1, Panel A shows the number of observations in each IRS asset level/year cell, and indicates that the share of sample observations rises almost monotonically with firm size, with very large firms with assets exceeding $250 million, comprising nearly 55 percent of the sample. The observations are fairly evenly spread over time. Importantly, the variation across asset level and over time reinforces that our tests have adequate power to gauge the impact of IRS audit rates on deterring firms from aggressively avoiding taxes, even when we estimate saturated regressions that control for year and firm size with dummy variables. Our identification strategy benefits from *AUDIT PROBABILITY* likely being exogenous since it stems from asset level, year, and their interaction.

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[9] Moreover, the 31 percent frequency may be a lower bound estimate given that firms may indirectly rely on historical audit rates to evaluate tax enforcement severity since insights shared by their tax advisors, external auditors, and peer institutions may be partly based on perceptions of such rates.

The limitations of calibrating IRS monitoring with *AUDIT PROBABILITY* include the fact that this approach assumes that corporate audits are completed in one year. The IRS (2008, 25) supports this assumption by asserting that "In general, examination activity is associated with returns filed in the previous calendar year." We note that audits are closed (and included in *AUDIT PROBABILITY*) when the examination is completed and audit adjustments are proposed, not when the case is finally closed with settlement or the end of litigation. Second, *AUDIT PROBABILTY*'s aggregation using asset size groups treats the probability of audit as a step function of only asset size, while it is most likely a relatively smooth function defined over many different factors. This likely underestimates (overestimates) the probability of audit for firms on the upper (lower) bound of an asset size group. Both of these problems are minimized to the extent that managers are using TRAC-like data to estimate audit probability for their firms.[10] We therefore seek to rigorously control for the role of firm size and time in the analysis since these characteristics dictate *AUDIT PROBABILITY*.

In Equation (1), X stands for a vector of firm-specific controls for size, leverage, capital expenditures, research and development costs, profitability, the presence of, and changes in, tax loss carryforwards, foreign income, and the presence of a Big 4 auditor. The choice and specification of these control variables, which we define in Appendix B, closely resembles recent research on the determinants of corporate tax avoidance (e.g., Dyreng at al. 2008, 2010; Wilson 2009; Chen et al. 2010).[11] We include dummy variables representing the Fama and French (1997) 48 industries given evidence that cash ETRs vary systematically across industries (Dyreng et al. 2008). Besides controlling for changing macroeconomic conditions and tax laws, we include year fixed effects in most estimations since our primary test variable, *AUDIT PROBABILITY*, partly depends on time. In sensitivity analysis, we also explore specifications that impose a linear trend on corporate tax avoidance over time with a variable, *TREND*, that has the value 1 for 1992, 2 for 1993, etc.

Sample Formation

We begin with the 173,231 firm-year observations available on Compustat for 1992–2008, the longest period with IRS audit rate data available from TRAC. We restrict our sample to U.S. incorporated firms (leaving 143,151 observations) that are headquartered in a U.S. state (140,489 observations). Next, the requirement for firms to have positive pretax income before special items reduces the sample to 87,961 observations.[12] Finally, we eliminate firm-years without the data necessary to calculate the control variables, leaving an unbalanced panel consisting of 66,310 observations for 10,626 unique firms. We summarize our sample selection process in Panel B of Table 1.

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[10] Further, all of these limitations suggest that researchers considering using *AUDIT PROBABILTY* need to weigh the limitations of the proxy against the benefit to answering an important research question. Given that our question lies at the heart of firm tax planning and governmental tax enforcement, we believe that relying on this proxy is justified in our setting despite its inherent shortcomings.

[11] Including endogenous right-hand-side variables as controls for factors that affect the budget set for the firm is common practice in accounting research, although it does raise some interpretational concerns. We control for factors that affect the ability or incentives of firms to practice tax avoidance and interpret the coefficient on *AUDIT PROBABILITY* as the impact that the IRS's actions have on corporate tax avoidance. We re-estimate our baseline regression without the firm-specific controls (i.e., containing only industry and year dummy variables) and still find strong support for the prediction in H1 that stricter IRS enforcement is associated with decreased corporate tax avoidance. Moreover, the fact that our evidence persists alleviates concern that our analysis suffers from an omitted variable problem.

[12] We follow extensive prior research by removing loss firms because of the difficulty of interpreting their ETRs, which naturally restricts the generalizability of our inferences to strictly profitable firms. This likely also makes the distribution of *AUDIT PROBABILITY* in our sample different than the universe of U.S. corporations since the probability of suffering a loss likely hinges on firm size (Mills 1998).

Descriptive Statistics

We present descriptive statistics for firms in our sample in Panel A of Table 2, which include that the mean (median) *CASH ETR* at 27 (25) percent is similar to recent research. Panels B and C of Table 2 show that the Pearson pair-wise correlation coefficient between *CASH ETR* and *AUDIT PROBABILITY* is positive and statistically significant at the 1 percent level, and this evidence persists for the alternative ETR proxies listed in this table. As discussed later, the four ETR proxies exhibit positive correlations with each other, but some of these correlations are relatively small, which is likely to reflect these variables capturing different facets of firms' tax avoidance activities, as well as measurement error. These correlations reinforce the importance of triangulating our analysis by specifying different ETR proxies in the regressions.

IV. EMPIRICAL RESULTS

Evidence on the Prediction in H1

In Table 3, we report evidence on the role that IRS monitoring plays in corporate tax avoidance according to Equation (1). In these linear regressions, our inferences reflect standard errors clustered by firm (as a time-dependent variance component will be partially captured by the year fixed effects).[13] The controls vary systematically in the expected directions in our baseline regression in Column (1), except for *FOREIGN INCOME*.[14] More relevant for our purposes, *AUDIT PROBABILITY* loads positively at the 1 percent level, which is consistent with the prediction in H1 since *CASH ETR* is an inverse measure of corporate tax avoidance. Economically, the coefficient estimate of 0.104 implies that firms' cash effective tax rates rise, on average, by 7.3 percent, from 25.5 percent to 27.4 percent, when the likelihood that the firm will be subject to an IRS audit increases from 19 percent (the 25th percentile in our sample) to 37 percent (the 75th percentile). Given our sample average cash taxes paid of $44.1 million, this 7.3 percent increase in *CASH ETR*s would equate to an increase in cash taxes paid by the average firm in our sample of $3.2 million, reinforcing that the impact is economically material.

In the next six regressions, we control for firm size and time in several other ways because these characteristics determine *AUDIT PROBABILITY*. The Column (2) results use nine dummy variables representing the asset deciles to provide the data more flexibility. In this more saturated model that also includes calendar year and industry dummies, we still find that *AUDIT PROBABILITY* loads highly positively in a two-tailed test. Similarly, the evidence supporting the prediction in H1 that firms are less tax aggressive when IRS monitoring is stricter persists when we

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[13] We only cluster at the firm level because the regressions include year fixed effects. Petersen (2009) stresses that cluster size needs to be sufficient to ensure viable estimates using clustered standard errors. It might be difficult to accept that our relatively short timeframe is sufficient to obtain the large-sample properties necessary to justify clustering by year. However, in an untabulated robustness test, we still find that *AUDIT PROBABILITY* loads positively at the 1 percent level when we cluster by both year and firm, consistent with the prediction in H1.

[14] Although controlling for capital expenditures reduces concern that fluctuations in bonus depreciation is spuriously responsible for our results, we continue to find supportive evidence when we re-estimate this regression after discarding observations exceeding the 90th percentile in this variable's distribution.

TABLE 2

Descriptive Statistics

Panel A: Distributional Statistics

Variable	Mean	STD	P25	P50	P75	Obs.
AUDIT PROBABILITY	0.29	0.13	0.19	0.29	0.37	66310
CASH ETR	0.27	0.23	0.08	0.25	0.37	66310
NON-CONFORMING CASH ETR	0.27	0.27	0.06	0.20	0.38	59205
3-YEAR CASH ETR	0.30	0.19	0.18	0.29	0.38	13292
GAAP ETR	0.32	0.16	0.28	0.36	0.39	62531
DISTRICT/SIZE/TIME AUDIT PROBABILITY	0.34	0.19	0.19	0.29	0.44	37631
DISTRICT/TIME AUDIT PROBABILITY	0.02	0.01	0.02	0.02	0.03	37631
Log(ASSETS)	5.82	2.14	4.32	5.79	7.26	66310
Log(SALES)	5.60	2.03	4.17	5.58	6.98	66310
Log(MVE)	5.50	2.17	3.99	5.49	6.98	56563
LEVERAGE	0.22	0.26	0.01	0.15	0.33	66310
INVENTORY INTENSITY	0.15	0.19	0.00	0.08	0.23	66310
R&D INTENSITY	0.02	0.05	0.00	0.00	0.02	66310
CAPITAL EXPENDITURES	0.07	0.09	0.02	0.04	0.08	66310
ROA	0.09	0.09	0.03	0.07	0.13	66310
NOL	0.23	0.42	0.00	0.00	0.00	66310
ΔNOL	0.00	0.07	0.00	0.00	0.00	66310
FOREIGN INCOME	0.01	0.03	0.00	0.00	0.00	66310
BIG FOUR	0.82	0.38	1.00	1.00	1.00	66310

Panel B: Pair-Wise Pearson Correlations of Variables *AUDIT PROBABILITY* to *Log(SALES)*

		1	2	3	4	5	6	7	8	9
1	*AUDIT PROBABILITY*	1								
2	*CASH ETR*	**0.08**	1							
3	*NON-CONFORMING CASH ETR*	**0.03**	**0.57**	1						
4	*3-YEAR CASH ETR*	**0.04**	**0.57**	**0.43**	1					
5	*GAAP ETR*	**0.15**	**0.33**	**0.26**	**0.31**	1				
6	*DISTRICT/SIZE/TIME AUDIT PROBABILITY*	**0.81**	**0.08**	0.00	**0.02**	**0.13**	1			
7	*DISTRICT/TIME AUDIT PROBABILITY*	**0.20**	**0.02**	**0.02**	**0.03**	0.00	**0.23**	1		
8	*Log(ASSETS)*	**0.66**	**0.06**	**−0.03**	−0.03	**0.14**	**0.63**	**−0.07**	1	
9	*Log(SALES)*	**0.61**	**0.08**	**0.01**	**0.01**	**0.19**	**0.60**	**−0.06**	**0.89**	1
10	*Log(MVE)*	**0.58**	**0.05**	**0.04**	−0.02	**0.15**	**0.55**	**−0.05**	**0.89**	**0.86**
11	*LEVERAGE*	**0.19**	**−0.05**	**−0.13**	**−0.08**	**0.05**	**0.16**	**−0.05**	**0.19**	**0.16**
12	*INVENTORY INTENSITY*	**−0.10**	**0.04**	**0.21**	**0.10**	**0.02**	**−0.09**	**0.03**	**−0.22**	**−0.04**
13	*R&D INTENSITY*	**−0.13**	**−0.08**	**−0.06**	**−0.07**	**−0.09**	**−0.14**	**0.04**	**−0.12**	**−0.12**
14	*CAPITAL EXPENDITURES*	0.00	**−0.05**	**−0.06**	**−0.04**	**0.04**	**−0.04**	**0.02**	**−0.09**	**−0.04**
15	*ROA*	**−0.13**	**−0.05**	**0.24**	**0.06**	**−0.01**	**−0.11**	0.01	**−0.19**	**−0.06**
16	*NOL*	**−0.10**	**−0.13**	**−0.14**	**−0.12**	**−0.09**	**−0.09**	−0.01	**−0.05**	0.00
17	*ΔNOL*	**0.08**	**0.07**	**0.05**	**0.04**	**0.10**	**0.07**	**−0.04**	**0.11**	**0.10**
18	*FOREIGN INCOME*	**0.10**	**−0.02**	**0.03**	−0.02	**−0.01**	**0.10**	−0.01	**0.16**	**0.20**
19	*BIG FOUR*	**0.32**	**0.03**	**0.00**	−0.02	**0.11**	**0.20**	0.00	**0.32**	**0.41**

(continued on next page)

TABLE 2 (continued)

Panel C: Pair-Wise Pearson Correlations of Variables *Log*(*MVE*) to *BIG FOUR*

	10	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**
10 *Log*(*MVE*)	**1**									
11 *LEVERAGE*	**0.07**	**1**								
12 *INVENTORY INTENSITY*	**−0.19**	**0.01**	**1**							
13 *R&D INTENSITY*	**0.06**	**−0.19**	**−0.03**	**1**						
14 *CAPITAL EXPENDITURES*	**0.02**	**0.16**	**0.02**	**−0.02**	**1**					
15 *ROA*	**0.07**	**−0.21**	**0.09**	**0.05**	**0.13**	**1**				
16 *NOL*	**−0.02**	**0.02**	0.01	**0.12**	**−0.02**	**−0.05**	**1**			
17 *ΔNOL*	**0.10**	**0.04**	**−0.03**	**−0.02**	**0.01**	**−0.10**	**−0.22**	**1**		
18 *FOREIGN INCOME*	**0.26**	**−0.04**	0.01	**0.16**	**0.01**	**0.16**	**0.10**	**0.03**	**1**	
19 *BIG FOUR*	**0.38**	**0.10**	**−0.02**	**0.08**	**0.07**	**0.02**	**−0.02**	**0.06**	**0.10**	**1**

This table presents distributional statistics and Pearson correlation coefficients between the regression variables for the 66,310 firm-year observations used in the hypotheses tests. Panel A includes the mean, standard deviation, first quartile, median, and third quartile. Panel B reports the Pearson pair-wise correlations in bold indicating statistical significance at the 1 percent level. *AUDIT PROBABILITY* is the number of face-to-face corporate audits completed in IRS fiscal year *t* in a given IRS asset size divided by total number of 1120 returns filed in calendar year *t*−1, in an IRS asset size. *CASH ETR* is cash taxes paid scaled by pretax income. *NON-CONFORMING CASH ETR* is the cash taxes paid scaled by cash flows from operations. The *3-YEAR CASH ETR* is the sum of year *t* through year *t*+*2*'s cash taxes paid scaled by the sum of pretax income over the same time period. *GAAP ETR* is the total income tax expense scaled by pretax income. *DISTRICT/SIZE/TIME AUDIT PROBABILITY* is the number of corporate tax audits completed in IRS fiscal year *t* in a given IRS district for a given IRS asset class group, divided by the number of corporate tax returns received in that same district and IRS asset size group in IRS calendar year *t*−1. *DISTRICT/TIME AUDIT PROBABILITY* is the number of corporate tax audits completed in IRS fiscal year *t* in a given IRS district, divided by the number of corporate tax returns received in that same district in IRS calendar year *t*−1. *Log*(*ASSETS*) is the natural log value of total firm assets. *Log*(*SALES*) is the natural log value of total sales. *Log*(*MVE*) is the natural log value of the market value of equity of the firm. *LEVERAGE* is the total long-term debt scaled by lagged assets. *INVENTORY INTENSITY* is the total inventory scaled by lagged assets. *R&D INTENSITY* is the research and development expense scaled by lagged assets. *CAPITAL EXPENDITURES* is the capital expenditures of the firm, scaled by lagged assets. *ROA* is the pretax income divided by assets. *NOL* is an indicator variable coded 1 if firm has a tax loss carryforward in the prior year, 0 otherwise. *ΔNOL* is the difference between year *t* and year *t*−1's tax loss carryforward, scaled by lagged assets. *FOREIGN INCOME* is the foreign income of the firm scaled by lagged assets. *BIG FOUR* reflects whether the firm appoints a Big 4 auditor in year *t*.

re-specify firm size with the natural logarithms of total sales and the market value of equity in Columns (3) and (4), respectively.[15]

Next, we apply two techniques to address the competing explanation that we are spuriously documenting that *CASH ETR* and *AUDIT PROBABILITY* are both decreasing during our sample period. Although including year fixed effects constrains our estimation to only within-year variance, using a linear trend and Fama-MacBeth regressions helps empirically clarify whether our evidence on the prediction in H1 merely stems from a trend. This examination is especially important given

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[15] Controlling for firm size is necessary given prior research that documents the link between ETRs and firm size and, more importantly, the fact that *AUDIT PROBABILITY* is a function of asset size. However, given that the OLS estimate for the coefficient on *AUDIT PROBABILITY* in the presence of controlling for size will reflect the portion of *AUDIT PROBABILITY* that is not explained by our size control, it begs the question of what is being estimated. First, *AUDIT PROBABILITY* is a step function of asset size, so that as we linearly or log-linearly control for asset size, we are identifying off the difference in functional forms. Further, as we use several alternative size controls in separate regressions, we are also able to identify not only off the differences from the step function to the log-linear control, but also the difference in sales, market equity, or assets. In addition, we estimate (untabulated) the regression in Column (1) of Table 3 without a firm size control, and continue to find empirical support at the 1 percent level for the prediction in H1. Later, we analyze whether our evidence is sensitive to replacing *AUDIT PROBABILITY* with a test variable that does not vary according to firm size.

TABLE 3

Corporate Tax Avoidance (*CASH ETR*) and IRS Monitoring

		Alternative Size Controls			Alternative Time Controls			Panel Data Estimation	
	Baseline (1)	Asset Deciles (2)	*Log*(*SALES*) (3)	*Log*(*MVE*) (4)	Without *AUDIT PROBABILITY* (5)	With *TREND* (6)	*TREND*^{2} (7)	Fixed Effects (8)	Random Effects (9)
AUDIT PROBABILITY	0.104***	0.114***	0.036**	0.028*		0.075***	0.099***	0.098***	0.110***
	(6.19)	(6.30)	(2.27)	(1.68)		(5.31)	(7.45)	(5.67)	(7.56)
SIZE	0.001		0.008***	0.008***	0.006***	0.003**	0.001	0.019***	0.006***
	(1.04)		(6.63)	(7.31)	(7.49)	(2.41)	(1.02)	(9.21)	(5.21)
TREND					−0.002***	−0.001*	0.000		
					(−6.00)	(−1.70)	(1.03)		
LEVERAGE	−0.079***	−0.080***	−0.079***	−0.082***	−0.078***	−0.079***	−0.079***	−0.049***	−0.064***
	(−14.26)	(−14.40)	(−14.24)	(−13.55)	(−14.05)	(−14.27)	(−14.25)	(−9.23)	(−15.11)
INVENTORY INTENSITY	0.034***	0.034***	0.031***	0.059***	0.032***	0.033***	0.035***	−0.007	0.050***
	(4.03)	(4.07)	(3.71)	(6.26)	(3.78)	(3.97)	(4.21)	(−0.60)	(7.45)
R&D INTENSITY	−0.296***	−0.309***	−0.279***	−0.313***	−0.301***	−0.299***	−0.301***	0.116**	−0.172***
	(−9.58)	(−10.08)	(−9.00)	(−9.31)	(−9.74)	(−9.70)	(−9.75)	(2.19)	(−6.67)
CAPITAL EXPENDITURES	−0.077***	−0.088***	−0.073***	−0.078***	−0.061***	−0.061***	−0.057***	0.085***	−0.013
	(−5.60)	(−6.36)	(−5.31)	(−4.95)	(−4.41)	(−4.39)	(−4.15)	(5.86)	(−1.13)
ROA	−0.141***	−0.136***	−0.147***	−0.163***	−0.143***	−0.144***	−0.144***	−0.323***	−0.276***
	(−9.08)	(−8.83)	(−9.50)	(−9.48)	(−9.22)	(−9.24)	(−9.30)	(−23.53)	(−23.39)
NOL	−0.058***	−0.056***	−0.058***	−0.055***	−0.058***	−0.058***	−0.059***	−0.049***	−0.056***
	(−18.18)	(−17.62)	(−18.39)	(−16.69)	(−18.20)	(−18.24)	(−18.51)	(−16.83)	(−22.85)
ΔNOL	0.115***	0.104***	0.113***	0.135***	0.111***	0.110***	0.107***	0.037***	0.056***
	(7.86)	(7.14)	(7.70)	(8.39)	(7.55)	(7.46)	(7.30)	(2.62)	(4.33)
FOREIGN INCOME	−0.064	−0.051	−0.104**	−0.133**	−0.034	−0.043	−0.050	−0.694***	−0.384***
	(−1.28)	(−1.03)	(−2.09)	(−2.54)	(−0.69)	(−0.87)	(−1.01)	(−12.86)	(−8.58)
BIG FOUR	0.017***	0.007*	0.010**	0.008*	0.015***	0.015***	0.017***	0.018***	0.012***
	(4.36)	(1.77)	(2.57)	(1.93)	(3.96)	(4.05)	(4.60)	(4.39)	(3.82)

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TABLE 3 (continued)

	Alternative Size Controls				Alternative Time Controls			Panel Data Estimation	
	Baseline (1)	Asset Deciles (2)	*Log*(*SALES*) (3)	*Log*(*MVE*) (4)	Without *AUDIT PROBABILITY* (5)	With *TREND* (6)	$TREND^2$ (7)	Fixed Effects (8)	Random Effects (9)
Constant	0.317***	0.290***	0.320***	0.347***	0.327***	0.315***	0.309***	0.182***	0.248***
	(30.57)	(26.63)	(30.79)	(21.28)	(33.68)	(31.45)	(31.44)	(16.88)	(41.68)
Year Fixed Effects	Yes	Yes	Yes	Yes	No	No	No	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Asset Decile Fixed Effects	No	Yes	No	No	No	No	No	No	No
Observations	66,310	66,310	66,310	56,563	66,310	66,310	66,310	66,310	66,310
R^2	0.067	0.071	0.069	0.075	0.056	0.056	0.056	0.388	0.044

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*, **, *** Denote two-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

This table presents estimation results from regressing firms' cash effective tax rates on IRS audit rates and controls. Unreported industry controls are based on the Fama and French (1997) industry classification. Beneath each coefficient estimate is reported the t-statistic based on robust standard errors adjusted for clustering by firm. Asset Deciles are indicator variables (unreported) representing the deciles in assets. *TREND* is a linear trend variable, with 1992 equal to 1, 1993 equal to 2, and so on. $TREND^2$ is the linear trend variable, squared.

All other variables are defined in the notes to Table 2, and in Appendix B.

the clearly decreasing trend in audit coverage that is visible in Panel A of Figure 1. Indeed, the probability of audit for the largest firms (asset sizes exceeding $250 million) has declined in the majority of years in our sample (65 percent), with 1993, 2001, 2002, 2004, 2005, and 2007 the only exceptions. Further, the average *CASH ETR* for firms in our sample has fallen in every year except 1995, 1997, 1998, 2004, 2005, 2006, 2007, and 2008. Collectively, this makes the time-size trend in both tax avoidance and audit coverage a largely monotonic decrease, motivating our analysis of this issue.

First, we replace the year dummies with a time trend variable (*TREND*) in the regressions in Columns (5) and (6). Although *TREND* loads negatively at the 1 percent level in Column (5), which excludes *AUDIT PROBABILITY*, when we include *AUDIT PROBABILITY* in Column (6), its negative relation with tax avoidance persists (t-statistic = 5.31). The coefficient magnitude for *TREND* in this regression falls by half—from −0.002 in Model (5) to −0.001 in Model (6). In other words, the descent in corporate tax enforcement between 1992 and 2008 partly explains the concurrent increase in tax avoidance during this period. Additionally, after including a *TREND*-squared variable in Model (7) to investigate the role that nonlinear time trends play, we continue to find that *AUDIT PROBABILITY* loads positively at the 1 percent level. We also apply the Fama-MacBeth estimation technique (untabulated), which enables inference in light of time effects or time-series dependence in the data. Operationally, this involves estimating 17 annual regressions spanning 1992 to 2008 and averaging the coefficients, which also leads to evidence that tough tax enforcement reduces corporate tax avoidance. Our core evidence on the relation between IRS enforcement and corporate tax aggressiveness remains in this Fama-MacBeth analysis, reinforcing that we are not spuriously capturing a time trend.[16]

Finally, in Models (8) and (9), we exploit the panel structure of our data by estimating fixed and random effects models, respectively, to handle firm heterogeneity (rendering industry fixed effects redundant). It is plausible that firms' unobservable, time-invariant characteristics, such as their political influence or state of residence, could affect both the intensity of IRS oversight and their tax avoidance. However, for both panel estimation techniques, the coefficient on *AUDIT PROBABILITY* continues to load positively at the 1 percent level, alleviating concern that omitted firm-specific determinants are spuriously responsible for our evidence on the link between corporate tax enforcement and avoidance.[17] In summary, the evidence reported in Table 3 suggests that higher cash effective tax rates accompany tighter IRS monitoring, although in the next sections we consider whether this inference survives additional sensitivity analysis.

Alternative Tax Avoidance Metrics

In the first three columns of Table 4, Panel A, we evaluate whether the negative relation that we observe between tax avoidance and IRS audit rates in Table 3 holds when we replace *CASH ETR* with alternative proxies. Importantly, Hanlon et al. (2011) document that IRS monitoring affects the financial reporting process. Since the denominator in *CASH ETR* is pretax net income, it may be susceptible to nontax avoidance influences of IRS external monitoring, meaning that we might

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[16] Another concern is the spike in tax enforcement in 2004 and 2005, when firms' ETRs may have been artificially high due to dividends repatriations. Our main results also hold after removing 2004 and 2005 from the analysis.

[17] A Hausman specification test rejects the null hypothesis of no correlation between the unobserved firm-specific random effects and the explanatory variables, implying that random effects estimation is biased, inconsistent, and not the proper design choice for the data. However, we also tabulate the random effects results since Griliches and Hausman (1986) stress that observing consistent estimates across alternative panel data estimation techniques supports the absence of serious errors in variables problems. Although we find consistent results with both fixed and random effects models, it follows that OLS better suits our data given the high between-firm variation in IRS audit rates relative to its within firm variation. Fixed effects may be an inefficient estimation method in such settings according to Beck (2001) and Plumper and Tröger (2007).

TABLE 4

Corporate Tax Avoidance and IRS Monitoring—Sensitivity Analysis

Panel A: Alternative Tax Avoidance and IRS Enforcement Measures

	Alternative Tax Avoidance Measures			Alternative IRS Enforcement Measures	
Dependent Variable	*NON-CONFORMING CASH ETR* (1)	*3-YEAR CASH ETR* (2)	*GAAP ETR* (3)	*CASH ETR* (4)	*CASH ETR* (5)
AUDIT PROBABILITY	0.092***	0.040**	0.109***		
	(4.44)	(2.13)	(8.26)		
DISTRICT/SIZE/TIME AUDIT PROBABILITY				0.034***	
				(3.39)	
DISTRICT/TIME AUDIT PROBABILITY					0.580**
					(2.17)
Lagged *CASH ETR*					
Log(ASSETS)	0.004**	−0.002*	0.007***	0.006***	0.008***
	(2.53)	(−1.66)	(7.19)	(4.87)	(7.95)
LEVERAGE	−0.079***	−0.064***	−0.012***	−0.082***	−0.081***
	(−12.62)	(−9.55)	(−2.58)	(−12.61)	(−12.49)
INVENTORY INTENSITY	0.327***	0.096***	0.010	0.032***	0.031***
	(21.85)	(9.28)	(1.56)	(3.27)	(3.23)
R&D INTENSITY	−0.370***	−0.275***	−0.242***	−0.342***	−0.351***
	(−10.20)	(−6.85)	(−8.53)	(−9.52)	(−9.72)
CAPITAL EXPENDITURE	−0.161***	−0.088***	0.043***	−0.057***	−0.058***
	(−9.63)	(−5.33)	(3.86)	(−3.66)	(−3.73)
ROA	0.717***	0.091***	0.015	−0.117***	−0.118***
	(33.23)	(5.27)	(1.05)	(−6.23)	(−6.25)
NOL	−0.061***	−0.043***	−0.024***	−0.072***	−0.072***
	(−17.20)	(−11.57)	(−8.97)	(−16.71)	(−16.73)
ΔNOL	0.218***	0.085***	0.168***	0.109***	0.110***
	(12.06)	(4.51)	(11.23)	(5.36)	(5.39)
FOREIGN INCOME	−0.126*	0.005	−0.213***	−0.188***	−0.186***
	(−1.89)	(0.09)	(−4.90)	(−2.94)	(−2.92)
BIG FOUR	0.007	0.008*	0.018***	0.027***	0.026***
	(1.43)	(1.83)	(5.50)	(5.14)	(5.06)
Constant	0.182***	0.372***	0.254***	0.303***	0.289***
	(15.22)	(28.03)	(34.89)	(22.86)	(18.84)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	59,205	47,087	62,531	37,631	37,631
R^2	0.165	0.081	0.091	0.066	0.066

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TABLE 4 (continued)

Panel B: Sensitivity Analysis

Dependent Variable	**Excluding CIC Firms** ***CASH ETR*** **(6)**	**IV Estimation** ***CASH ETR*** **(7)**	**Lagged** ***AUDIT PROBABILITY CASH ETR*** **(8)**	**Lagged** ***CASH ETR CASH ETR*** **(9)**	**Tobit** ***CASH ETR*** **(10)**
AUDIT PROBABILITY	0.262***	1.731***	0.073***	0.038***	0.125***
	(3.86)	(11.87)	(4.22)	(2.76)	(8.88)
Lagged *CASH ETR*				0.313***	
				(40.86)	
Log(ASSETS)	0.015***	−0.076***	−0.003**	−0.003***	0.003***
	(4.79)	(−10.94)	(−2.32)	(−2.97)	(3.67)
LEVERAGE	−0.068***	−0.118***	−0.085***	−0.063***	−0.095***
	(−9.00)	(−21.89)	(−12.92)	(−12.12)	(−22.59)
INVENTORY INTENSITY	0.022**	0.071***	0.059***	0.050***	0.049***
	(2.15)	(9.37)	(6.07)	(6.67)	(7.11)
R&D INTENSITY	−0.299***	−0.268***	−0.286***	−0.175***	−0.301***
	(−8.00)	(−11.39)	(−8.10)	(−6.16)	(−12.70)
CAPITAL EXPENDITURE	−0.043***	−0.052***	−0.089***	−0.046***	−0.072***
	(−2.58)	(−4.12)	(−5.42)	(−3.51)	(−5.68)
ROA	−0.139***	−0.141***	−0.162***	−0.155***	−0.117***
	(−7.17)	(−11.72)	(−8.97)	(−10.45)	(−9.64)
NOL	−0.087***	−0.054***	−0.051***	−0.036***	−0.064***
	(−18.45)	(−21.87)	(−14.78)	(−13.16)	(−25.71)
ΔNOL	0.067***	0.070***	0.183***	0.124***	0.128***
	(3.98)	(4.49)	(9.30)	(7.16)	(8.24)
FOREIGN INCOME	−0.006	−0.216***	−0.134***	−0.156***	−0.025
	(−0.06)	(−5.01)	(−2.62)	(−4.08)	(−0.61)
BIG FOUR	0.002	0.014***	0.013***	0.009***	0.021***
	(0.32)	(4.73)	(3.07)	(2.76)	(7.09)
Constant	0.210***	0.077***	0.327***	0.231***	0.288***
	(9.89)	(3.30)	(27.20)	(23.84)	(32.59)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	29,914	66,310	51,684	51,684	66,310
R^2	0.080		0.070	0.153	0.1931

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*, **, *** Denote two-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table presents estimation results from regressing firms' effective tax rates on IRS audit rates and controls. Unreported industry controls are based on the Fama and French (1997) industry classification. Beneath each coefficient estimate is reported the t-statistic based on robust standard errors adjusted for clustering by firm.
All variables are defined in the notes to Table 2, and in Appendix B.

inadvertently capture only a denominator effect. Moreover, *CASH ETR* only reflects nonconforming tax avoidance. If firms practice tax avoidance that simultaneously decreases cash taxes paid and pretax income, then any change in the ETR is purely a mathematical outcome (Hanlon and Heitzman 2010). In order to ensure that our results do not stem from a mechanical denominator effect and to incorporate a measure of conforming tax avoidance into our analysis, we re-estimate the baseline regression using cash flows from operations when calculating the denominator of *NON-CONFORMING CASH ETR* (Hanlon and Heitzman 2010). This variable loads positively (t-statistic = 4.44) in Model (1), providing some assurance that our earlier evidence supporting the prediction in H1 is not an artifact of accounting-based earnings management.

Although we follow Dyreng et al. (2010) by primarily gauging tax avoidance with the one-year average *CASH ETR*, we also conduct a sensitivity test using a three-year average for two reasons. First, Dyreng et al. (2008) stress that a long-run average cash ETR smoothes transient one-year shocks to cash taxes paid or pretax income, providing a more stable measure of corporate tax avoidance. Second, traditional models of tax compliance suggest that the cost to a firm of a tax audit is not only the tax and penalties imposed on underreported income in one year, but also in all years within the statute of limitations. The IRS is apt to investigate additional years when they detect under-reporting in a certain year. Consequently, a longer-term measure of tax avoidance may better suit our research questions if managers select a consistent tax strategy realizing that years are unlikely to be audited in isolation. We calculate *3-YEAR CASH ETR* as the sum of cash taxes paid in years t, $t+1$, and $t+2$, divided by pretax income less special items in years t, $t+1$, and $t+2$. Although there are several upsides to using this specification in our analysis, we pay a heavy price in terms of sample attrition. Nonetheless, we find that *3-YEAR CASH ETR* loads positively at the 5 percent level in Column (2) despite the reduction in power, supporting the intuition that IRS monitoring constrains longer run corporate tax avoidance.

We also examine whether our results are robust to traditional GAAP measures of ETR. After specifying this variable by dividing the firm's total tax expense for the year by its pretax income, we find that *GAAP ETR* loads positively at the 1 percent level in Column (3). Our results (untabulated) also hold when we specify a domestic *GAAP ETR* as the dependent variable.[18]

Alternative IRS Monitoring Measures

In Columns (4) and (5) of Table 4, Panel A, we consider whether our core evidence on the prediction in H1 is sensitive to focusing on alternative proxies for corporate tax enforcement that vary on characteristics besides strictly firm size and time. We rely on *AUDIT PROBABILITY* in our primary analysis because the underlying data cover a longer period, 1992–2008, when tax enforcement levels were intermittently rising and falling. However, in this section, we exploit that, for the shorter 1992–2000 timeframe, the IRS also compiled audit coverage statistics aggregated by IRS district, which enables us to generate another two proxies for tax enforcement that vary according to: (1) geography, firm size, and year; and (2) geography and year. Table 5 displays audit probabilities for two randomly selected IRS districts. Significant cross-sectional variation in corporate tax enforcement is evident at the district level. Importantly, this district-level variation in audit rates varies across firm size and over time, ensuring that a district-level identification strategy

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[18] Our results also hold when using *GAAP ETR* after shortening the estimation period to 1993–2008 to reflect changes in *GAAP ETR*s arising from FAS 109 (now ASC 740-10 and 740-30). There are several problems with using *GAAP ETR*, including that firms may accrue a larger tax cushion in response to increased perceived enforcement (this is specifically forbidden under ASC 740-10), and that this measure is artificially inflated for firms with employee stock options plans (Hanlon and Shevlin 2002). Avoiding these complications is one reason we specify *CASH ETR* as our main proxy. However, to ensure that this sensitivity test is robust to adjusting for the bias created by the tax implications of stock options, we perform several tests. First, we add back the tax benefit created by stock option exercises (the sum of Compustat items txbco and txbcof) to tax expense in our computation of *GAAP ETR* (this test is limited because the disclosure of stock option tax benefits is sparse [Hanlon and Shevlin 2002]). In another specification, we control for the dollar value of options granted as reported by the firm (ExecuComp item OPTION_AWARDS_RPT_VALUE) scaled by assets, and the dollar value of option awards (ExecuComp item OPTION_AWARDS) also scaled by assets. All of these tests yield results that are almost identical to our earlier evidence, reinforcing that option usage by firms is unlikely to be behind our core results in this robustness test.

TABLE 5

Examples of Audit Probabilities for Selected Years and Asset Sizes for Two IRS Districts, and the Percentage Differences between the Two Districts

	Fiscal Year				
Return Type/Asset Class	**2000**	**1998**	**1996**	**1994**	**1992**
Percent of Returns Audited—Pennsylvania District					
< \$250,000 Assets	0.33%	0.86%	1.22%	0.89%	1.13%
\$1 Million < \$5 Million Assets	1.06%	4.35%	7.10%	7.34%	6.24%
\$5 Million < \$10 Million Assets	2.14%	8.04%	16.63%	19.16%	16.27%
\$50 Million < \$100 Million Assets	10.31%	22.19%	28.82%	36.62%	27.78%
\$250 Million Assets or more	22.85%	30.96%	42.73%	48.53%	44.60%
Percent of Returns Audited—Rocky Mountain District					
< \$250,000 Assets	0.26%	0.48%	0.69%	0.66%	1.76%
\$1 Million < \$5 Million Assets	1.80%	3.94%	3.89%	5.54%	10.26%
\$5 Million < \$10 Million Assets	4.66%	8.35%	9.75%	14.64%	19.73%
\$50 Million < \$100 Million Assets	8.48%	12.08%	19.05%	21.58%	33.33%
\$250 Million Assets or more	28.20%	28.64%	36.47%	57.64%	90.27%
Percentage Difference Between Pennsylvania and the Rocky Mountain IRS Districts					
< \$250,000 Assets	21.21%	44.19%	43.44%	25.84%	−55.75%
\$1 Million < \$5 Million Assets	−69.81%	9.43%	45.21%	24.52%	−64.42%
\$5 Million < \$10 Million Assets	−117.76%	−3.86%	41.37%	23.59%	−21.27%
\$50 Million < \$100 Million Assets	17.75%	45.56%	33.90%	41.07%	−19.98%
\$250 Million Assets or more	−23.41%	7.49%	14.65%	−18.77%	−102.40%

These data are taken from the Transactional Records Access Clearinghouse TracFed database. They are representative examples of the Audit Probabilities in two different districts, and the percentage difference between those districts.

is powerful.[19] Accordingly, we rely on the variation in audit rates by IRS district to re-analyze the prediction in H1.

The first of our geography-based enforcement measures, *DISTRICT/SIZE/TIME AUDIT PROBABILITY*, amounts to the number of corporate tax audits completed in IRS fiscal year *t* in a given IRS district for a given IRS asset level, divided by the number of corporate tax returns received in that same district and IRS asset level group in IRS calendar year *t*–1. The second measure, *DISTRICT/TIME AUDIT PROBABILITY*, reflects the number of corporate tax audits completed in IRS fiscal year *t* in a given IRS district, divided by the number of corporate tax returns received in that same district in IRS calendar year *t*–1. This variable does not directly depend on firms' asset level, implying that observing a positive coefficient on *DISTRICT/TIME AUDIT PROBABILITY* would reinforce our earlier analysis indicating that firm size is not spuriously responsible for our core evidence. In the resulting smaller samples, we find that *DISTRICT/SIZE/*

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[19] Although the shorter 1992–2000 interval reduces sample size, integrating geography helps us to address a specific form of potential endogeneity. Prior research (e.g., Loughran and Schultz 2005; Loughran 2007, 2008) argues that corporate location decisions are exogenous since they are driven by proximity to customers, suppliers, and production inputs rather than, in our case, firms' attempts to lower IRS monitoring. Corroborating prior research (e.g., Hilary and Hui 2009; Pirinsky and Wang 2010; El Ghoul et al. 2012), we find that the firms in our sample seldom relocate. We verify in an untabulated regression that our inference on the prediction in H1 using location-based proxies holds when we remove from the analysis the 647 firms (4,430 observations) that moved their business headquarters anytime between 1995 and 2008 (the timeframe for which we can compile time-varying location data from regulatory filings with the SEC).

TIME AUDIT PROBABILITY and *DISTRICT/TIME AUDIT PROBABILITY* load positively at the 1 and 5 percent levels in Columns (4) and (5), respectively.

Another concern is that the largest firms in our sample almost certainly belong to the IRS's Coordinated Industry Case (CIC) program, which routinely subjects them to constant audit (Hanlon et al. 2007). In sharp contrast, our primary test variable assigns the largest firms in our sample with assets exceeding $250 million IRS audit rates that range from 26.29 percent for 2007 to 55.52 percent for 1993. However, it is difficult to reliably identify CIC firms. Gleason and Mills (2002), citing a 1995 IRS publication, suggest that the CIC program includes more than 1,000 firms each year, while Hanlon et al. (2007) estimate that the actual number is closer to 1,200 firms annually. Complicating matters, these estimates of the number of firms in the CIC program include private firms for which data constraints prevent us from including in our analysis. Moreover, inspecting mandatory SEC filings for our entire 1992–2008 study period confirms that firms seldom publicly divulge when they belong to the CIC program; i.e., we could locate fewer than 10 cases of such disclosures.

To tackle the issue that our results may be driven by the large firms in the CIC program, we follow Gleason and Mills (2011) and El Ghoul et al. (2011) by isolating whether our evidence on the prediction in H1 holds when we exclude likely CIC firms.[20] Given Hanlon et al.'s (2007) evidence that 94.1 percent of CIC firms have greater than $250 million in assets, we discard all firms that fall into this category. More generally, focusing on the firms with under $250 million in assets, which comprise 45.1 percent of our sample, is also constructive for ensuring that our results reflect pervasive economic phenomena rather than large firms dominating the analysis. Although our sample shrinks materially from 66,310 to 29,914 observations, the coefficient on *AUDIT PROBABILITY* remains positive and highly statistically significant in Table 4, Panel B, Column (6), helping to justify dismissing the presence of large CIC firms as a competing explanation.

We have outlined several limitations of the TRAC data for examining our research question. Although relying on this data source involves several shortcomings, it is publicly available and readily accessible to both researchers and managers alike. While there is a paucity of other data on the incidence of IRS audits, another potential source does exist in the form of the disclosure of such audits by the firms themselves in their public regulatory filings. It should be noted that using this as a method for documenting IRS audits brings some severe problems. Gleason and Mills (2002) find that many firms fail to disclose when they are subject to an IRS audit—even when the amounts under investigation are material for financial reporting purposes—suggesting that disclosure is a choice variable. Additionally, the nature of the data-collection process injects substantial noise into the empirical measure.[21]

To examine our research question using disclosed audit data from regulatory filings, we assemble a sample of all observations that have a 10-K, 8-K, or 10-Q in the EDGAR database. We code an indicator variable, *DISCLOSED IRS AUDIT*, 1 for all firm-years that mention the words "IRS" or "Internal Revenue Service" within ten words of the words "examination," "audit," or "investigation" in their filings. Electronic regulatory filings are only available since 1994, materially shrinking our sample size. We then average this indicator variable over IRS asset size groups and years to specify a variable analogous to *AUDIT PROBABILITY*, which we label *DISCLOSED AUDIT PROBABILITY*. We implement this averaging independent of our sample, so this measure reflects the disclosures of all public firms with available 10-Ks, 8-Ks, and 10-Qs for which we have information on asset size. Using *DISCLOSED IRS AUDIT* as a proxy assumes that managers have perfect foresight on audit likelihood (or at least of disclosing being audited), and plan accordingly. Alternatively, *DISCLOSED AUDIT PROBABILITY* would be appropriate if managers develop their expectations about the IRS audit coverage from conversations with firms of equal size (as our survey evidence suggests is the case).

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[20] Although the IRS explains how CIC membership is determined in the Internal Revenue Manual 4.46.2, data restrictions and differences in consolidation rules prevent us from precisely identifying CIC firms.

[21] Computer programs can successfully detect the mention of phrases related to an IRS audit in a specific 10-K, although mapping this into firm-years is difficult. First, firms disclose audits of prior years. Further, some firms mention that they are not under audit, and the variation in ways they do this makes it difficult for a computer program to reliably eliminate these firms. For example, a December 31, 2009 8-K of Inergy Holdings states "No Company Benefit Plan is under audit or is the subject of an investigation by the IRS."

In a pair of untabulated regressions, we re-estimate Model 1 (Column 1 in Table 3) using *DISCLOSED IRS AUDIT* and *DISCLOSED AUDIT PROBABILITY* to represent tax enforcement and find that these alternative test variables both load positively at the 1 percent level. We observe similar results after excluding potential CIC firms from these two estimations, although the statistical significance is weaker for *DISCLOSED AUDIT PROBABILITY* (one-sided p-value of 0.069). Despite the fact that there are many drawbacks to using disclosed IRS audits to quantify firms' expectations, we continue to find evidence lending support to the prediction in H1.

Other Sensitivity Tests

Next, we consider whether our evidence that IRS monitoring deters tax avoidance is robust to relaxing the assumption of exogeneity of the IRS's auditing decision. For two reasons, our theoretical and empirical treatment of the relation between IRS tax enforcement and corporate tax avoidance to this point assumes that the level of IRS enforcement is exogenously determined. First, our measure of IRS monitoring is the probability that a given firm in a large pool of firms is audited. For its selection of firms to audit, IRS monitoring would be endogenous to corporate tax avoidance only to the extent that one single firm is able to affect the IRS's auditing decisions with respect to the entire pool. Second, there is evidence that the IRS inefficiently allocates resources, and that resources at the IRS are more tied to the political processes that ultimately fund the IRS than by the actual need for IRS tax enforcement.[22] Accordingly, we believe that endogeneity does not pose a serious concern.

In order to confront the threat that this endogenous relationship is spuriously responsible for our inferences, we implement an instrumental variables technique.[23] We specify the number of corporate tax returns filed for the particular asset size class in the previous year as an instrument. Our measure of IRS tax enforcement is the ratio of audits completed in this year in a given IRS asset size class, divided by the number of returns filed in that same asset size class in the previous year. Our choice of instrument relies upon the fact that the variation in *AUDIT PROBABILITY* is induced by both the denominator and numerator in this ratio—variations in actual returns audited by the IRS in each asset class (the numerator, which itself is determined by the IRS and Congress), and variations in the number of returns filed (the denominator, which happens as a result of likely exogenous factors).[24] Our first-stage regression then becomes:

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[22] Beron et al. (1992) make the assertion that IRS resources are exogenously dispersed geographically, which is challenged in Slemrod and Yitzhaki (2002). Bagchi (2012) finds that IRS funding and enforcement levels reflect political forces.

[23] In another natural approach to tackling potential endogeneity stemming from omitted variables, we report in Table 3 that *AUDIT PROBABILITY* continues to load positively at the 1 percent level when we emphasize the panel structure of our data by estimating firm fixed and random effects models. However, an instrumental variable framework is necessary to handle dynamic endogeneity of firm characteristics.

[24] Finding strong and valid instruments is a vexing problem in tax settings, including ours. Some corporate tax avoidance strategies may actually result in variations in the number of tax returns filed. We assume this effect is negligible. Further, we assume that firms (especially the large firms in our sample) do not avoid taxes by merely neglecting to file a tax return. Finally, general economic conditions may both change the need for more government revenue (increasing IRS audit levels) and decrease the number of returns filed. However, to the extent that we include fixed year effects, constraining our estimation to using within-year variation for identification would only bias our results to the extent that economic conditions affected different-sized firms within the same years differently. To the extent that any of these assumptions are violated, our instrument may be invalid, and the results of this test should be interpreted with caution.

$$AUDIT\ PROBABILITY = \beta_0 + \beta_1 RETURNS\ FILED\ IN_{t-1} + \beta_2 X + \beta_3 INDUSTRY + \beta_4 YEAR + \varepsilon. \quad (2)$$

From Equation (2), we extract fitted values of *AUDIT PROBABILITY*, which we label *AUDIT PROBABILITY HAT*. The first-stage regression (untabulated), indicates that the number of returns filed is an adequate instrument.[25] Next, we estimate the following model:

$$CASH\ ETR = \beta_0 + \beta_1 AUDIT\ PROBABILITY\ HAT + \beta_2 X + \beta_3 INDUSTRY + \beta_4 YEAR + \nu. \quad (3)$$

After using the number of tax returns filed in a given asset size class as an instrument, we obtain results consistent with our previous estimations. The coefficient on *AUDIT PROBABILITY HAT*, 1.73, remains statistically significant at the 1 percent level in the second-stage model as tabulated in Table 4, Panel B, Column (7). Although the magnitude of this coefficient is implausibly large, this regression reinforces that our core result on the prediction in H1 persists even after our attempts to correct for endogeneity. However, we stress that we do not consider endogeneity to be a serious concern given that macro-level IRS auditing policy is largely the result of political forces exogenous to the issues that we are analyzing.[26]

We specify contemporaneous IRS audit rates as our primary test variable under the assumption that managers form rational expectations about the likelihood that firms will experience an IRS audit. Naturally, the actual incidence of IRS audit rates only becomes observable to managers with delay. After relaxing the assumption of rational expectations by replacing *AUDIT PROBABILITY* with its one-year lag in the next regression to ensure that historical rates are known to managers before they settle on the aggressiveness of their firms' tax positions, we still find supportive evidence at the 1 percent level in Column (8) of Table 4, Panel B.[27]

Given that prior tax avoiders tend to be current tax avoiders (e.g., Dyreng et al. 2008; Lisowsky 2010), we add one-year lagged cash ETR to the baseline regression in Column (9) of Table 4, Panel B to control for the persistence in tax avoidance. Predictably, the magnitude of the coefficient for *AUDIT PROBABILITY* falls steeply in this estimation, although this test variable, which now carries a substantially different interpretation, remains positive and highly statistically significant. Our core evidence on the prediction in H1 also holds when we include twice-lagged *CASH ETR* in this specification.

Since we restrict *CASH ETR* to lie between 0 and 1, ordinary least squares (OLS) may result in biased estimates. Data inspection reveals that 9 percent of the *CASH ETR* observations have the value 0 and another 3 percent have the value 1. The evidence reported in Column (10) of Table 4, Panel B confirms that our inferences are materially insensitive to estimating our model with double-censored Tobit. Predictably, the Tobit model yields estimates that are very similar to the OLS estimates given the relatively small amount of censoring in the data (Armstrong et al. 2011).

Another potential concern is that the asset size groups underlying *AUDIT PROBABILITY* reflect nominal dollars, which will gradually translate into an increasing number of firms populating the largest bracket with inflation. In particular, this would be a larger concern if the IRS audited a constant number of firms in each bracket; e.g., the top bracket would be subject to less coverage as the denominator of audit coverage (total number of returns filed) increased, but the number of audits stayed the same. To ensure that inflation is not spuriously behind our results, we eliminate all firms that have changed IRS asset-size categories during our sample period, which leaves only 28,682 observations. In this smaller sample, we still find (untabulated) evidence that *AUDIT PROBABILITY* loads positively at the 1 percent level.[28]

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[25] Specifically, the coefficient *RETURNS FILED IN* $_{t-1}$ is significant at all conventional levels, and the model has an F-statistic, 2562, that far exceeds 10, which is a standard threshold for delineating weak instruments; e.g., Stock and Watson (2002). Tabulation of the first-stage regression, as well as other analysis and explanation of our instrumental variable strategy, are available from the authors upon request.

[26] Further, Larcker and Rusticus (2009) and Francis and Lennox (2010) stress that corrections for endogeneity that is only potentially a problem routinely generate estimates that are inferior to those obtained by simply using standard ordinary least squares.

[27] Although this is another way to deal with endogeneity, the impact of *AUDIT PROBABILITY* on *CASH ETR* subsides both statistically and economically as we inject more noise into the analysis by moving away from contemporaneous IRS audit rates. Since the IRS can take more than a year to conclude its audit, we also verify that our evidence holds when we integrate several "lead" test variables into the ETR estimations.

Next, large changes in *CASH ETR* might be caused by actual payments to the IRS (and not by decreased avoidance in anticipation of IRS audits). However, this possibility is difficult to rule out. In an attempt to investigate this issue, we first estimate our model on the sample of firms that never mention an IRS settlement in their 10-K, 10-Q, or 8-K filings made with the SEC during our entire sample period (firms with the words "IRS" or "Internal Revenue Service" within ten words of "settlement," a sample that admittedly relies upon firms' disclosure decisions). Although this translates into eliminating 16,508 observations for firms that mention IRS settlements, our main results estimated in Model 1 (in Column 1 of Table 3) still remain.

We also manually inspect the 10-Ks of 50 random firm-years with the largest increases in *CASH ETR* for evidence that these firms made recent settlements with the IRS (i.e., firms that had a *CASH ETR* increase exceeding 10,000 basis points). Of the 50 firm-years, we found that eight firms specifically mention IRS settlements, confirming the importance of our analysis that excludes firms that have ever mentioned an IRS settlement. Our analysis suggests that *CASH ETR*s can drastically change for a variety of reasons, rather than stemming from a single factor.

Isolating Different Portions of the Distribution

Next, we consider whether the role that strict IRS monitoring plays in constraining corporate tax avoidance varies across the distribution of cash ETRs. This involves sharpening the analysis by estimating a simultaneous quantile regression to estimate the impact of *AUDIT PROBABILITY* at the 5th, 10th,. . .,90th, 95th *CASH ETR* quantiles.[29] IRS audit rates may be almost irrelevant to firms near the extremes of the *CASH ETR* distribution. In contrast to firms undertaking tax positions that are very aggressive (meaning that they fully anticipate close IRS scrutiny, and will potentially have lower cash ETRs) or very conservative (meaning that they have nothing to hide, and will potentially have higher cash ETRs), firms toward the middle of this distribution may be more sensitive to corporate tax enforcement. The quantile regression results that we summarize in Figure 2 lend support to this intuition; i.e., we observe that IRS monitoring has a larger impact both economically and statistically for firms between these extremes. This quantile regression analysis does allow other interpretations (e.g., firms near the left end of the *CASH ETR* distribution may simply be those experiencing persistent losses), although it does empirically clarify that the positive relation between IRS monitoring and tax avoidance holds across the vast majority of the distribution.

Supplementary Analysis: The Role of Cross-Sectional Variation in Corporate Governance

Next, we evaluate whether the impact of close IRS monitoring on disciplining firms intensifies when their corporate governance is relatively poor. Some research suggests that managers rely on the complexity inherent in sophisticated strategies for reducing corporate tax obligations—which

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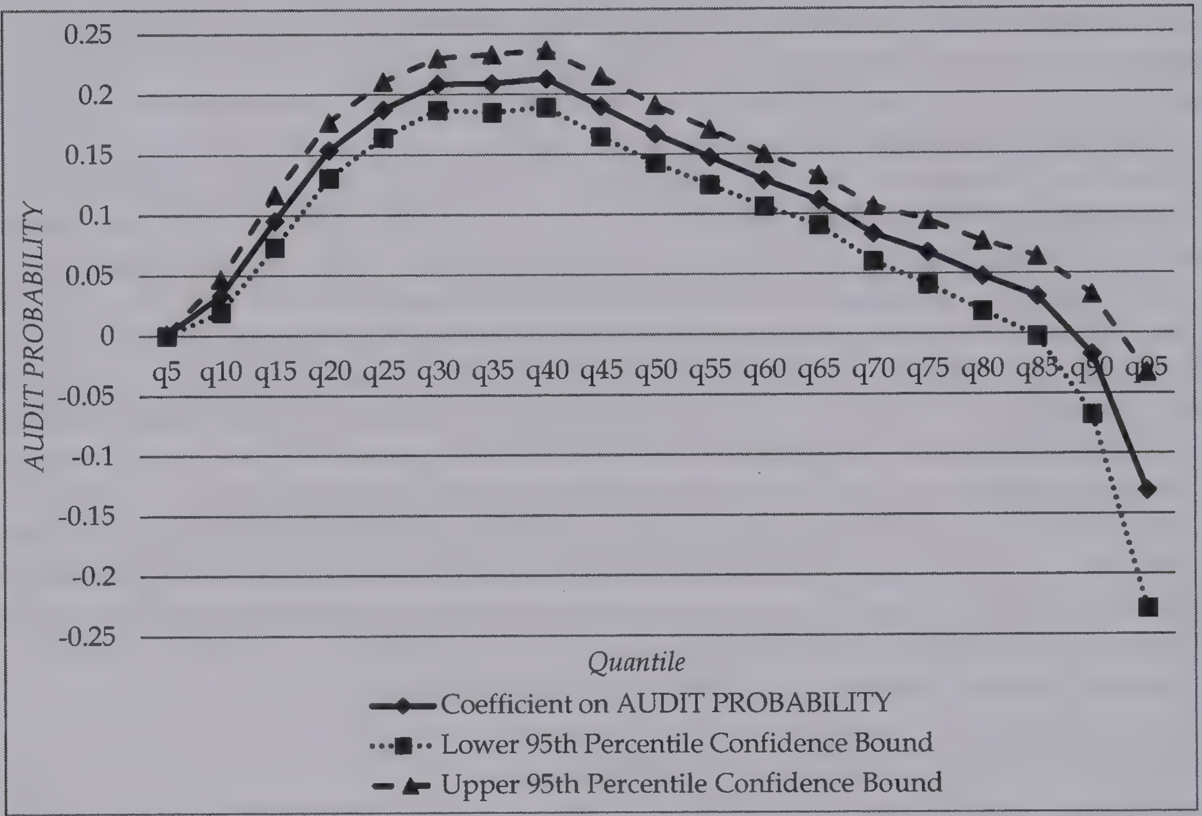
[28] Removing firms that have changed size categories disproportionately excludes small firms from the analysis.

[29] A quantile regression estimates the effect at the Xth percentile of the distribution by minimizing the asymmetrically weighted sum of absolute residuals, in contrast to ordinary least squares regression, which estimates the effect at the mean of the distribution by minimizing the sum of squared residuals. See Koenker and Hallock (2001) for a review of work using quantile regressions.

FIGURE 2
Effects of IRS Tax Enforcement on Different Quantiles of *CASH ETR*



This figure depicts the quantile estimates obtained from estimating this equation:

$$CASH\ ETR = \beta_0 + \beta_1 AUDIT\ PROBABILITY + \beta_2 X + Year\ Fixed\ Effects + \varepsilon_{it},$$

on the 5th, 10th, 15th, 20th, 25th, 30th, 35th, 40th, 45th, 50th, 55th, 60th, 65th, 70th, 75th, 80th, 85th, 90th, and 95th quantiles of the distribution of *CASH ETR*. Coefficient values are on the Y-axis and percentiles of the distribution are on the X-axis. The upper and lower 95th percentile confidence intervals surround the coefficient estimates. *AUDIT PROBABILITY* is the number of face-to-face corporate audits completed in IRS fiscal year *t* in a given IRS asset-size category divided by total number of 1120 returns filed in calendar year *t*–1, in an IRS asset-size category. *CASH ETR* is cash taxes paid scaled by pretax income. *X* represents a vector of control variables that are defined in Appendix B: *Log*(*ASSETS*), *LEVERAGE*, *INVENTORY INTENSITY*, *R&D INTENSITY*, *CAPITAL EXPENDITURES*, *ROA*, *NOL*, *ΔNOL*, *FOREIGN INCOME*, and *BIG FOUR*.

they can justify under the guise that lowering taxes benefits shareholders as the residual claimants—to cover their tracks when they are diverting resources from the firms that they run (e.g., Slemrod 2004; Desai 2005). This agency narrative is grounded in the complementarities between corporate tax avoidance and managers' diversionary activities. More recent evidence implies that investors' reactions to managers' attempts to minimize taxes vary across firms according to corporate governance quality (e.g., Desai and Dharmapala 2006, 2009; Guedhami and Pittman 2008; Wilson 2009). However, in the opposite direction, Rego and Wilson (2010) and Seidman and Stomberg (2011) provide evidence that casts some doubt on whether tax sheltering and managerial rent extraction are actually complementary activities.

In extending this line of research, we analyze managers' perceptions by isolating whether the role of external monitoring by the IRS in constraining tax avoidance turns on firm-level governance. Although recent theory (e.g., Desai et al. 2007) and evidence (e.g., El Ghoul et al. 2011) suggest that strict tax enforcement narrows the scope for managers to deprive outside investors by siphoning resources, it follows that firms' own governance institutions will mediate the link between IRS monitoring and corporate tax avoidance. In other words, weak internal governance renders firms more vulnerable to managerial self-dealing hidden through aggressive tax planning, translating into IRS monitoring playing a larger role in reducing tax avoidance in these situations. Consequently, we add a conditioning variable, *GOVERNANCE*, and its interaction with *AUDIT PROBABILITY* to Equation (1) to examine whether the impact of IRS audit rates on constraining aggressive tax planning is concentrated in poorly governed firms.

In successive regressions, this involves focusing on four firm-level governance institutions. After Desai and Dharmapala (2006), El Ghoul et al. (2011), and Kim et al. (2011), we initially gauge *GOVERNANCE* with the index that Gompers et al. (2003) compile according to a count of the presence of 24 anti-takeover provisions that reflect discipline from the market for corporate control. Higher scores indicate when the market for corporate control is more marginalized since defensive tactics entrench incumbent managers. In these cases, governance is worse since the firm becomes less likely to experience an unsolicited takeover bid when the number of provisions is higher. Given the cardinal properties of Gompers et al.'s (2003) index (Cremers and Nair 2005; Larcker et al. 2007), we construct a dummy variable, *HIGH G-INDEX*, that takes the value of 1 when the firm is above the median, and 0 otherwise. We also measure *GOVERNANCE* with the presence of a Big 4 auditor, *BIG FOUR*, known to more tightly constrain managers' discretion over the financial reporting process according to extensive prior research (Francis 2004).[30]

In our next pair of conditioning variables, we shift our focus from external governance to internal governance. We specify our third proxy for *GOVERNANCE* as the fraction of the firm that is owned by institutional investors, which we label *INSTITUTIONAL HOLDINGS*, after Desai and Dharmapala (2006), El Ghoul et al. (2011), and Kim et al. (2011).[31] Isolating institutional investors enables cleaner analysis since other blockholders may be the very corporate insiders who require monitoring. Institutional investors are eager to protect their interests by actively monitoring managers against implementing policies that undermine long-term firm value.[32] We expect to observe that the importance of IRS audit rates to corporate tax avoidance falls when institutional investors hold larger equity positions.

Finally, we sharpen the analysis of the role that institutional shareholders play in moderating the link between corporate tax avoidance and enforcement by separately examining dedicated institutional owners, which we label *DEDICATED INSTITUTIONAL HOLDINGS*, which have even stronger incentives to closely discipline managers toward pursuing long-term value maximization. Recent evidence implying that dedicated, long-term institutions more actively monitor firms helps validate our focus on these investors (e.g., Hartzell and Starks 2003; Chen et al. 2007).[33] We expect that firms with a relatively large number of dedicated institutional owners holding larger equity stakes have better governance characteristics, which, in turn, substitutes for IRS monitoring.

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[30] We concede that endogeneity in the forms of selection by Big 4 auditors and screening by their clients (e.g., Guedhami et al., 2009) can affect auditor choice. Further, given that auditors can provide tax-related services to their clients, and services provided by a high-quality auditor may be more defensible before the IRS, this may provide an alternative explanation for the results reported in this section related to auditor choice.

[31] There is some evidence that Gompers et al.'s (2003) index that reflects exposure to the market for corporate control complements internal governance through institutional block ownership (Cremers and Nair 2005). However, we concede that there may be links between our conditioning variables; e.g., large institutional shareholders can discipline managers by facilitating takeovers according to prior theory (Shleifer and Vishny 1986) and evidence (Ivashina et al. 2004). Since Gompers et al.'s (2003) index is quite stationary over time and 1998 has the most comprehensive coverage, we also follow Desai and Dharmapala (2006) by using the 1998 version in our analysis. We calculate institutional equity stakes with data from Schedule 13F that firms file with the SEC, which we extract from the Thomson-Reuters Institutional Holdings (13F) database.

[32] It is important to stress at this stage that aggressive tax avoidance may be a positive net present value (NPV) project, although activist institutions may conclude that self-dealing managers will exploit complex tax planning to provide cover when they are denying shareholders by diverting corporate resources.

In Table 6, we report our least-squares estimation results for this analysis. Given that *G-INDEX* reflects worse governance, we expect to observe a positive relation with *CASH ETR* when we interact this conditioning variable with *AUDIT PROBABILITY*. In contrast, since we code our other firm-level governance proxies, *BIG FOUR*, *INSTITUTIONAL HOLDINGS*, and *DEDICATED INSTITUTIONAL HOLDINGS*, so that higher values reflect better governance, we expect a negative relation for their interactions with *AUDIT PROBABILITY*.

The interaction involving *HIGH G-INDEX*, which identifies firms suffering relatively poor discipline from the market for corporate control, loads positively at the 5 percent level in a two-tailed test in Column (1), consistent with IRS monitoring having a stronger impact on deterring corporate tax avoidance in these situations. We follow Desai and Dharmapala (2006) and El Ghoul et al. (2011), among others, in specifying *HIGH G-INDEX* after bisecting the sample according to the count of anti-takeover provisions given the cardinal properties of the underlying data. This evidence persists when we analyze the continuous version of Gompers et al.'s (2003) index, which is allowed to vary by firm-year, in Column (2). Additionally, we continue to find supportive evidence (at the 1 percent level) in Column (3) where we consider firms well governed when they appoint a Big 4 auditor.

In Column (4), we find that the interaction given by the product of *INSTITUTIONAL HOLDINGS* and *AUDIT PROBABILITY* enters negatively at the 1 percent level. This evidence reinforces the intuition that corporate tax avoidance becomes more sensitive to IRS audit rates in the absence of tough external monitoring by institutional shareholders. Similarly, after narrowing the focus in Column (5) to the equity stakes held by dedicated institutional shareholders, the interaction term remains negative and highly statistically significant. In untabulated analysis, the results in Columns (4) and (5) persist if we code the two institutional holdings variables as binary variables. Collectively, the results in Table 6 suggest that the importance of IRS monitoring to constraining firms from undertaking aggressive tax positions rises when firm-level governance is relatively lax, corroborating recent research on the complementarities between managers pursuing aggressive tax planning and their diversionary practices (e.g., Desai and Dharmapala 2006, 2009; Desai et al. 2007; Guedhami and Pittman 2008).

Although our evidence supports recent theory on the interplay between firm-level governance and aggressive tax planning, there are certainly caveats to this finding. Both defining and measuring the type of governance we expect to shape tax planning across firms is difficult, reinforcing that our tests should be interpreted with caution. Moreover, firms' governance reflects their decisions, making it hard to interpret this choice from the standpoint of firm value maximization. Finally, some recent evidence calls into question the empirical relevance of Desai and Dharmapala's (2006) stylized theory. For example, Rego and Wilson (2010) find some results that are hard to square with this theory. Moreover, Seidman and Stomberg (2011) report evidence inconsistent with Desai and Dharmapala's (2006) conclusion that tax sheltering and managers' diversion of corporate resources are complements. Extant research would benefit from additional evidence that helps to clarify whether corporate tax avoidance and managerial rent extraction are complementary activities in practice for a broad sample of U.S. public firms.

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[33] In contrast, institutions striving to exploit short-lived private information tend to have shorter investment horizons and trade more frequently (e.g., Yan and Zhang 2009; Baik et al. 2010). Moreover, Bushee (1998) documents that the presence of transient institutional shareholders leads to value-destroying activities that increase stock prices only over the short term. He relies on portfolio turnover and concentration measures to sort institutions into dedicated, quasi-indexer, and transient investor categories. We use this classification scheme, which Professor Bushee generously provides on his website.

TABLE 6

Corporate Tax Avoidance and IRS Monitoring—The Role of Firm-Level Governance

Governance Variable	*HIGH G-INDEX* (1)	*G-INDEX* (2)	*BIG FOUR* (3)	*INSTITUTIONAL HOLDINGS* (4)	*DEDICATED INSTITUTIONAL HOLDINGS* (5)
AUDIT PROBABILITY	0.095**	0.002	0.186***	0.149***	0.131***
	(2.57)	(0.02)	(7.18)	(7.59)	(7.40)
GOVERNANCE	−0.030*	−0.007**	0.037***	0.041***	0.240***
	(−1.88)	(−2.19)	(5.60)	(3.67)	(5.72)
*AUDIT PROBABILITY * GOVERNANCE*	0.078** (2.03)	0.019** (2.43)	−0.096*** (−4.20)	−0.148*** (−4.78)	−0.646*** (−5.14)
Log(ASSETS)	−0.007***	−0.005**	0.001	0.001	0.001
	(−3.48)	(−2.40)	(0.86)	(0.69)	(0.53)
LEVERAGE	−0.065***	−0.096***	−0.078***	−0.079***	−0.079***
	(−4.60)	(−6.70)	(−14.13)	(−14.27)	(−14.15)
INVENTORY INTENSITY	0.038*	0.041*	0.034***	0.035***	0.035***
	(1.73)	(1.88)	(4.06)	(4.16)	(4.17)
R&D INTENSITY	−0.310***	−0.251***	−0.299***	−0.298***	−0.303***
	(−4.61)	(−3.61)	(−9.69)	(−9.69)	(−9.81)
CAPITAL EXPENDITURES	−0.182*** (−5.24)	−0.186*** (−4.69)	−0.079*** (−5.73)	−0.078*** (−5.61)	−0.078*** (−5.64)
ROA	−0.122***	−0.110***	−0.139***	−0.140***	−0.143***
	(−3.68)	(−2.94)	(−8.97)	(−8.98)	(−9.21)
NOL	−0.029***	−0.024***	−0.057***	−0.057***	−0.057***
	(−4.90)	(−4.11)	(−18.07)	(−18.05)	(−18.13)
ΔNOL	0.204***	0.061	0.113***	0.114***	0.113***
	(4.07)	(1.02)	(7.73)	(7.79)	(7.74)
FOREIGN INCOME	−0.152**	−0.118	−0.062	−0.051	−0.059
	(−2.10)	(−1.61)	(−1.24)	(−1.01)	(−1.18)
BIG FOUR	0.012	0.009		0.016***	0.015***
	(1.17)	(0.82)		(4.04)	(3.95)
Constant	0.366***	0.480***	0.298***	0.302***	0.311***
	(11.02)	(7.49)	(25.59)	(27.28)	(29.33)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	14,585	8,943	66,310	66,310	66,310
R^2	0.065	0.084	0.067	0.068	0.068

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*, **, *** Denote two-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table presents estimation results from regressing firms' effective tax rates on IRS audit rates, firm-level governance, and controls. Unreported industry controls are based on the Fama and French (1997) industry classification. Beneath each coefficient estimate is reported the t-statistic based on robust standard errors adjusted for clustering by firm. *HIGH G-INDEX* is an indicator variable coded 1 if the firm has an above median value of the Gompers Index Score for firm *i* in 1998. *G-INDEX* is the Gompers Index Score for firm *i* in year *t*. *BIG FOUR* is an indicator variable coded 1 if the firm was audited by a Big 4 auditor in year *t*, and 0 otherwise. *INSTITUTIONAL HOLDINGS* is the portion of the firm held by institutional investors in year *t*. *DEDICATED INSTITUTIONAL HOLDINGS* is the portion of the firm held by dedicated institutional investors in year *t*.
All other variables are defined in the notes to Table 2, and in Appendix B.

V. CONCLUSIONS

We examine whether strict IRS monitoring constrains corporate tax avoidance evident in firms' effective tax rates. In regressions that control for firm size, time, and other determinants, we report robust evidence that firms exhibit less tax avoidance when the IRS imposes tougher enforcement. Our coefficient estimates suggest that raising the probability that the firm will experience an IRS audit from 19 percent (the 25th percentile in our data) to 37 percent (the 75th percentile) is associated with firms' cash effective tax rates increasing by, on average, 7 percent, implying that the importance of IRS monitoring is economically large. In supplementary analysis, we report evidence supporting that the impact of IRS monitoring on corporate tax avoidance intensifies when firm-level governance is worse.

Our research suggests that increases in IRS monitoring decreases corporate tax avoidance, which would stimulate increases in tax revenue. However, the policy implications of this result require careful interpretation. An optimal tax enforcement policy does not necessarily maximize tax revenue net of collection costs, but rather it maximizes societal welfare (assuming that societal welfare maximization defines optimality, a standard assumption in economics that is philosophically controversial; see Mankiw and Weinzierl 2010). Since tax enforcement consumes real resources and produces nothing—rather, it merely transfers private wealth to the state—the optimal enforcement policy often entails less enforcement than would be implied by maximizing tax revenue (Slemrod and Yitzhaki 2002). Moreover, simply increasing the number of audits performed by the IRS may not constitute optimal tax policy given that less costly methods of inducing taxpayer compliance may be available such as promoting cooperation between taxpayers and the IRS (Beck and Lisowsky 2011), requiring more corporate tax disclosure (Hasegawa et al. 2012), and raising the penalty for committing tax evasion (e.g., Becker 1978; Allingham and Sandmo 1972).

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APPENDIX A

EXPLANATION OF SURVEY ANALYSIS

In December 2010, we surveyed a portion of the membership of the Tax Executive Institute. A survey was already being administered in order to understand the behavior of multinational firms, and several questions were added as a supplement to that survey. These additional questions were appended to the original survey after it had been sent to the membership by TEI via email 42 days prior, but before a second email was sent to the membership by TEI to ask for their response. After our supplementary questions were added, we received responses from 50 tax directors.[34] We list the questions and responses below:

1. When trying to make tax decisions, do you assess the probability of being audited by a tax authority? (n = 50)

   Yes: 72%
   No: 28%

2. When trying to assess the probability of audit by a tax authority, which of the following sources of information about the tax authority's audit intensity have you used? (Choose as many as apply). (This question was only posed to the 72 percent who answered yes to question 1.) (n = 29)

   Discussions with tax advisor: 93.1%
   Discussions with peer institutions: 51.7%
   Discussions with auditor: 37.9%
   Past audit rates disclosed by the tax authority: 31%
   TRAC data (IRS audit rates) or equivalent: 3.4%

3. From your experience, does a company's assessment of a higher probability of tax authority audit lead the company to: (n = 44)

   Take a less aggressive tax position due to the risk of being challenged: 59.1%
   Have no effect on the tax position taken: 38.6%
   Take a more aggressive tax position for expected bargaining purposes: 2.3%

4. Has the importance of assessing the tax authority's audit probability when making tax decisions changed in the last 10 years? (n = 38)

   It has become much more important: 10.5%
   It has become more important: 44.7%
   It has not changed: 31.6%
   It has become less important: 13.2%
   It has become much less important: 0.0%

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[34] Both emails were sent to members at 2,700 firms. The original survey was sent 42 days prior to the tax authority supplement, and 172 people had already responded. The response rate for the entire survey period was 8.1 percent, which is comparable to previous surveys of senior executives, including the 9 percent response rate in Graham and Harvey (2001) and 9 percent rate of corporate taxpayers responding to Slemrod and Venkatesch (2002), although it is lower than Graham et al.'s (2011) 26.6 percent response rate.

5. If there is a change in assessed tax risk and a company wanted to alter its plan or position, what percentage of tax plans or positions can be changed? (n = 25)

   Within one month: 12%
   Within six months: 39.6%
   Within one year: 69.2%
   Within two–three years: 91.25%
   Within three–five years: 100%
   Greater than five years: 100%

APPENDIX B
VARIABLE DEFINITIONS

CASH ETR$_{it}$ = cash effective tax rate, cash taxes paid scaled by pretax income (less special items) = $\text{txpd}_{it}/(\text{pi}_{it} - \text{spi}_{it})$;
GAAP ETR$_{it}$ = cash effective tax rate, total tax expense scaled by pretax income = $\text{txt}_{it}/\text{pi}_{it}$;
3-YEAR CASH ETR$_{it}$ = sum of three years cash taxes paid, scaled by the sum of three years of pretax net income = $\sum TXPD_{t,t+1,t+2}/\sum(\text{pi} - \text{spi})_{t,t+1,t+2}$; and
NON-CONFORMING CASH ETR$_{it}$ = cash taxes paid scaled by operating cash flows = $\text{txpd}_{it}/\text{oancf}_{it}$.

Both *CASH ETR* and *GAAP ETR* are set equal to missing if pretax income is negative or zero, and all tax avoidance measures are bound between 0 and 1.

Log(ASSETS)$_{it}$ = ln(Total Assets) = $\ln(\text{at}_{it})$;
Log(MVE)$_{it-1}$ = ln(Market Value of Equity) = $\ln(\text{prcc\_f}_{it-1} * \text{csho}_{it-1})$;
Log(SALES)$_{it}$ = ln(Total Sales) = $\ln(\text{sale}_{it})$;
LEVERAGE$_{it}$ = total long-term debt scaled by lagged assets = $\text{dltt}_{it}/\text{at}_{it-1}$;
INVENTORY INTENSITY$_{it}$ = total inventory scaled by lagged assets = $\text{invt}_{it}/\text{at}_{it-1}$;
R&D INTENSITY$_{it}$ = research and development expense scaled by lagged assets = $\text{xrd}_{it}/\text{sale}_{it}$; if xrd is missing, then rdint is set to 0;
CAPITAL EXPENDITURES$_{it}$ = total capital expenditures scaled by lagged assets = $\text{capx}_{it}/\text{at}_{it-1}$;
ROA$_{it}$ = pretax income divided by assets = $\text{pi}_{it}/\text{at}_{it}$;
NOL$_{it}$ = indicator variable coded 1 if firm has a tax loss carry forward (tlcf < 0), 0 otherwise;
ΔNOL$_{it}$ = difference between last year's NOL carry forward and this year's NOL carry forward, scaled by lagged assets = $(\text{tlcf}_{it} - \text{tlcf}_{it-1})/\text{at}_{it-1}$;
FOREIGN INCOME$_{it}$ = amount of foreign income earning by a firm, scaled by lagged assets = $\text{pifo}_{it}/\text{at}_{it-1}$;
BIG FOUR$_{it}$ = indicator variable coded as 1 if the firm appoints a Big 4 auditor in year *t*, 0 otherwise;
AUDIT PROBABILITY$_{ts}$ = number of face-to-face corporate audits completed in IRS fiscal year *t* in asset class a, divided by total number of 1120s filed in calendar year *t*–1 in asset class a;
HIGH G-INDEX$_{i}$ = indicator variable coded 1 if the firm has an above median value of the Gompers Index Score for firm *i* in 1998, 0 otherwise; the G-Index is the sum of 24 corporate governance rules, higher numbers indicate worst corporate governance, as derived in Gompers et al. (2003);
G-INDEX$_{it}$ = Gompers Index Score for firm *i*; the G-Index is the sum of 24 corporate governance rules, higher numbers indicate worst corporate governance, as derived in Gompers et al. (2003);

$INSTITUTIONAL\ HOLDINGS_{it}$ = percentage of the firm's equity held by institutional investors in year *t*; calculated from data provided in the Thomson-Reuter's Institutional Holdings (13F) Database;

$DEDICATED\ INSTITUTIONAL\ HOLDINGS_{it}$ = percentage of the firm's equity held by dedicated institutional investors in year *t*, classified using Bushee (1998);

$DISTRICT/SIZE/TIME\ AUDIT\ PROBABILITY_{tsd}$ = number of corporate tax audits completed in IRS fiscal year *t* in an IRS district for a IRS asset class group, divided by the number of corporate tax returns in the same district and IRS asset size group in IRS calendar year $t-1$; and

$DISTRICT/TIME\ AUDIT\ PROBABILITY_{ts}$ = number of corporate tax audits completed in IRS fiscal year *t* in a given IRS district, divided by the number of corporate tax returns received in that same district in IRS calendar year $t-1$.

All continuous variables are winsorized at the 1st and 99th percentiles.

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Materiality Judgments and the Resolution of Detected Misstatements: The Role of Managers, Auditors, and Audit Committees

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ABSTRACT: This study investigates how manager and auditor incentives, along with audit committee characteristics, are associated with materiality judgments about detected misstatements. Using data on detected misstatements that occurred between 2003 and 2006, we find auditors' incentives to protect their reputations weaken the effect of managerial incentives associated with the pressure created by analyst following; auditors are less likely to allow managers to waive material misstatements as audit fees increase. Regarding audit committee characteristics, results reveal that audit committees with greater financial expertise are less likely to allow managers to waive material misstatements compared to audit committees with less expertise.

Keywords: *audit committees; audit fees; error correction; materiality; stock analysts.*

Data Availability: *Data used in the study are available from public sources.*

I. INTRODUCTION

Auditors detect and inform client managers and audit committees of misstatements, and these agents must reach agreement about whether managers will correct the misstatements prior to issuing the financial statements. Managers may waive correcting misstatements if auditors and audit committees conclude that the misstatements do not render the financial statements *materially* incorrect. Yet, the Securities and Exchange Commission (SEC) and others have asked the rhetorical question: If a misstatement is immaterial, then why not correct it (Levitt 1998)? Given the absence of bright-line criteria for assessing materiality,

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judgments about resolving misstatements may be strategic to achieve desired financial reporting outcomes (Acito et al. 2009). Analysis of the role of managers, auditors, and audit committees in misstatement materiality judgments is therefore important because it can aid understanding of observed audit and financial reporting outcomes that can affect users. However, such analysis has been difficult to pursue because information on materiality judgments in resolving detected misstatements either has not been publicly observable (Kinney and Libby 2002; Nelson et al. 2002) or has been limited to specific industries or financial issues (e.g., lease accounting as in Acito et al. [2009]).

In this study we make use of regulation concerning the resolution of detected misstatements contained in Staff Accounting Bulletin No. 108 (SAB 108). The implementation of SAB 108 provides disclosure data on detected misstatements that were previously judged immaterial and were not corrected in the financial statements until the release of the new guidance (SEC 2006). We use the SAB 108 disclosures to measure both the qualitative and the quantitative materiality of misstatements during the periods in which they remained uncorrected.[1] Using these disclosures, we test a conceptual model of the manager-auditor-audit committee resolution process for detected misstatements that includes analysis of how qualitative and quantitative materiality judgments about detected misstatements are influenced by managers' incentives, auditors' incentives, and audit committee characteristics.

SAB 108 became effective for annual financial statements covering the first fiscal year-end after November 15, 2006. Prior to SAB 108, the quantitative materiality of detected misstatements was evaluated using either a balance sheet *or* an income statement approach. The income statement approach focuses on the materiality of current-year misstatements and the effect of reversing prior-year misstatements on the income statement, which may allow misstatements to accumulate on the balance sheet. The balance sheet approach focuses on ensuring the year-end balance sheet is correct. Audit firms' client portfolios historically varied in the use of these approaches (Nelson et al. 2005; Keune and Johnstone 2009). In contrast, SAB 108 requires using a "dual approach" (SEC 2006), applying *both* the balance sheet and the income statement approaches. Upon adopting SAB 108, companies with misstatements judged material under the dual approach could correct the prior-period misstatements by recording a one-time cumulative transition adjustment to retained earnings, adjusting current-year earnings, or restating financial statements (SEC 2006). Our sample consists of misstatements that were detected but remained uncorrected prior to the adoption of SAB 108, as they were initially judged immaterial using the materiality and correction guidance in effect at the time of detection.

Before stating our results and contributions, we acknowledge features of our data that limit one's ability to make inferences about audit quality based on SAB 108 misstatement corrections. As we note above, the misstatements in our sample were judged immaterial at the time of detection. Considering the size of these misstatements, we find that while the largest is greater than 50 percent of normal income, the median misstatement is 2 percent of normal income. This implies that some auditors detected relatively small misstatements, and the detection of small misstatements is one indication of high audit quality. Thus, the mere fact that a company corrected and disclosed prior-period misstatements when adopting SAB 108 does not necessarily indicate low audit quality. It is therefore an open question as to whether auditors, managers, and audit committees should be criticized for not correcting the misstatements earlier, or commended for complying with SAB 108 by disclosing and correcting the misstatements. Further, we note that managers cannot waive the correction of a misstatement unless a misstatement exists, and the distribution of misstatements across companies and their auditors may not be random or exogenous. Thus, readers should consider our inferences in light of the fact that our sample only comprises companies for which a misstatement exists. Given these caveats, we are circumspect in our inferences regarding the association between audit quality and SAB 108 misstatement corrections. Instead, we focus our analyses and inferences on the impact of proxies for managers' and auditors' incentives, as well as audit committee characteristics, on our measures of the qualitative and quantitative materiality of waived misstatements.

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[1] During our sample period, determining materiality involves evaluating whether a misstatement will affect the judgments of a "reasonable person" (FASB 1980). The SEC (1999) issued Staff Accounting Bulletin No. 99 (SAB 99), which requires consideration of both quantitative and qualitative characteristics when judging misstatement materiality. Quantitative characteristics concern the size of a misstatement in relation to the overall financial statements and are often based on income statement components (Holstrum and Messier 1982; Brody et al. 2003; Messier et al. 2005). Qualitative characteristics concern the effect of a misstatement in relation to users' decisions. SAB 99 identifies considerations that would cause a quantitatively small misstatement to be qualitatively material, which includes whether the misstatement hides a failure to meet analysts' consensus expectations for the company.

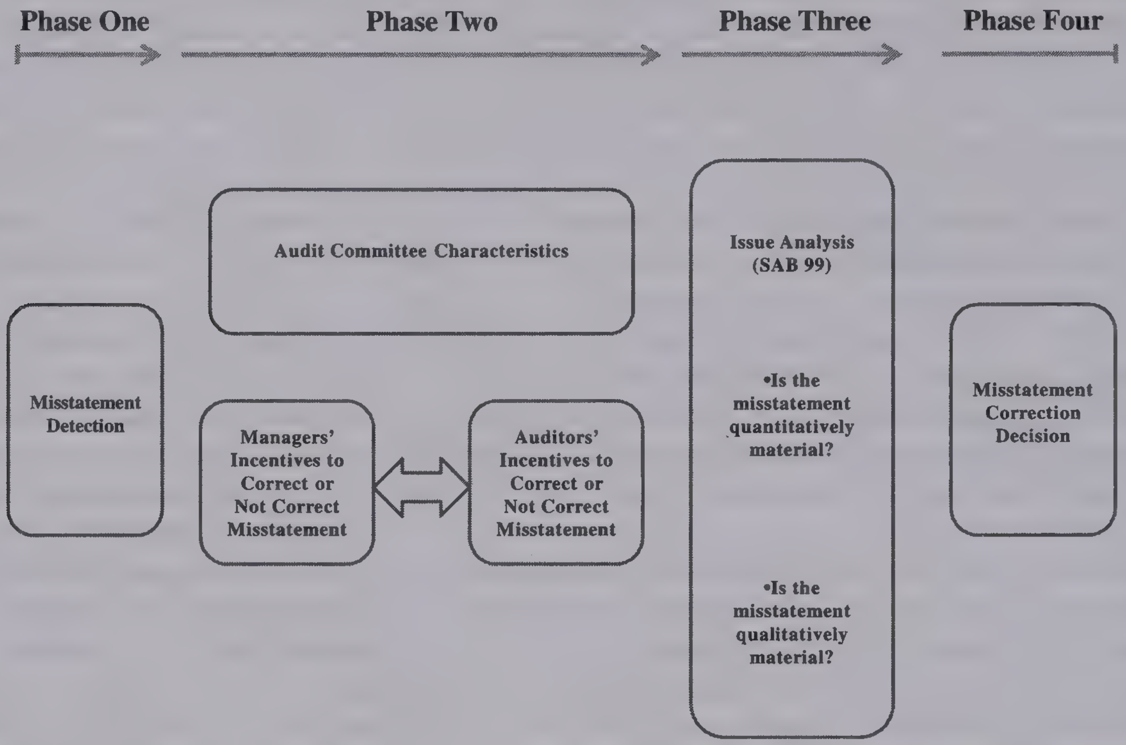
We first test the association between the qualitative characteristics of misstatements and managers' incentives as proxied by analyst following. We construct a unique new variable using SAB 108 misstatement and analysts' forecast data that enables us to identify companies that meet or beat analysts' consensus forecasts by waiving the correction of misstatements. In other words, the qualitatively material misstatements in our sample, if corrected, would have resulted in the respective companies missing analysts' consensus forecasts. We find that managers are generally more likely to waive qualitatively material misstatements as analyst following increases, but this effect is primarily present when audit fees are relatively low. This result extends the materiality literature by illustrating the role of managers' incentives in materiality judgments and the use of materiality justifications to achieve desirable financial reporting outcomes (Acito et al. 2009), as well as the curtailing effect that auditors of high-fee clients have on managers' behavior. Further, by using publicly available data, we complement prior archival (Wright and Wright 1997), survey (Nelson et al. 2002, 2003), and experimental research (Libby and Kinney 2000; Braun 2001; Nelson et al. 2005; DeZoort et al. 2006; Ng 2007; Ng and Tan 2007) on materiality judgments in which earnings management is a concern.

Next, we test the association between audit fees and the likelihood that auditors will allow managers to waive material misstatements. One theory suggests that economic dependence exists between auditors and their clients, with auditors catering to managers' demands (e.g., DeAngelo 1981b; Beck et al. 1988; Magee and Tseng 1990). A competing theory suggests that auditors protect their reputations by rejecting managers' demands when the downside risk to doing so is high (Reynolds and Francis 2001; Larcker and Richardson 2004). Consistent with the theory of auditor reputation protection, we find auditors are less likely to allow managers to waive quantitatively material misstatements as audit fees increase. We also find a negative interaction between audit fees and analyst pressure on the likelihood that auditors will allow managers to waive qualitatively material misstatements. Specifically, auditors' incentives to protect their reputations weaken the effect of managerial incentives associated with the pressure created by analyst following; auditors are less likely to allow managers to waive qualitatively material misstatements as audit fees increase. These results extend the literature on auditor independence in relation to professional services fees. Prior studies examine the relation between such fees and various proxies for audit and financial reporting quality (for instance, accounting accruals, auditors' going-concern reports, and restatements; see, e.g., Reynolds and Francis 2001; Frankel et al. 2002; Geiger and Rama 2003; Kinney et al. 2004; Larcker and Richardson 2004). Our study complements this literature by providing evidence on the association between audit fees and the materiality of waived misstatements.

In our last set of tests, we follow the recommendation of Caskey et al. (2010) to include audit committee characteristics in analyses concerning decisions made by auditors and managers. SAS 89 requires that audit committees be informed of auditors' and managers' judgments about

FIGURE 1

The Manager-Auditor-Audit Committee Resolution Process for Detected Misstatements


Phase One
Phase Two
Phase Three
Phase Four
Audit Committee Characteristics
Misstatement Detection
Managers' Incentives to Correct or Not Correct Misstatement
Auditors' Incentives to Correct or Not Correct Misstatement
Issue Analysis (SAB 99)
•Is the misstatement quantitatively material?
•Is the misstatement qualitatively material?
Misstatement Correction Decision

misstatements. We examine the effect of audit committee expertise on the likelihood of detected material misstatements being waived. We find that audit committees with greater financial expertise are less likely to allow managers to waive qualitatively or quantitatively material misstatements than are audit committees with less expertise. These results extend prior research on the role of audit committee characteristics in judgments (e.g., Abbott et al. 2004).

In Section II we detail the manager-auditor-audit committee resolution process for detected misstatements and describe academic theory underlying our hypotheses. We describe our research design in Section III and present our results in Section IV. Section V concludes.

II. BACKGROUND AND HYPOTHESES

The Manager-Auditor-Audit Committee Resolution Process for Detected Misstatements

Figure 1 depicts a conceptual model that describes the manager-auditor-audit committee resolution process that occurs upon misstatement detection. Managers, auditors, and audit committees begin by evaluating the misstatement (Phase One). These agents' respective incentives and professional characteristics then affect judgments related to the misstatement (Phase Two), including interpretations of professional standards on materiality (Phase Three). These judgments thus affect misstatement correction decisions (Phase Four). This resolution process involves judgment because materiality assessments are not based on simple bright-line thresholds (SEC 1999) and provide auditors with an opportunity to curtail aggressive manager choices during misstatement evaluation (Nelson et al. 2002; Trotman et al. 2005; Sanchez et al. 2007).

Managers' Incentives and the Resolution of Detected Misstatements

Analyst coverage, as well as compensation contracts, create incentives for managers to manage earnings (Levitt 1998; Matsunaga and Park 2001; Brown and Caylor 2005; Burns and Kedia 2006).[2] For example, Graham et al. (2005) report that CFOs work to meet or beat earnings benchmarks to achieve credibility in the capital markets, maintain or increase stock prices, enhance managers' reputations, and convey future corporate growth prospects. Similarly, Dhaliwal et al. (2004) find that managers actively manage earnings to meet analysts' forecasts, and Cheng and Warfield (2005) find a positive association between the number of analysts covering a company and earnings reports that meet or just beat analysts' forecasts. Bartov et al. (2002) provide an explanation for this relation by showing that companies that meet or beat analysts' earnings expectations have higher quarterly returns than companies with similar forecast errors that do not meet expectations. Importantly, their results show that the premium to meeting or beating persists even when achieved via earnings management.

A prominent earnings management benchmark is thus meeting or beating analysts' consensus forecasts (Ashbaugh et al. 2003; Lim and Tan 2008). Indeed, archival research reveals that auditors judge a pattern of just meeting consensus forecasts as the most common indicator of a client's earnings management risk (Bedard and Johnstone 2004). SAB 99 recognizes that a misstatement that hides a failure to meet analysts' consensus forecast can be qualitatively material even if it is quantitatively immaterial. Using SAB 108 data, we identify misstatements that enabled companies to meet or beat analysts' consensus forecasts; if those same misstatements had instead been corrected, the corrections would have resulted in the companies missing analysts' consensus forecasts.[3] Based on this discussion, we predict that the level of analyst following is positively related to the likelihood that managers will waive correcting misstatements that, if corrected, would cause the company to miss its consensus forecast:

> **H1:** Analyst following increases the likelihood that managers will waive material misstatements.

Auditors' Incentives and the Resolution of Detected Misstatements

The theory of economic dependence suggests that auditors are dependent on clients and cater to managers' financial reporting demands to earn future fees (e.g., DeAngelo 1981a, 1981b; Beck et al. 1988; Magee and Tseng 1990; Johnstone et al. 2001; Kinney and Libby 2002). Some research supports this theory (e.g., Frankel et al. 2002; Ashbaugh et al. 2003; Hoitash et al. 2007; Hatfield et al. 2008; Lim and Tan 2008). For example, Libby and Kinney (2000) show that auditors consider a correction of an earnings overstatement to be less likely if it would cause a missed forecast. Ng (2007) reports that auditors are less likely to require correction of a quantitatively immaterial misstatement when doing so would cause the company to miss analysts' forecasts compared to when it would disrupt the prior-year earnings trend, and Ng and Tan (2007) show that auditors are less likely to require correction of a quantitatively immaterial audit difference that affects the company's ability to meet analysts' consensus forecasts when the manager expresses concern about adverse consequences.

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[2] We acknowledge that some prior literature characterizes analysts as external monitors who reduce managers' incentives and ability to manage earnings (e.g., Jensen and Meckling 1976; Healy and Palepu 2001).

[3] We measure and interpret qualitative materiality consistent with experimental studies that examine situations in which waiving a misstatement allows a client to meet or beat analysts' consensus forecasts (e.g., Ng and Tan 2007).

In our setting, the theory of economic dependence suggests that auditors are more likely to allow managers to waive material misstatements as their economic dependence increases, implying a positive relation between audit fees and the likelihood of managers waiving material misstatements. As audit fees increase, auditors face greater future income risk (DeAngelo 1981a, 1981b), which provides managers the incentive to pressure auditors to allow them to waive misstatements (Wright and Wright 1997; Nelson et al. 2002).

While there is support for the theory of economic dependence, other results in the literature are inconsistent with this theory. Braun (2001) shows that auditors' decisions to allow managers to waive a material misstatement are not influenced by the size of a client's audit fees in relation to the local office's total fees, and that auditors are less likely to allow managers to waive a material misstatement when the company or misstatement poses greater auditor litigation risk. Other experimental research finds that auditors are unwilling to allow managers to waive misstatements, even when future audit fees are at risk (Hatfield et al. 2008). Archival research examining auditors' incentives and the meet-or-beat benchmark finds no association between audit fees and companies meeting or beating forecasts (Ashbaugh et al. 2003; Lim and Tan 2008). Taken together, this work implies that auditors' reputational incentives also play a role.

The theory of reputation protection suggests that auditors value their reputations, identify risks posed by their clients, and respond by exerting effort (Bedard and Johnstone 2004) so as to protect their reputations and reduce litigation risk (e.g., Stice 1991; Lys and Watts 1994; Johnstone and Bedard 2001). Some prior research provides evidence supporting this theory (e.g., Reynolds and Francis 2001; Larcker and Richardson 2004; Mitra et al. 2009). In our setting, the theory of reputation protection predicts that auditors are less likely to allow managers to waive material misstatements as the downside risk of reputation loss increases with audit fees, which implies a negative relation between audit fees and the likelihood that a manager waives a material misstatement. Reputation loss associated with relatively high audit fees is especially relevant during our sample period because audit fees are publicly available. Larcker and Richardson (2004, 625) find a negative relation between the level of audit fees and the size of discretionary accruals, which the authors attribute to "auditor behavior being constrained by reputation effects." In essence, as audit fees increase, the auditor experiences greater reputation risk due to increased visibility, which leads to less willingness to allow managers to waive material misstatements. Also consistent with the theory of reputation protection, research shows that auditors require clients with lower financial reporting quality to pay higher audit fees to compensate for the extra time required to perform necessary audit work (Kinney et al. 2004). In our setting, this implies that auditors that charge higher (lower) fees perform more (less) audit work and are less (more) likely to allow managers to waive material misstatements.

Based on these mixed results concerning the effects of auditors' incentives, we make the following nondirectional prediction regarding the association between audit fees and the likelihood that auditors will allow managers to waive material misstatements:

H2: Audit fees affect the likelihood that auditors will allow managers to waive material misstatements.

Of course, we study a setting in which managers decide whether to correct detected misstatements *and* auditors decide whether to agree with managers' decisions. Thus, the joint role of managers' incentives to manage earnings and auditors' incentives to allow managers to manage earnings is important (Antle and Nalebuff 1991; Levitt 1998; Kinney and Libby 2002; Nelson et al. 2002). Prior research examining the use of auditors' materiality judgments to meet or beat analysts' forecasts focuses on the role of analysts' forecasts and managers' incentives, but has not explicitly considered the role of auditors' incentives.

To understand how auditors' and managers' incentives interact, first assume that analyst pressure increases the likelihood that managers will waive material misstatements. If auditors make judgments consistent with reputation protection, then they will be less likely to allow managers to waive material misstatements as audit fees increase (i.e., a negative interaction between audit fees and analyst pressure). However, if auditors make judgments consistent with the theory of economic dependence, they will be more likely to allow managers to waive material misstatements as audit fees increase (i.e., a positive interaction between audit fees and analyst pressure). Given these competing possibilities and the lack of empirical research in this area, we test a nondirectional prediction regarding the interaction between managers' and auditors' incentives and the likelihood that auditors will allow managers to waive correcting misstatements that, if corrected, would cause the company to miss its consensus forecast:

H3: The interaction between analyst following and audit fees affects the likelihood that auditors will allow managers to waive material misstatements.

Audit Committee Financial Expertise and the Resolution of Detected Misstatements

SAS 89 requires auditors to inform the audit committee about significant adjustments affecting the financial reporting process (paragraph .09) and about uncorrected misstatements that managers have judged immaterial (paragraph .10). These requirements allow audit committee oversight of misstatement resolution (Johnstone et al. 2001; Cohen et al. 2002, 2010; Ng and Tan 2003). We expect audit committee financial expertise to affect this oversight role.

Prior research shows that an audit committee's expertise affects its performance. Abbott et al. (2004) report a negative association between audit committee financial expertise and the presence of restatements, and Farber (2005) shows that companies experiencing fraud have fewer audit committee financial experts. In our setting, we expect that audit committees with greater financial expertise are more familiar with materiality concepts and hence are less likely to allow managers to waive material misstatements than are those with less expertise. Therefore, we make the following prediction with respect to the relation between audit committee financial expertise and the likelihood that managers will waive material misstatements:

H4: Audit committees with more financial expertise decrease the likelihood that managers will waive material misstatements.

III. RESEARCH DESIGN

Sample Description

Our data-collection period covers 10-Qs filed from November 15, 2006 to February 28, 2007 and 10-Ks filed from November 15, 2006 to February 15, 2008, and our analyses examine waived misstatements that existed in the financial statements during the period January 1, 2003 to September 30, 2006.[4] To identify these misstatements, we read SAB 108 disclosures to find companies that corrected misstatements under SAB 108.[5] The calculations of our dependent and control variables require that companies disclose misstatement-type descriptions (e.g., derivative, business combination, or revenue) and individual misstatement amounts during specific years in our sample period.[6] Our sample includes individual misstatements in financial statements in a specific year. We conduct our tests at the individual misstatement level to avoid netting or aggregating misstatements and to control for the accounting complexity of misstatements.

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[4] Although some SAB 108 companies did disclose misstatements that existed prior to this period, we restrict our sample to examine the period after the Sarbanes-Oxley Act of 2002 (SOX) because the introduction of SOX could have changed the misstatement resolution process.

[5] An example of a SAB 108 disclosure is available in Keune and Johnstone (2009, Exhibit 1).

Table 1, Panel A reports sample selection information, while Panels B and C report industry and year concentrations. The sample initially consists of 1,290 SAB 108 misstatements with necessary information. We eliminate 295 misstatements detected and corrected by companies in the year of SAB 108 implementation. These misstatements differ from other SAB 108 misstatements because they were not evaluated by auditors, managers, and audit committees in past periods. Next, we eliminate 32 misstatements related to non-U.S. and holding firms, 7 misstatements without Compustat data, 87 misstatements without Audit Analytics data, and 86 misstatements without audit committee and governance data. We estimate Model 3 (detailed below), which examines quantitatively material misstatements using a normal income measure, on this sample of 783 misstatements. Model 2 (detailed below) examines quantitatively material misstatements using a pretax income measure, which requires removing 200 additional misstatements at companies with current-year losses and small profits. Model 1 (detailed below) examines misstatements that allowed companies to meet or beat analysts' median consensus forecasts. To arrive at the sample of 340 misstatements for this model, we eliminate from the Model 3 sample 178 misstatements without I/B/E/S data and 265 misstatements for companies that did not meet or beat analysts' median consensus forecasts.

Model 1: Qualitative Materiality

We test H1–H4 by estimating three logit regression models (collectively "Model 1a, 1b, and 1c" or "Model 1") in which the dependent variable includes a benchmark test relating to meeting or beating analysts' consensus forecasts. Our first model is Model 1a:

$$\begin{aligned} QUAL\_MATERIAL = {} & \alpha + \beta_1 ANALYST + \beta_2 FEE + \beta_3 ANALYST \times FEE + \beta_4 ACEXP \\ & + \beta_5 ACMTG + \beta_6 MIS\_TYPE + \beta_7 TENURE + \beta_8 BIG\_N \\ & + \beta_9 BLOCK + \beta_{10} CEOCOB + \beta_{11} INVREC + \beta_{12} AGE + \beta_{13} MW \\ & + \beta_{14} LITRISK + \beta_{15} LOSS + \beta_{16} SEG + \beta_{17} SALESGROWTH \\ & + \beta_{18} ASSETS + \beta_{19} Industry_i + \beta_{20} Year_i + \varepsilon. \end{aligned} \tag{1a}$$

To capture the use of misstatements to meet or beat analysts' consensus forecasts, we compare the per-share effect of each individual misstatement to the relevant analyst consensus forecast for year *t*. Our proxy for qualitative materiality, *QUAL_MATERIAL*, equals 1 if, in year *t*, a company reported basic earnings per share (EPS) that meets or beats the last median consensus forecast before the earnings report but would have missed the forecast if the company corrected the misstatement amount affecting net income during year *t*, and equals 0 otherwise.[7] Appendix A illustrates measurement of the dependent variables, and Table 2 provides variable definitions.

Hypothesis-Testing Independent Variables

We measure analyst following as the log of the number of analysts' forecasts used to determine the consensus forecast, *ANALYST*. To capture audit fees, *FEE*, we employ four complementary measures. First, we analyze the magnitude of audit fees, *LNAUDIT*, which we measure as the

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[6] Consequently, our analyses do not include misstatements for which the company did not provide sufficient disclosure to allow us to ascertain the misstatement amounts present in the financial statements.

[7] Other benchmarks mentioned in SAB 99 include reporting net income versus net loss and reporting an earnings increase versus earnings decrease from the prior year. We are unable to conduct tests using these benchmarks due to the low frequency of SAB 108 misstatements used to achieve these benchmarks ($n \leq 20$ for each benchmark).

TABLE 1

Misstatements Sample

Panel A: Number of Misstatements

	Models 1 and 3	Model 2
Misstatements with identifying data	1,290	
Less: Misstatements detected by companies in the same year the misstatements were corrected	295	
Less: Non-U.S. and holding companies	32	
Less: Compustat missing data	7	
Less: Audit Analytics missing data	87	
Less: Audit committee and governance missing data	86	
Model 3 sample	783	783
Less: Companies with current-year losses and small profits		200
Model 2 sample		583
Less: I/B/E/S missing data	178	
Less: Companies that do not meet or beat analyst median consensus forecast	265	
Model 1 sample	340	

Panel B: Sample Frequency by Industry

		% of Misstatements		
Industry Description	**Two-Digit SIC Codes**	**Model 1 (n = 340)**	**Model 2 (n = 583)**	**Model 3 (n = 783)**
Ag., Forestry, Mining	00–19	6.8%	8.4%	7.7%
Manufacturing 1	20–29	13.5%	17.3%	14.8%
Manufacturing 2	30–39	11.2%	8.4%	7.8%
Trans., Comm., Utilities	40–49	6.8%	7.5%	10.7%
Wholesale and Retail	50–59	8.5%	7.4%	6.0%
Banks, Insurance, Real Estate	60–69	24.1%	28.3%	28.4%
Hotels and Services	70–79	25.6%	17.0%	19.9%
Health, Legal, Ed., Eng. Svcs	80–99	3.5%	5.7%	4.7%
Total		100.0%	100.0%	100.0%

Panel C: Sample Frequency by Year

	% of Misstatements		
Year	**Model 1 (n = 340)**	**Model 2 (n = 583)**	**Model 3 (n = 783)**
2003	25.6%	24.2%	26.4%
2004	30.0%	31.2%	30.4%
2005	38.2%	37.9%	37.4%
2006[a]	6.2%	6.7%	5.7%
Total[a]	100.0%	100.0%	100.0%

[a] Percentages do not sum to 100.0 due to rounding.

TABLE 2

Variable Definitions

Variable Name		Description
Dependent Variables		
QUAL_MATERIAL	=	1 if company reported basic EPS that meet or beat last analysts' median consensus forecast before earnings report, but would have missed the forecast if it corrected the income statement misstatement, 0 otherwise;
QUANT_MATERIAL_PRETAX	=	1 if abs(larger of income statement or balance sheet misstatement amount/pretax income using Compustat "pi") is $\geq$ 5 percent, 0 otherwise; and
QUANT_MATERIAL_NORM	=	1 if abs(larger of income statement or balance sheet misstatement amount/normal income) is $\geq$ 5 percent, 0 otherwise.
Independent Variables		
ANALYST	=	ln(analysts' forecasts determining consensus) using I/B/E/S "numest";
FEE	=	*LNAUDIT*, *ABNORMALAUDIT*, *OFFICEAUDIT*, and *FIRMAUDIT*;
LNAUDIT	=	ln(Audit Fees) from Audit Analytics;
ABNORMALAUDIT	=	residual (i.e., *LNAUDIT* − predicted value) from audit fee estimation model where a negative (positive) value implies audit fees lower (higher) than predicted for auditor's effort and risk;
OFFICEAUDIT	=	fractional percentile rank of client's audit fees within opinion issuing office of the audit firm in Audit Analytics where higher values imply larger fees;
FIRMAUDIT	=	fractional percentile rank of client's audit fees within auditor's firm in Audit Analytics where higher values imply larger fees; and
ACEXP	=	percentage of audit committee members with financial expertise.
Control Variables		
FE_NOSCALE	=	$\lvert$[(reported earnings − waived error) − consensus forecast]$\rvert$;
FE_SCALED	=	$\lvert$[(reported earnings − waived error) − consensus forecast]/End of year common stock price$\rvert$ × 100;
ACMTG	=	number of audit committee meetings during the year;
MIS_TYPE	=	1 if revenue, reserve, tax, derivative, business combination, lease, pension, or stock option misstatement, 0 otherwise;
TENURE	=	1 if auditor tenure in Audit Analytics is three or more years, 0 otherwise;
BIG_N	=	1 if KPMG, PricewaterhouseCoopers, Deloitte, or Ernst & Young is auditor in Audit Analytics, 0 otherwise;
BLOCK	=	1 if any stockholder other than a manager or director owns more than 5 percent of outstanding shares, 0 otherwise;
CEOCOB	=	1 if the CEO serves as the chair of the board of directors, 0 otherwise;
INVREC	=	sum of total inventory (Compustat "invt") and total receivables (Compustat "rect") deflated by total assets (Compustat "at");
AGE	=	ln(data year − first year company listed in Compustat);

(continued on next page)

TABLE 2 (continued)

Variable Name		Description
MW	=	1 if SOX Section 302 or 404 material weakness is reported in 10-K according to Audit Analytics, 0 otherwise;
LITRISK	=	1 if industry is 2833–2836, 8731–8734, 3570–3577, 7370–7374, 3600–3674, 5200–5961, 0 otherwise;
LOSS	=	1 if net loss in any of last three years (using Compustat "ib"), 0 otherwise;
SEG	=	ln(number of business segments from Compustat segment file);
SALESGROWTH	=	(current year net sales − prior year net sales)/prior year net sales (using Compustat "sale");
ASSETS	=	ln(total assets) using Compustat "at";
Industry	=	industry indicator variables are SIC 01–14, SIC 15–19, SIC 20–21, SIC 22–23, SIC 24–27, SIC 28–32, SIC 33–34, SIC 35–39, SIC 40–48, SIC 49, SIC 50–52, SIC 53–59, SIC 60–69, and SIC 70–79; and
Year	=	year indicator variables.

natural logarithm of audit fees. As noted by Wright and Wright (1997, 20), "the larger the audit fees, the greater is the risk to future income and the higher is the incentive to waive adjustments consistent with client pressures." The empirical question that we address is whether auditors acquiesce to this pressure in a setting in which third parties can observe audit fees.

Second, we examine a client's abnormal or unexpected audit fees, *ABNORMALAUDIT*, measured using the residual from a model predicting *LNAUDIT*.[8] Consistent with Larcker and Richardson (2004), we expect auditors' behavior to vary depending on whether they are paid more or less for the audit than a relevant economic benchmark. If the abnormal fee is less than or equal to 0, then auditors presumably have little to lose by insisting that managers correct the misstatement because their audit fees are not commensurate with engagement risk and effort. If the abnormal fee is greater than 0, in contrast, then auditors are likely to feel greater pressure to maintain the relationship with the client. However, in this setting auditors' judgments may not necessarily be biased in favor of managers' preferences. Instead, auditors in the post-SOX period may have competing incentives to charge abnormally high audit fees to perform additional work to protect their reputations and avoid second-guessing by regulators and third-party observers.

Our third and fourth measures of audit fees are the fractional rank of the client's audit fees within the opinion-issuing office of the audit firm, *OFFICEAUDIT*, which represents the importance of the client to the office and its partners, and the fractional rank of the client's audit fees within the opinion-issuing firm, *FIRMAUDIT*, which represents the importance of the client to the entire firm and its partners.[9] Client importance could motivate auditors to allow managers to waive misstatements.

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[8] To predict *LNAUDIT*, we estimate the following model from Larcker and Richardson (2004), which is based on Simunic (1984) and Craswell et al. (1995) and uses a sample of 12,236 firm-years from Compustat and Audit Analytics: $LNAUDIT = \alpha + \beta_1 ASSETS + \beta_2 SEG + \beta_3 INVREC + \beta_4 DEBT + \beta_5 INCOME + \beta_6 LOSS + \beta_7 GC + \beta_8 MW + \beta_9 Industry_i$; where *DEBT* = (debt in current and long-term liabilities, Compustat "dlc" and "dltt")/total assets (Compustat "at"); *INCOME* = operating income after depreciation (Compustat "oiadp")/total assets (Compustat "at"); *GC* = 1 if there is a going concern opinion in Audit Analytics, and 0 otherwise; and all other variables are as defined in Table 2. The model predicts *LNAUDIT* with an Adjusted R^2 of 0.711, which is comparable to the Adjusted R^2 of 0.749 for total fees reported by Larcker and Richardson (2004).

[9] The fractional rank variable ranges from 0.01 to 1.00, where 0.01 (1.00) is the smallest (largest) client within the sub-population.

Frankel et al. (2002), however, find that the rank of the client's fees within the audit firm is negatively associated with various earnings management measures including earnings surprises and the magnitude of absolute, income-increasing, and income-decreasing discretionary accruals. They interpret these results as indicating that the client's financial importance and visibility provide the audit firm with incentives to invest in its reputational capital. Consistent with prior research, we posit that investments in reputational capital and risk management likely occur through increased audit effort (Bedard and Johnstone 2004; Kinney et al. 2004). In sum, our four audit fee measures capture potentially divergent incentives: the pressure to acquiesce to managers' demands and the pressure to maintain a valuable reputation. Our final hypothesis-testing variable measures audit committee financial expertise, *ACEXP*, as the percentage of audit committee members with financial expertise.[10]

Control Variables

We control for audit committee diligence using the number of audit committee meetings during the year, *ACMTG*.[11] Abbott et al. (2004) report a negative association between the number of audit committee meetings and restatements, suggesting that more diligent audit committees provide more rigorous oversight. Similarly, Farber (2005) shows that companies experiencing fraud have less diligent audit committees. These results suggest that audit committees that meet more frequently may be less likely to allow managers to waive material misstatements. However, audit committees may also have more meetings in order to evaluate and resolve issues concerning the misstatements. Thus, we make no directional prediction for the relation between material waived misstatements and the number of audit committee meetings.

We control for the complexity of the misstatement using *MIS_TYPE*, which is an indicator variable equal to 1 if the misstatement involves complex accounting judgments, and 0 otherwise. Consistent with prior research, we expect that complex misstatements are more likely to be material (Keune and Johnstone 2009). Next, we control for the length of an auditor-client relationship using *TENURE*, an indicator variable equal to 1 if the auditor-client relationship has been in effect three or more years, and 0 otherwise. Dopuch et al. (2001) suggest that longer-tenured auditors display reduced independence, but other research shows that companies with short (long) auditor relationships are associated with higher (lower) audit and financial reporting quality (Geiger and Raghunandan 2002; Johnson et al. 2002; Myers et al. 2003; Carcello and Nagy 2004). Thus, we make no directional prediction for *TENURE*.[12] We control for audit firm size using the indicator variable *BIG_N*. Although large audit firms may be associated with higher audit quality (DeAngelo 1981a, 1981b) and audit fees (Ashbaugh et al. 2003), no prior research suggests that Big N audit firms are more or less likely to allow managers to waive a material misstatement, so we make no directional prediction for this variable.

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[10] We define audit committee financial experts as members with accounting expertise (i.e., Certified Public Accountants, Chartered Accountants, chief accounting officers, vice presidents of finance, controllers, treasurers, and Big N or national audit firm employees or partners) or finance expertise (i.e., business school faculty, bankers, investment bankers, Certified Financial Analysts, and venture capitalists). We use this definition because companies' required disclosures of designated audit committee financial experts do not include all relevant financial experts (Carcello et al. 2006).

[11] Audit committee independence may also be related to audit committee effectiveness. The average audit committee independence in our post-SOX sample is high (99.4 percent) and shows virtually no variation. Therefore, we do not control for audit committee independence.

[12] Our dichotomous measurement is consistent with Johnson et al. (2002) and Carcello and Nagy (2004). We are unable to use continuous (Geiger and Raghunandan 2002) or decile (Myers et al. 2003) measures as Compustat does not have sufficient details to compute these measures for all audit firms in our sample, and Audit Analytics coverage began in 1999, which implies that Audit Analytics data would only provide truncated measures of auditor tenure.

We also include controls for various company characteristics. First, we control for the presence of large non-director or manager blockholders (*BLOCK*), consistent with Carcello et al. (2006). Large blockholders may play a monitoring role (Shleifer and Vishny 1986) that can serve as a substitute for the audit committee's governance. Thus, we expect the presence of blockholders to be negatively associated with material misstatements being waived. Second, we control for the presence of a CEO who also serves as the chairman of the board (*CEOCOB*). A CEO who serves as the board chair provides less independent corporate oversight (Jensen 1993). Thus, such duality is expected to be positively associated with material misstatements being waived. Next, we control for both the relative size of inventories and receivables (*INVREC*) and firm age (*AGE*), but make no directional predictions for either of these variables. We also control for disclosure of an internal control material weakness (*MW*), litigation risk (*LITRISK*; Francis et al. 1994), recent losses (*LOSS*), company complexity (*SEG*), and growth (*SALESGROWTH*). We make no directional prediction for any of these variables, as they may be positively associated with managers waiving material misstatements because of the risk and complexity that they represent, but auditors may be less likely to allow managers of companies with greater risk and complexity to waive material misstatements. Finally, we control for the size of the company (*ASSETS*) and make no directional prediction for this variable.[13]

Forecast Error: Models 1b and 1c

While we interpret a positive association between analyst following and the qualitative materiality of the misstatement as higher analyst following creating more pressure for managers to meet or beat analyst consensus forecast, an alternative explanation is that companies with higher analyst following may be more likely to meet or just beat analyst consensus forecast because the presence of more analysts creates more accurate consensus forecasts. Thus, results on this variable of interest could be due to a mechanical relation between analyst following and analyst forecast error.[14] To address this alternative explanation, we re-estimate Model 1a using two alternative specifications that include measures of unsigned forecast error (*FE*), along with the interaction of unsigned forecast error with audit fee (*FE* × *FEE*) to ensure that the *ANALYST* × *FEE* interaction is not subject to confounding (Models 1b and 1c). We predict a negative association between forecast error and *QUAL_MATERIAL* because correcting the misstatement should be less likely to cause the company to miss its forecast as forecast error increases in our sample of companies that meet or beat analyst consensus forecast; we make no directional prediction for the *FE* × *FEE* interaction.

We use two measures of forecast error. Our first measure of forecast error is an unscaled version that equals $|[(\text{reported earnings} - \text{waived error}) - \text{consensus forecast}]|$. This measure is *FE_NOSCALE* in Model 1b, and allows us to control for the raw size of the unsigned forecast error. Cheong and Thomas (2011) show that *signed* forecast error does not vary with scale and recommend that researchers consider using unscaled forecast error measures.

Our second measure of forecast error is a version scaled by stock price that equals $|[(\text{reported earnings} - \text{waived error}) - \text{consensus forecast}]/\text{end of year common stock price}| \times 100$. This measure is *FE_SCALED* in Model 1c. *FE_SCALED* allows us to control for the relative size of forecast error, which is likely to be associated with companies' ability to generate similarly large misstatements that exceed forecast error. Scaled forecast error is the predominant measure used in the literature (e.g., Bowen et al. 2002; Eames and Glover 2003).[15] Further, Ball (2011) notes that Cheong and Thomas (2011) only provide empirical support regarding *signed* forecast error and applying their conclusions to *unsigned* forecast error (*FC_NOSCALE*) may not be appropriate in other settings. We use these measures in the following variations of Model 1a:

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[13] We include industry and year indicator variables in all models. Our industry dummies are consistent with Frankel et al. (2002) and Ashbaugh et al. (2003) except that we add SIC 60-69 to their categories due to the prominence of financial services companies in our sample.

[14] An examination from 2003–2006 of I/B/E/S companies' last analyst consensus forecast before earnings release suggests that unsigned and unscaled forecast error is negatively associated with analyst following ($n = 18{,}401$, $r = -0.20$, $p < 0.001$). While this relation also holds for unsigned and unscaled forecast error ($n = 340$, $r = -0.15$, $p = 0.004$) and unsigned and scaled forecast error ($n = 340$, $r = -0.22$, $p < 0.001$) in our sample, the correlation between analyst following and consensus forecast error is not as large as intuition may suggest.

$$\begin{aligned} QUAL\_MATERIAL = {} & \alpha + \beta_1 ANALYST + \beta_2 FEE + \beta_3 ANALYST \times FEE + \beta_4 ACEXP \\ & + \beta_5 FE\_NOSCALE + \beta_6 FE\_NOSCALE \times FEE + \beta_7 ACMTG \\ & + \beta_8 MIS\_TYPE + \beta_9 TENURE + \beta_{10} BIG\_N + \beta_{11} BLOCK \\ & + \beta_{12} CEOCOB + \beta_{13} INVREC + \beta_{14} AGE + \beta_{15} MW + \beta_{16} LITRISK \\ & + \beta_{17} LOSS + \beta_{18} SEG + \beta_{19} SALESGROWTH + \beta_{20} ASSETS \\ & + \beta_{21} Industry_i + \beta_{22} Year_i + \varepsilon. \end{aligned} \tag{1b}$$

$$\begin{aligned} QUAL\_MATERIAL = {} & \alpha + \beta_1 ANALYST + \beta_2 FEE + \beta_3 ANALYST \times FEE + \beta_4 ACEXP \\ & + \beta_5 FE\_SCALED + \beta_6 FE\_SCALED \times FEE + \beta_7 ACMTG \\ & + \beta_8 MIS\_TYPE + \beta_9 TENURE + \beta_{10} BIG\_N + \beta_{11} BLOCK \\ & + \beta_{12} CEOCOB + \beta_{13} INVREC + \beta_{14} AGE + \beta_{15} MW + \beta_{16} LITRISK \\ & + \beta_{17} LOSS + \beta_{18} SEG + \beta_{19} SALESGROWTH + \beta_{20} ASSETS \\ & + \beta_{21} Industry_i + \beta_{22} Year_i + \varepsilon. \end{aligned} \tag{1c}$$

Models 2 and 3: Quantitative Materiality

We also test H2 and H4 by estimating logit regression models in which the dependent variables include a pretax income measure (Model 2) or a normal income measure (Model 3):

$$\begin{aligned} QUANT\_MATERIAL\_PRETAX = {} & \alpha + \beta_1 FEE + \beta_2 ACEXP + \beta_3 ACMTG + \beta_4 MIS\_TYPE \\ & + \beta_5 TENURE + \beta_6 BIG\_N + \beta_7 BLOCK + \beta_8 CEOCOB \\ & + \beta_9 INVREC + \beta_{10} AGE + \beta_{11} MW + \beta_{12} LITRISK \\ & + \beta_{13} LOSS + \beta_{14} SEG + \beta_{15} SALESGROWTH \\ & + \beta_{16} ASSETS + \beta_{17} Industry_i + \beta_{18} Year_i + \varepsilon. \end{aligned} \tag{2}$$

$$\begin{aligned} QUANT\_MATERIAL\_NORM = {} & \alpha + \beta_1 FEE + \beta_2 ACEXP + \beta_3 ACMTG + \beta_4 MIS\_TYPE \\ & + \beta_5 TENURE + \beta_6 BIG\_N + \beta_7 BLOCK + \beta_8 CEOCOB \\ & + \beta_9 INVREC + \beta_{10} AGE + \beta_{11} MW + \beta_{12} LITRISK \\ & + \beta_{13} LOSS + \beta_{14} SEG + \beta_{15} SALESGROWTH \\ & + \beta_{16} ASSETS + \beta_{17} Industry_i + \beta_{18} Year_i + \varepsilon. \end{aligned} \tag{3}$$

For each year a misstatement is present during our sample period, we identify the amount of the misstatement on the balance sheet and on the income statement. Consistent with the dual approach required by SAB 108, we calculate the quantitative materiality of misstatements in year *t* using the greater of these two amounts.

Quantitative materiality can be measured using various thresholds (Brody et al. 2003), but 5 percent of pretax income is the most common measure for public companies (Leslie 1985; Messier et al. 2005). Therefore, we first estimate the quantitative materiality of misstatements as 5 percent of pretax income, *QUANT_MATERIAL_PRETAX*. Note, however, a pretax income measure is less appropriate when companies report net losses or breakeven results, because materiality cannot be negative in a net loss year or almost zero in a breakeven year (Leslie 1985). To address this issue, we omit observations with current-year losses and small profits that are less than 1 percent of total assets when using *QUANT_MATERIAL_PRETAX* as our dependent variable.

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[15] We performed a search for studies that include measures of forecast error for each of the last ten years in *The Accounting Review*, *Journal of Accounting Research*, *The Journal of Accounting and Economics*, and *Contemporary Accounting Research*. The search yielded 40 articles, each of which uses some variation of a scaled forecast error variable, almost always scaled by stock price. Therefore, the overwhelming pattern in the literature is to use a stock-price scaled forecast error variable.

We next estimate the quantitative materiality of misstatements as 5 percent of pretax normal income, *QUANT_MATERIAL_NORM*.[16] This approach allows us to use all of the observations in our sample without removing loss and small-profit observations. Leslie (1985, 30) reports a study that finds that the materiality decisions of companies with loss and breakeven results are "within 5% to 10% of what might be considered normal net income, based on return on stockholders' equity." Following Leslie (1985), we estimate the "normal income" of our sample companies as the greatest of the industry return on equity (*ROE*) multiplied by the company's stockholders' equity, the *ROE* of 1 percent for companies in the overall market multiplied by the company's stockholders' equity, or the company's reported pretax income.[17] All other variables in Models 2 and 3 are as previously defined for Model 1a.

IV. RESULTS

Descriptive Statistics

Table 3, Panel A compares companies waiving qualitatively material versus qualitatively immaterial misstatements. Results show that companies waiving qualitatively material misstatements have lower audit fees (*LNAUDIT*: $p = 0.01$), lower abnormal audit fees (*ABNORMALAUDIT*: $p = 0.01$), and audit fees that are marginally less important to the auditor's local office or firm portfolio (*OFFICEAUDIT*: $p = 0.09$; *FIRMAUDIT*: $p = 0.10$), as compared to companies waiving qualitatively immaterial misstatements. Companies waiving qualitatively material misstatements also have fewer audit committee financial experts (*ACEXP*: $p < 0.01$), have lower absolute and relative forecast errors (*FE_NOSCALE*: $p < 0.01$; *FE_SCALED*: $p < 0.01$), are marginally more likely to have misstatements involving complex accounting judgments (*MIS_TYPE*: $p = 0.09$), have higher relative inventories and receivables (*INVREC*: $p = 0.02$), and are marginally less likely to report a recent loss (*LOSS*: $p = 0.08$).

Table 3, Panels B and C compare quantitatively material to quantitatively immaterial misstatements by splitting the groups based on *QUANT_MATERIAL_PRETAX* (Panel B) and *QUANT_MATERIAL_NORM* (Panel C). Results are similar across the two panels and show that companies waiving quantitatively material misstatements have lower audit fees (*LNAUDIT*: $p < 0.01$ and $p < 0.01$ in Panels B and C, respectively) and audit fees that are less important to

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[16] Gleason and Mills (2002) define normal income as the greater of a company's pretax income or 5 percent of assets, where 5 percent of assets is the median pretax return on assets for their 100 large manufacturing companies over a nine-year period.

[17] More specifically, we use the Compustat database to identify the yearly median *ROE* for the industry of each company within our sample. We define a company's industry at the two-digit SIC code level. We calculate *ROE* as Compustat pretax income (pi)/total stockholders' equity (seq). This measure can be used to approximate a company's normal income by multiplying the applicable *ROE* by stockholders' equity unless the industry has been depressed during the period. In the case of an industry-wide market downturn, Leslie (1985, 31) recommends using a "rate of return on equity as compared with all business in general." In untabulated analysis we estimate the median *ROE* for U.S. companies to be 1.3 percent, 1.7 percent, 1.8 percent, and 1.9 percent for 2003, 2004, 2005, and 2006, respectively. The overall median *ROE* for U.S. companies across our sample period is 1.6 percent. To be conservative, we define *ROE* as 1.0 percent for companies in the overall market during our sample period.

TABLE 3
Descriptive Statistics

Panel A: Comparison of Qualitatively Material to Immaterial Misstatements

	Qualitatively Material Misstatements[a] (n = 40)			Qualitatively Immaterial Misstatements[a] (n = 300)				
Variables[b]	**Mean**	**Std. Dev.**	**Med**	**Mean**	**Std. Dev.**	**Med**	**t-statistic[c]**	**p-value[c]**
ANALYST	1.62	1.04	1.79	1.55	1.09	1.61	−0.42	0.68
LNAUDIT	13.53	0.93	13.72	14.00	1.28	13.94	2.88	0.01
ABNORMALAUDIT	−0.37	0.63	−0.34	−0.04	0.75	−0.08	2.66	0.01
OFFICEAUDIT	0.62	0.27	0.70	0.69	0.25	0.76	1.69	0.09
FIRMAUDIT	0.69	0.16	0.70	0.74	0.19	0.77	1.68	0.10
ACEXP	0.41	0.24	0.33	0.53	0.28	0.50	3.05	< 0.01
FE_NOSCALE	0.03	0.04	0.02	0.09	0.20	0.03	4.34	< 0.01
FE_SCALED	0.16	0.28	0.08	0.51	1.19	0.14	4.33	< 0.01
ACMTG	9.70	3.24	9.50	8.76	3.72	8.00	−1.52	0.13
MIS_TYPE	0.65	0.48	1.00	0.49	0.50	0.00	2.87	0.09
TENURE	0.85	0.36	1.00	0.74	0.44	1.00	1.74	0.19
BIG_N	0.98	0.16	1.00	0.97	0.17	1.00	0.00	1.00
BLOCK	0.88	0.33	1.00	0.83	0.38	1.00	0.24	0.62
CEOCOB	0.58	0.50	1.00	0.69	0.46	1.00	1.76	0.19
INVREC	0.36	0.23	0.28	0.27	0.23	0.22	−2.40	0.02
AGE	2.49	0.29	2.64	2.52	0.33	2.64	0.46	0.65
MW	0.05	0.22	0.00	0.12	0.32	0.00	1.00	0.32
LITRISK	0.18	0.38	0.00	0.30	0.46	0.00	2.01	0.16
LOSS	0.18	0.38	0.00	0.32	0.47	0.00	2.99	0.08
SEG	1.21	0.75	1.24	1.30	0.81	1.10	0.69	0.49
SALESGROWTH	0.14	0.15	0.13	0.18	0.24	0.10	1.38	0.17
ASSETS	7.19	1.58	7.00	7.27	1.78	7.67	0.27	0.79

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[a] Qualitatively material (immaterial) SAB 108 misstatements are *QUAL_MATERIAL* = 1 (0).

[b] Variable definitions are in Table 2.

[c] Continuous variable statistics are t-statistics from means tests. Indicator variable statistics are Chi-squares with continuity corrections. All p-values are two-tailed.

Panel B: Comparison of Quantitatively Material to Immaterial Misstatements (*QUANT_MATERIAL_PRETAX*)

	Quantitatively Material Misstatements[a] (n = 113)			Quantitatively Immaterial Misstatements[a] (n = 470)				
Variables[b]	**Mean**	**Std. Dev.**	**Med**	**Mean**	**Std. Dev.**	**Med**	**t-statistic[c]**	**p-value[c]**
LNAUDIT	13.12	1.07	13.12	13.72	1.15	13.65	5.26	< 0.01
ABNORMALAUDIT	−0.30	0.68	−0.21	−0.20	0.73	−0.18	1.26	0.21
OFFICEAUDIT	0.53	0.28	0.57	0.64	0.26	0.71	4.50	< 0.01
FIRMAUDIT	0.61	0.19	0.59	0.71	0.18	0.72	5.32	< 0.01

(continued on next page)

TABLE 3 (continued)

	Quantitatively Material Misstatements[a] (n = 113)			Quantitatively Immaterial Misstatements[a] (n = 470)				
Variables[b]	**Mean**	**Std. Dev.**	**Med**	**Mean**	**Std. Dev.**	**Med**	**t-statistic[c]**	**p-value[c]**
ACEXP	0.52	0.27	0.50	0.52	0.31	0.50	−0.08	0.94
ACMTG	7.46	2.77	7.00	8.75	4.23	8.00	4.05	< 0.01
MIS_TYPE	0.67	0.47	1.00	0.48	0.50	0.00	14.34	< 0.01
TENURE	0.68	0.47	1.00	0.74	0.44	1.00	1.76	0.18
BIG_N	0.89	0.32	1.00	0.91	0.29	1.00	0.35	0.56
BLOCK	0.90	0.30	1.00	0.78	0.42	1.00	9.23	< 0.01
CEOCOB	0.66	0.48	1.00	0.63	0.48	1.00	0.25	0.62
INVREC	0.23	0.21	0.16	0.33	0.25	0.25	4.41	< 0.01
AGE	2.54	0.53	2.48	2.63	0.40	2.71	1.73	0.09
MW	0.11	0.32	0.00	0.13	0.34	0.00	0.23	0.63
LITRISK	0.30	0.46	0.00	0.18	0.39	0.00	6.90	< 0.01
LOSS	0.45	0.50	0.00	0.18	0.39	0.00	36.24	< 0.01
SEG	1.21	0.73	1.10	1.20	0.86	1.10	−0.17	0.87
SALESGROWTH	0.11	0.18	0.11	0.16	0.23	0.10	2.58	0.01
ASSETS	6.00	1.66	5.82	7.26	1.53	7.08	7.99	< 0.01

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[a] Quantitatively material misstatements refers to *QUANT_MATERIAL_PRETAX* = 1. Quantitatively immaterial misstatements refers to *QUANT_MATERIAL_PRETAX* = 0.

[b] Variable definitions are in Table 2.

[c] Continuous variable statistics are t-statistics from means tests. Indicator variable statistics are Chi-squares with continuity corrections. All p-values are two-tailed.

Panel C: Comparison of Quantitatively Material to Immaterial Misstatements (*QUANT_MATERIAL_NORM*)

	Quantitatively Material Misstatements[a] (n = 243)			Quantitatively Immaterial Misstatements[a] (n = 540)				
Variables[b]	**Mean**	**Std. Dev.**	**Med**	**Mean**	**Std. Dev.**	**Med**	**t-statistic[c]**	**p-value[c]**
LNAUDIT	13.40	1.16	13.53	13.82	1.31	13.70	4.49	< 0.01
ABNORMALAUDIT	−0.14	0.70	−0.15	−0.15	0.79	−0.18	−0.21	0.83
OFFICEAUDIT	0.61	0.27	0.67	0.66	0.26	0.73	2.08	0.04
FIRMAUDIT	0.69	0.20	0.72	0.72	0.19	0.75	2.06	0.04
ACEXP	0.51	0.26	0.50	0.51	0.30	0.50	−0.01	0.99
ACMTG	7.98	3.29	8.00	9.96	7.33	8.00	5.21	< 0.01
MIS_TYPE	0.60	0.49	1.00	0.48	0.50	0.00	8.18	< 0.01
TENURE	0.65	0.48	1.00	0.75	0.43	1.00	8.10	< 0.01
BIG_N	0.80	0.40	1.00	0.89	0.32	1.00	8.82	< 0.01
BLOCK	0.91	0.29	1.00	0.80	0.40	1.00	14.52	< 0.01
CEOCOB	0.65	0.48	1.00	0.60	0.49	1.00	1.58	0.21
INVREC	0.25	0.22	0.18	0.33	0.26	0.27	4.69	< 0.01
AGE	2.44	0.49	2.48	2.60	0.40	2.71	4.59	< 0.01

(continued on next page)

TABLE 3 (continued)

Variables[b]	Quantitatively Material Misstatements[a] (n = 243) Mean	Std. Dev.	Med	Quantitatively Immaterial Misstatements[a] (n = 540) Mean	Std. Dev.	Med	t-statistic[c]	p-value[c]
MW	0.13	0.33	0.00	0.13	0.34	0.00	0.00	0.97
LITRISK	0.26	0.44	0.00	0.18	0.38	0.00	7.04	0.01
LOSS	0.69	0.46	1.00	0.24	0.43	0.00	141.09	< 0.01
SEG	1.16	0.77	0.69	1.23	0.88	1.10	1.06	0.29
SALESGROWTH	0.12	0.27	0.07	0.16	0.22	0.10	2.21	0.03
ASSETS	6.08	1.74	5.90	7.36	1.61	7.23	9.72	< 0.01

[a] Quantitatively material misstatements refers to *QUANT_MATERIAL_NORM* = 1. Quantitatively immaterial misstatements refers to *QUANT_MATERIAL_NORM* = 0.
[b] Variable definitions are in Table 2.
[c] Continuous variable statistics are t-statistics from means tests. Indicator variable statistics are Chi-Squares with continuity corrections. All p-values are two-tailed.

the auditor's local office or firm portfolio (*OFFICEAUDIT*: $p < 0.01$ and $p = 0.04$; *FIRMAUDIT*: $p < 0.01$ and $p = 0.04$), as compared to companies waiving quantitatively immaterial misstatements. Companies waiving quantitatively material misstatements also have fewer audit committee meetings (*ACMTG*: $p < 0.01$ and $p < 0.01$), are more likely to have misstatements involving complex accounting judgments (*MIS_TYPE*: $p < 0.01$ and $p < 0.01$), have shorter relationships with their audit firm (*TENURE*: $p < 0.01$ in Panel C only), are less likely to be audited by a Big 4 auditor (*BIG_N*: $p < 0.01$ in Panel C only), are more likely to have large blockholders (*BLOCK*: $p < 0.01$ and $p < 0.01$), have lower relative inventories and receivables (*INVREC*: $p < 0.01$ and $p < 0.01$), and are younger (*AGE*: $p = 0.09$ and $p < 0.01$). Further, companies waiving quantitatively material misstatements are more likely to be in a litigious industry (*LITRISK*: $p < 0.01$ and $p = 0.01$), are more likely to experience a loss (*LOSS*: $p < 0.01$ and $p < 0.01$), have lower sales growth (*SALESGROWTH*: $p = 0.01$ and $p = 0.03$), and are smaller in terms of assets (*ASSETS*: $p < 0.01$ and $p < 0.01$), as compared to companies waiving immaterial misstatements.

In untabulated analyses, the correlations among the independent variables for Models 1a–1c, Model 2, and Model 3 are relatively low. The only exceptions are the correlations between company size (*ASSETS*) and the measures for audit fees (*FEE*) and analyst following (*ANALYST*), which are consistently above 0.50. However, collinearity does not appear to be an issue in any of the hypothesis-testing models. The variance inflation factors (VIFs) from linear regressions are all consistently well below the 10.00 cutoff suggested by Belsley et al. (1980).

Model 1: Qualitative Materiality Tests (*QUAL_MATERIAL*)

H1 predicts that analyst following increases the likelihood that managers will waive material misstatements.[18] Consistent with this expectation, the *ANALYST* coefficient across Models 1a–1c in

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[18] We estimate the models in Tables 4 and 5 and all robustness tests using robust standard errors clustered by company. We also winsorize all continuous variables at the 1st and 99th percentiles.

Panels A, C, E, and G of Table 4 generally shows a positive one-tailed association between analyst following and the likelihood of managers waiving a qualitatively material misstatement in models in which the independent variable is the level of audit fees (*LNAUDIT*: $p = 0.04$, $p = 0.09$, $p = 0.04$, respectively), abnormal audit fees (*ABNORMALAUDIT*: $p = 0.05$, $p = 0.11$, $p = 0.04$, respectively), the office rank of audit fees (*OFFICEAUDIT*: $p = 0.04$, $p = 0.05$, $p = 0.05$, respectively), and the firm rank of audit fees (*FIRMAUDIT*: $p = 0.03$, $p = 0.08$, $p = 0.05$, respectively). Overall, *ANALYST* is statistically significant in models when we do not control for forecast error or control for scaled forecast error (*FE_SCALED*) and is weaker or marginal when we control for unscaled forecast error (*FE_NOSCALE*).

It is important to note, however, that the coefficient on *ANALYST* only tests the association between analyst following and the dependent variable conditional on mean-centered audit fees being zero because *ANALYST* is interacted with *FEE*.[19] This association may be different across levels of *FEE* because the marginal effect of *ANALYST* is the partial derivative with respect to *ANALYST* and considers the interaction term with *FEE*. To provide greater insight on the main effect of *ANALYST*, we examine the marginal effects of a one-unit change in *ANALYST* on a change in the probability of managers waiving a qualitatively material misstatement at three levels of *FEE*: at the mean, at the mean less one standard deviation (i.e., relatively low), and at the mean plus one standard deviation (i.e., relatively high). See Panels B, D, F, and H in Table 4.[20] These analyses show that the main effect of *ANALYST* is strongest when audit fees are relatively low and is weakest when audit fees are relatively high. For example, increasing analyst following in Model 1a by one unit is associated with an increase of 18 percent ($p < 0.01$) in the likelihood of waiving a qualitatively material misstatement when *LNAUDIT* is relatively low, but increasing analyst following by one unit is associated with a zero percent ($p = 0.43$) increase in the likelihood of waiving a qualitatively material misstatement when *LNAUDIT* is relatively high. Taken together, these results generally show that analyst following is positively associated with the probability of managers waiving a qualitatively material misstatement, and this association is primarily present when audit fees are relatively low.[21] However, these results must be interpreted in light of the interaction between analyst following and audit fees, which we discuss below.

H2 predicts that audit fees affect the likelihood that auditors will allow managers to waive material misstatements. The results across Models 1a–1c in Panels A, C, E, and G of Table 4 generally show a negative association between audit fees and the likelihood of managers waiving a qualitatively material misstatement in models in which the independent variable is the level of audit fees (*LNAUDIT*: $p < 0.01$, $p = 0.03$, $p = 0.01$, respectively), abnormal audit fees (*ABNORMALAUDIT*: $p < 0.01$, $p = 0.01$, $p = 0.01$, respectively), the office rank of audit fees (*OFFICEAUDIT*: $p = 0.25$, $p = 0.06$, $p = 0.05$, respectively), and the firm rank of audit fees (*FIRMAUDIT*: $p = 0.10$, $p = 0.05$, $p = 0.02$, respectively). These results generally show that audit

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[19] With regard to interpreting the coefficient for a variable that is also in the interaction, Jaccard (2001, 20) notes that "[a] common mistake . . . is to interpret this coefficient as if it represents a nonconditioned main effect." In other words, the coefficient for *ANALYST* represents the effect of *ANALYST* on *QUAL_MATERIAL* when mean-centered audit fees are zero.

[20] A marginal effect of 50 percent or 0.50 in a nonlinear model is most accurately interpreted as "y increases with x at a rate such that, if the rate were constant, y would increase by 0.5 if x increased by 1" (Stata 2009, 977). All marginal effects and p-values are calculated at the mean of the variable of interest using the Stata 11 margins command with the dydx option that appropriately recognizes when a covariate is used in a product term (e.g., *ANALYST* and *ANALYST* × *FEE*). This command uses the actual observation-level values of covariates unless otherwise specified and calculates the marginal effects using the sample average of individual marginal effects, which is the method favored in current practice (Greene 2008, 775).

[21] The exception is in the model using *OFFICEAUDIT* as the measure of audit fees. In that case, the main effect of *ANALYST* is significant at higher levels of audit fees and, as we discuss subsequently, we do not find evidence of an *ANALYST* × *OFFICEAUDIT* interaction.

TABLE 4

Logit Regression Models 1a, 1b, and 1c: *QUAL_MATERIAL*

Panel A: *LNAUDIT* as *FEE*

		Model 1a No Control for Forecast Error			Model 1b *FE_NOSCALE*			Model 1c *FE_SCALED*		
Variable[a]	**Pred. Sign**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**
Constant		−5.60	0.13		−5.44	0.15		−3.55	0.35	
ANALYST[c]	+ (H1)	0.68	0.04	6.1%	0.53	0.09	3.8%	0.67	0.04	3.1%
FEE	+/− (H2)	−1.27	< 0.01	−5.0%	−1.38	0.03	−4.4%	−1.56	0.01	−2.6%
ANALYST × *FEE*[c]	+/− (H3)	−0.46	0.05	−4.8%	−0.51	0.11	−4.6%	−0.60	0.10	−2.7%
ACEXP	− (H4)	−2.44	0.03	−3.0%	−2.36	0.06	−2.6%	−2.20	0.05	−1.4%
FE_NOSCALE	−				−9.52	0.12	−4.6%			
FE_NOSCALE × *FEE*[c]	+/−				−5.27	0.40	−5.4%			
FE_SCALED	−							−3.02	0.01	−2.9%
FE_SCALED × *FEE*[c]	+/−							−1.74	0.02	−3.1%
ACMTG	+/−	0.21	0.02	6.3%	0.23	0.02	6.5%	0.24	0.01	4.0%
MIS_TYPE	+	1.10	< 0.01	6.7%	1.13	< 0.01	6.0%	1.13	0.01	3.5%
TENURE	+/−	1.84	0.01	8.0%	1.64	0.02	6.3%	1.45	0.05	3.3%
BIG_N	+/−	1.31	0.20	4.7%	1.14	0.28	3.8%	1.21	0.23	2.2%
BLOCK	−	0.94	0.22	4.3%	1.07	0.18	4.1%	1.46	0.09	2.9%
CEOCOB	+	−0.32	0.29	−2.0%	−0.26	0.66	−1.4%	−0.71	0.25	−2.4%
INVREC	+/−	2.33	0.14	4.1%	1.89	0.27	2.8%	0.81	0.66	0.6%
AGE	+/−	−1.06	0.18	−1.8%	−1.15	0.16	−1.7%	−1.54	0.11	−1.2%
MW	+/−	−0.56	0.55	−2.8%	−0.79	0.52	−3.1%	−0.44	0.69	−1.1%
LITRISK	+/−	−0.63	0.39	−3.3%	−0.66	0.39	−3.0%	−1.38	0.24	−3.3%
LOSS	+/−	−1.25	0.09	−6.2%	−0.99	0.27	−4.4%	−0.57	0.52	−1.5%
SEG	+/−	0.64	0.09	3.9%	0.70	0.06	3.7%	0.87	0.07	2.9%
SALESGROWTH	+/−	−1.12	0.34	−1.4%	−1.26	0.28	−1.3%	−1.25	0.30	−0.8%
ASSETS	+/−	0.21	0.53	2.5%	0.15	0.66	1.5%	−0.09	0.81	−0.4%
Industry$_j$ and *Year*$_i$	NA									
Qualitatively material n				40			40			40
Qualitatively immaterial n				300			300			300
Pseudo R^2				0.28			0.30			0.32

Dependent variable, *QUAL_MATERIAL*, equals 1 if company reported basic EPS that meet or beat last analysts' median consensus forecast before earnings report, but would have missed the forecast if it corrected the income statement misstatement, 0 otherwise. All standard errors are clustered by company.

[a] Variable definitions are in Table 2. *ANALYST*, *FEE*, *FE_NOSCALE*, and *FE_SCALED* are mean centered.

[b] All p-values are two-tailed, except for those variables with directional expectations. Economic effects for continuous independent variables represent the effect of a one standard deviation increase from the mean (including adjusting the interaction term) has on the predicted probability of the dependent variable being 1 and for indicator variables represent the effect a change from 0 to 1 has on the predicted probability of the dependent variable being 1. The calculations include the constant, as well as all other model variables at their means (Long and Freese 2005). The economic effect for the interaction term represents the effect of a one standard deviation increase from the mean in both *ANALYST* and *FEE*, as well as the associated increase in the interaction of the two terms. The calculations include the constant, as well as all other model variables at their means.

[c] *FEE* denotes mean-centered *LNAUDIT*, *ABNORMALAUDIT*, *OFFICEAUDIT*, and *FIRMAUDIT*, respectively.

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TABLE 4 (continued)

Panel B: Marginal Effect of *ANALYST* for *LNAUDIT* as *FEE* Models

	Model 1a No Control for Forecast Error		Model 1b *FE_NOSCALE*		Model 1c *FE_SCALED*	
	Marg.	**p**	**Marg.**	**p**	**Marg.**	**p**
FEE = Mean − 1 sd	18%	< 0.01	16%	0.01	16%	< 0.01
FEE = Mean	7%	0.03	5%	0.09	5%	0.09
FEE = Mean + 1 sd	0%	0.43	0%	0.86	−1%	0.85
Interaction Effect Analyses						
Percent of neg. int. effects (Figure 2)		93%		89%		90%
1 sd decrease in *FEE* from mean at *ANALYST* mean (Figure 3)		19%		18%		15%
1 sd decrease in *FEE* from mean at *ANALYST* mean plus one sd (Figure 3)		44%		40%		42%

Panel C: *ABNORMALAUDIT* as *FEE*

		Model 1a No Control for Forecast Error			Model 1b *FE_NOSCALE*			Model 1c *FE_SCALED*		
Variable[a]	**Pred. Sign**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**
Constant		−0.98	0.70		−1.69	0.54		−0.74	0.79	
ANALYST[c]	+ (H1)	0.66	0.05	4.6%	0.53	0.11	1.9%	0.74	0.04	1.9%
FEE	+/− (H2)	−1.39	< 0.01	−3.0%	−2.36	0.01	−2.1%	−2.61	0.01	−1.4%
ANALYST × *FEE*[c]	+/− (H3)	−0.59	0.05	−3.4%	−0.75	0.03	−2.0%	−0.80	0.05	−1.4%
ACEXP	− (H4)	−2.82	0.01	−2.5%	−3.28	0.02	−1.5%	−2.77	0.02	−0.9%
FE_NOSCALE	−				−15.13	0.09	−2.3%			
FE_NOSCALE × *FEE*[c]	+/−				−21.23	0.04	−2.5%			
FE_SCALED	−							−3.88	0.02	−1.5%
FE_SCALED × *FEE*[c]	+/−							−4.14	0.03	−1.6%
ACMTG	+/−	0.25	< 0.01	6.3%	0.34	< 0.01	5.6%	0.32	< 0.01	3.4%
MIS_TYPE	+	1.04	< 0.01	4.8%	1.24	< 0.01	3.2%	1.22	< 0.01	2.0%
TENURE	+/−	1.86	0.01	6.1%	1.48	0.02	2.7%	1.18	0.10	1.5%
BIG_N	+/−	1.24	0.25	3.5%	0.66	0.55	1.2%	0.94	0.41	1.0%
BLOCK	−	0.90	0.23	3.2%	0.97	0.21	1.8%	1.35	0.10	1.5%
CEOCOB	+	−0.32	0.29	−1.5%	−0.31	0.59	−0.8%	−0.75	0.21	−1.4%
INVREC	+/−	1.99	0.20	2.6%	0.82	0.66	0.5%	0.06	0.97	0.0%
AGE	+/−	−0.81	0.29	−1.1%	−0.79	0.35	−0.6%	−0.81	0.38	−0.4%
MW	+/−	−1.71	0.10	−4.6%	−1.93	0.18	−2.6%	−1.54	0.25	−1.5%
LITRISK	+/−	−0.28	0.69	−1.2%	−0.27	0.71	−0.6%	−1.63	0.14	−2.0%
LOSS	+/−	−1.35	0.06	−5.1%	−1.16	0.20	−2.4%	−0.74	0.39	−1.0%
SEG	+/−	0.53	0.10	2.4%	0.68	0.04	1.8%	0.90	0.04	1.7%
SALESGROWTH	+/−	−0.67	0.56	−0.7%	−0.84	0.47	−0.4%	−0.71	0.55	−0.2%
ASSETS	+/−	−0.55	0.02	−2.9%	−0.55	0.02	−1.5%	−0.82	< 0.01	−1.2%

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TABLE 4 (continued)

Variable[a]	Pred. Sign	Model 1a No Control for Forecast Error Coeff.	p^b	Econ.	Model 1b *FE_NOSCALE* Coeff.	p^b	Econ.	Model 1c *FE_SCALED* Coeff.	p^b	Econ.
Industry$_j$ and *Year$_i$*	NA									
Qualitatively material n				40			40			40
Qualitatively immaterial n				300			300			300
Pseudo R^2				0.28			0.32			0.34

Dependent variable, *QUAL_MATERIAL*, equals 1 if company reported basic EPS that meet or beat last analysts' median consensus forecast before earnings report, but would have missed the forecast if it corrected the income statement misstatement, 0 otherwise. All standard errors are clustered by company.

[a] Variable definitions are in Table 2. *ANALYST*, *FEE*, *FE_NOSCALE*, and *FE_SCALED* are mean centered.

[b] All p-values are two-tailed, except for those variables with directional expectations. Economic effects for continuous independent variables represent the effect of a one standard deviation increase from the mean (including adjusting the interaction term) has on the predicted probability of the dependent variable being 1 and for indicator variables represent the effect a change from 0 to 1 has on the predicted probability of the dependent variable being 1. The calculations include the constant, as well as all other model variables at their means (Long and Freese 2005). The economic effect for the interaction term represents the effect of a one standard deviation increase from the mean in both *ANALYST* and *FEE*, as well as the associated increase in the interaction of the two terms. The calculations include the constant, as well as all other model variables at their means.

[c] *FEE* denotes mean-centered *LNAUDIT*, *ABNORMALAUDIT*, *OFFICEAUDIT*, and *FIRMAUDIT*, respectively.

Panel D: Marginal Effect of *ANALYST* for *ABNORMALAUDIT* as *FEE* Models

	Model 1a No Control for Forecast Error Marg.	p	Model 1b *FE_NOSCALE* Marg.	p	Model 1c *FE_SCALED* Marg.	p
FEE = Mean − 1 sd	14%	< 0.01	15%	< 0.01	11%	< 0.01
FEE = Mean	6%	0.04	5%	0.04	6%	0.01
FEE = Mean + 1 sd	1%	0.32	1%	0.83	2%	0.15
Interaction Effect Analyses						
Percent of neg. int. effects		97%		91%		94%
1 sd decrease in *FEE* from mean at *ANALYST* mean		9%		11%		9%
1 sd decrease in *FEE* from mean at *ANALYST* mean plus 1 sd		23%		27%		29%

Panel E: *OFFICEAUDIT* as *FEE*

Variable[a]	Pred. Sign	Model 1a No Control for Forecast Error Coeff.	p^b	Econ.	Model 1b *FE_NOSCALE* Coeff.	p^b	Econ.	Model 1c *FE_SCALED* Coeff.	p^b	Econ.
Constant		0.86	0.73		−1.48	0.57		0.98	0.69	
ANALYST[c]	+ (H1)	0.65	0.04	4.9%	0.57	0.05	2.7%	0.56	0.05	1.9%
FEE	+/− (H2)	−1.72	0.25	−1.8%	−3.39	0.06	−1.9%	−3.12	0.05	−1.3%
ANALYST × *FEE*[c]	+/− (H3)	0.68	0.50	2.8%	0.69	0.55	−0.2%	0.95	0.53	0.2%
ACEXP	− (H4)	−2.14	0.06	−2.3%	−1.91	0.09	−1.4%	−1.68	0.08	−0.9%

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TABLE 4 (continued)

		Model 1a No Control for Forecast Error			Model 1b *FE_NOSCALE*			Model 1c *FE_SCALED*		
Variable^{a}	**Pred. Sign**	**Coeff.**	**p**^{b}	**Econ.**	**Coeff.**	**p**^{b}	**Econ.**	**Coeff.**	**p**^{b}	**Econ.**
FE_NOSCALE	–				−12.63	0.04	−3.1%			
FE_NOSCALE × *FEE*^{c}	+/–				−35.14	0.04	−3.4%			
FE_SCALED	–							−3.62	< 0.01	−2.3%
FE_SCALED × *FEE*^{c}	+/–							−6.50	< 0.01	−2.3%
ACMTG	+/–	0.21	0.01	5.7%	0.26	< 0.01	5.0%	0.25	< 0.01	3.3%
MIS_TYPE	+	1.09	< 0.01	5.6%	1.17	< 0.01	4.0%	1.18	< 0.01	2.8%
TENURE	+/–	1.35	0.03	5.3%	1.22	0.04	3.2%	1.22	0.07	2.2%
BIG_N	+/–	0.04	0.96	0.2%	0.30	0.71	0.9%	0.07	0.93	0.2%
BLOCK	–	0.84	0.27	3.3%	1.25	0.09	2.9%	1.44	0.09	2.2%
CEOCOB	+	−0.24	0.34	−1.3%	−0.04	0.94	−0.1%	−0.44	0.49	−1.1%
INVREC	+/–	1.69	0.19	2.4%	1.44	0.26	1.3%	0.04	0.98	0.0%
AGE	+/–	−1.03	0.21	−1.5%	−1.04	0.24	−1.0%	−1.14	0.25	−0.7%
MW	+/–	−1.07	0.26	−3.8%	−1.35	0.25	−2.9%	−1.08	0.35	−1.7%
LITRISK	+/–	−0.31	0.68	−1.5%	−0.45	0.58	−1.4%	−1.80	0.08	−3.1%
LOSS	+/–	−1.18	0.09	−5.1%	−0.75	0.33	−2.2%	−0.60	0.40	−1.2%
SEG	+/–	0.14	0.66	0.6%	0.36	0.22	1.1%	0.40	0.34	0.8%
SALESGROWTH	+/–	−0.49	0.66	−0.5%	−0.39	0.70	−0.3%	−0.27	0.79	−0.1%
ASSETS	+/–	−0.39	0.14	−2.6%	−0.36	0.15	−1.6%	−0.65	0.03	−1.6%
Industry$_j$ and *Year*$_t$	NA									
Qualitatively material n				40			40			40
Qualitatively immaterial n				300			300			300
Pseudo R^2				0.22			0.27			0.29

Dependent variable, *QUAL_MATERIAL*, equals 1 if company reported basic EPS that meet or beat last analysts' median consensus forecast before earnings report, but would have missed the forecast if it corrected the income statement misstatement, 0 otherwise. All standard errors are clustered by company.

[a] Variable definitions are in Table 2. *ANALYST*, *FEE*, *FE_NOSCALE*, and *FE_SCALED* are mean centered.

[b] All p-values are two-tailed, except for those variables with directional expectations. Economic effects for continuous independent variables represent the effect of a one standard deviation increase from the mean (including adjusting the interaction term) has on the predicted probability of the dependent variable being 1 and for indicator variables represent the effect a change from 0 to 1 has on the predicted probability of the dependent variable being 1. The calculations include the constant, as well as all other model variables at their means (Long and Freese 2005). The economic effect for the interaction term represents the effect of a one standard deviation increase from the mean in both *ANALYST* and *FEE*, as well as the associated increase in the interaction of the two terms. The calculations include the constant, as well as all other model variables at their means.

[c] *FEE* denotes mean-centered *LNAUDIT*, *ABNORMALAUDIT*, *OFFICEAUDIT*, and *FIRMAUDIT*, respectively.

Panel F: Marginal Effect of *ANALYST* for *OFFICEAUDIT* as *FEE* Models

	Model 1a No Control for Forecast Error		Model 1b *FE_NOSCALE*		Model 1c *FE_SCALED*	
	Marg.	**p**	**Marg.**	**p**	**Marg.**	**p**
FEE = Mean − 1 sd	5%	0.14	4%	0.24	3%	0.29
FEE = Mean	5%	0.04	4%	0.07	4%	0.05
FEE = Mean + 1 sd	5%	0.05	4%	0.07	5%	0.04

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TABLE 4 (continued)

	Model 1a No Control for Forecast Error		Model 1b *FE_NOSCALE*		Model 1c *FE_SCALED*	
	Marg.	**p**	**Marg.**	**p**	**Marg.**	**p**
Interaction Effect Analyses						
Percent of neg. int. effects		55%		83%		67%
1 sd decrease in *FEE* from mean at *ANALYST* mean		3%		4%		2%
1 sd decrease in *FEE* from mean at *ANALYST* mean plus 1 sd		3%		5%		2%

Panel G: *FIRMAUDIT* as *FEE*

		Model 1a No Control for Forecast Error			Model 1b *FE_NOSCALE*			Model 1c *FE_SCALED*		
Variable[a]	**Pred. Sign**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**
Constant		−0.37	0.89		−1.78	0.53		−0.11	0.97	
ANALYST[c]	+ (H1)	0.69	0.03	6.8%	0.54	0.08	4.1%	0.62	0.05	3.2%
FEE	+/− (H2)	−4.36	0.10	−3.9%	−5.93	0.05	−3.8%	−7.06	0.02	−2.6%
ANALYST × *FEE*[c]	+/− (H3)	−2.98	0.06	−3.4%	−3.41	0.12	−4.0%	−3.86	0.08	−2.7%
ACEXP	− (H4)	−2.20	0.05	−3.2%	−2.03	0.08	−2.4%	−1.75	0.09	−1.3%
FE_NOSCALE	−				−11.08	0.07	−5.0%			
FE_NOSCALE × *FEE*[c]	+/−				−45.12	0.15	−5.7%			
FE_SCALED	−							−3.64	< 0.01	−3.5%
FE_SCALED × *FEE*[c]	+/−							−12.43	0.01	−3.6%
ACMTG	+/−	0.19	0.02	6.4%	0.22	0.01	6.4%	0.22	0.01	4.2%
MIS_TYPE	+	0.94	0.01	6.3%	1.02	0.01	5.6%	1.05	0.01	3.7%
TENURE	+/−	1.72	0.01	8.5%	1.54	0.02	6.3%	1.40	0.04	3.7%
BIG_N	+/−	−0.24	0.77	−1.8%	0.23	0.80	1.2%	0.60	0.54	1.6%
BLOCK	−	0.93	0.24	4.8%	1.26	0.11	4.9%	1.65	0.07	3.7%
CEOCOB	+	−0.43	0.23	−3.1%	−0.26	0.67	−1.5%	−0.74	0.23	−2.9%
INVREC	+/−	2.10	0.15	4.0%	1.88	0.22	2.9%	0.50	0.77	0.4%
AGE	+/−	−1.04	0.20	−2.0%	−1.23	0.15	−1.8%	−1.56	0.10	−1.4%
MW	+/−	−0.72	0.44	−3.8%	−1.00	0.40	−3.9%	−0.55	0.62	−1.6%
LITRISK	+/−	−0.67	0.38	−4.0%	−0.67	0.41	−3.2%	−1.84	0.10	−4.9%
LOSS	+/−	−1.34	0.06	−7.5%	−0.98	0.27	−4.6%	−0.71	0.39	−2.2%
SEG	+/−	0.31	0.38	1.8%	0.46	0.19	2.4%	0.60	0.16	2.1%
SALESGROWTH	+/−	−0.96	0.38	−1.4%	−1.24	0.27	−1.4%	−1.17	0.30	−0.8%
ASSETS	+/−	−0.10	0.75	−1.1%	−0.10	0.76	−0.9%	−0.36	0.31	−1.6%
Industry$_j$ and *Year*$_i$	NA									
Qualitatively material n				40			40			40
Qualitatively immaterial n				300			300			300
Pseudo R^2				0.24			0.27			0.30

Dependent variable, *QUAL_MATERIAL*, equals 1 if company reported basic EPS that meet or beat last analysts' median consensus forecast before earnings report, but would have missed the forecast if it corrected the income statement misstatement, 0 otherwise. All standard errors are clustered by company.

[a] Variable definitions are in Table 2. *ANALYST*, *FEE*, *FE_NOSCALE*, and *FE_SCALED* are mean centered.

(continued on next page)

TABLE 4 (continued)

[b] All p-values are two-tailed, except for those variables with directional expectations. Economic effects for continuous independent variables represent the effect of a one standard deviation increase from the mean (including adjusting the interaction term) has on the predicted probability of the dependent variable being 1 and for indicator variables represent the effect a change from 0 to 1 has on the predicted probability of the dependent variable being 1. The calculations include the constant, as well as all other model variables at their means (Long and Freese 2005). The economic effect for the interaction term represents the effect of a one standard deviation increase from the mean in both *ANALYST* and *FEE*, as well as the associated increase in the interaction of the two terms. The calculations include the constant, as well as all other model variables at their means.

[c] *FEE* denotes mean-centered *LNAUDIT*, *ABNORMALAUDIT*, *OFFICEAUDIT*, and *FIRMAUDIT*, respectively.

Panel H: Marginal Effect of *ANALYST* for *FIRMAUDIT* as *FEE* Models

	Model 1a No Control for Forecast Error		**Model 1b *FE_NOSCALE***		**Model 1c *FE_SCALED***	
	Marg.	**p**	**Marg.**	**p**	**Marg.**	**p**
FEE = Mean − 1 sd	17%	0.02	17%	0.04	12%	0.01
FEE = Mean	7%	0.03	6%	0.05	6%	0.01
FEE = Mean + 1 sd	1%	0.40	0%	0.92	2%	0.59
Interaction Effect Analyses						
Percent of neg. int. effects		99%		97%		96%
1 sd decrease in *FEE* from mean at *ANALYST* mean		8%		10%		7%
1 sd decrease in *FEE* from mean at *ANALYST* mean plus 1 sd		27%		27%		26%

fees are negatively associated with the probability of managers waiving a qualitatively material misstatement. Again, these results must be interpreted in light of the interaction between analyst following and audit fees, which we discuss below.

H3 predicts the interaction between analyst following and audit fees. The results across Models 1a–1c in Panels A, C, E, and G of Table 4 are consistent with a negative interaction between analyst following and audit fees in models in which the independent variable is the level of audit fees (*LNAUDIT*: $p = 0.05$, $p = 0.11$, $p = 0.10$, respectively), abnormal audit fees (*ABNORMALAUDIT*: $p = 0.05$, $p = 0.03$, $p = 0.05$, respectively), and the firm rank of audit fees (*FIRMAUDIT*: $p = 0.06$, $p = 0.12$, $p = 0.08$, respectively), but not the office rank of audit fees (*OFFICEAUDIT*: $p = 0.50$, $p = 0.55$, $p = 0.53$, respectively). However, Ai and Norton (2003) and Greene (2010) demonstrate that the coefficient on an interaction term is the "partial interaction," such that its significance should not be interpreted as necessarily indicative of the presence or absence of an interaction effect.[22]

Ai and Norton (2003) state that to better understand interaction effects, one should graphically examine the interaction effects of the individual sample observations. In our context, an interaction effect is the effect that a change in *FEE* or *ANALYST* has on the partial effect of the other variable on the probability that managers will waive a misstatement that, if corrected, would cause the company to miss analysts' consensus forecast. Figure 2 illustrates the interaction effect z-statistics for each observation in the *LNAUDIT* model (Norton et al. 2004).[23] The plots show that 93 percent, 89 percent, and 90 percent of the z-statistics are negative (Table 4, Panel B) for the *ANALYST* × *LNAUDIT* interaction in Models 1a, 1b, and 1c, respectively.

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[22] More specifically, Ai and Norton (2003, 124) illustrate that "the statistical significance of the interaction effect cannot be tested with a simple t-test on the coefficient of the interaction term β_{12}." Ai and Norton (2003) further illustrate that understanding the direction and magnitude of interactions in logit models requires more than a t-test of the coefficient on the interaction term because the interaction effect can be dependent on the other independent variables in the model. The interaction effect can thus have a direction that is opposite of the coefficient's sign. Analyzing only the marginal effect of the interaction term would be: $(\partial\varphi(u))/\partial(x_1x_2) = \beta_{12}\varphi'(u)$. Norton et al. (2004) calculate the interaction effect for each observation in the sample using the cross-partial derivative of the expected value of y as: $[(\partial^2\phi(u))/(\partial x_1\partial x_2)] = \beta_{12}\varphi'(u) + (\beta_1 + \beta_{12}x_2)(\beta_2 + \beta_{12}x_1)\varphi''(u)$.

Greene (2010) cautions against overreliance on statistical measures such as Ai and Norton's (2003) interaction effects and recommends using an alternative graphical analysis to understand the economic significance of interactions.[24] Following this advice, in Figure 3 we depict the negative *ANALYST* × *LNAUDIT* interactions, and in Panels B, D, F, and H of Table 4 we summarize the economic effects for Model 1.[25] Using the specification in Model 1a shows that the predicted probability that a waived misstatement allows companies to meet or beat analysts' consensus forecasts increases 19 percent when *ANALYST* is at its mean and *LNAUDIT* decreases one standard deviation from its mean, but increases 44 percent when *ANALYST* is one standard deviation above its mean and *LNAUDIT* decreases one standard deviation from its mean. These increases are economically significant. According to Greene (2010, 294), one can interpret an interaction effect in Figure 3 as "the change in the distance between the two sets of predicted probabilities." For example, the interaction effect in Model 1a using *LNAUDIT* is 25 percent (calculated as 44 percent − 19 percent). This interaction effect is also economically significant. Results using the specifications in Model 1b and 1c yield similar results. Figure 3 thus further confirms the presence of a negative *ANALYST* × *FEE* interaction. Taken together, the interaction effects that we document are consistent with the importance of the joint role of managers' and auditors' incentives on the waiving of qualitatively material misstatements.[26]

Turning to our last hypothesis, H4 predicts that audit committees with more financial expertise decrease the likelihood that managers will waive material misstatements. Supporting H4, we find that audit committee financial expertise is negatively related to the likelihood of managers waiving a qualitatively material misstatement in models using each of the audit fee measures and each of the model specifications across Models 1a–1c in Panels A, C, E, and G of Table 4 (*LNAUDIT*: $p = 0.03$, $p = 0.06$, and $p = 0.05$ for Models 1a, 1b, and 1c, respectively), (*ABNORMALAUDIT*: $p = 0.01$, $p = 0.02$, $p = 0.02$, respectively), (*OFFICEAUDIT*: $p = 0.06$, $p = 0.09$, $p = 0.08$, respectively), and (*FIRMAUDIT*: $p = 0.05$, $p = 0.08$, $p = 0.09$, respectively).[27] In terms of control variables, the results across Models 1a–1c in Panels A, C, E, and G of Table 4 show that forecast error is negatively

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[23] The interaction effect z-statistics show a similar pattern when using the *ABNORMALAUDIT* and *FIRMAUDIT* variables. To avoid repetition, we report plots using the variable *LNAUDIT* as a prototypical example. Plots for the other variables are available from the authors upon request. Table 4, Panel D shows that 97 percent, 91 percent, and 94 percent of the z-statistics are negative for the *ANALYST* × *ABNORMALAUDIT* interaction in Models 1a, 1b, and 1c, respectively. Further, 99 percent, 97 percent, and 96 percent of the z-statistics are negative (Table 4, Panel H) for the *ANALYST* × *FIRMAUDIT* interaction in Models 1a, 1b, and 1c, respectively.

[24] Greene (2010, 295) "does not fault Ai and Norton's (2003) suggested calculations. Rather, we argue that the process of statistical effects about partial effects, and interaction terms in particular, produces generally uninformative and sometimes contradictory and misleading results. The mechanical reliance on statistical measures of significance obscures the economic, numerical content of the estimated model."

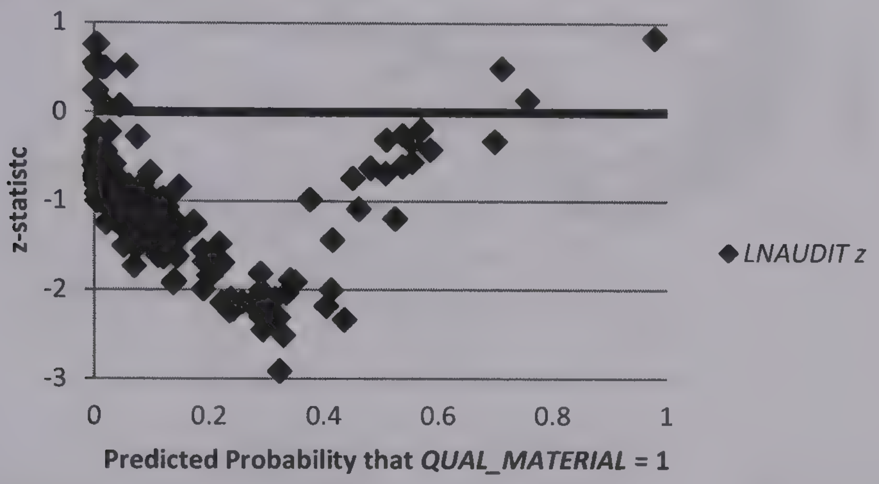
[25] The plots depicting the *ANALYST* × *ABNORMALAUDIT* and the *ANALYST* × *FIRMAUDIT* interaction effects show a similar pattern, and are available from the authors upon request.

[26] To examine the robustness of our results to the historical materiality approach, we re-run the analyses in Tables 4 and 5 using an indicator variable for companies using the income statement approach and an indicator variable for companies using the balance sheet approach, with the intercept being no disclosure regarding the prior approach. These variables are not significant ($p > 0.10$) in our models and do not change our inferences.

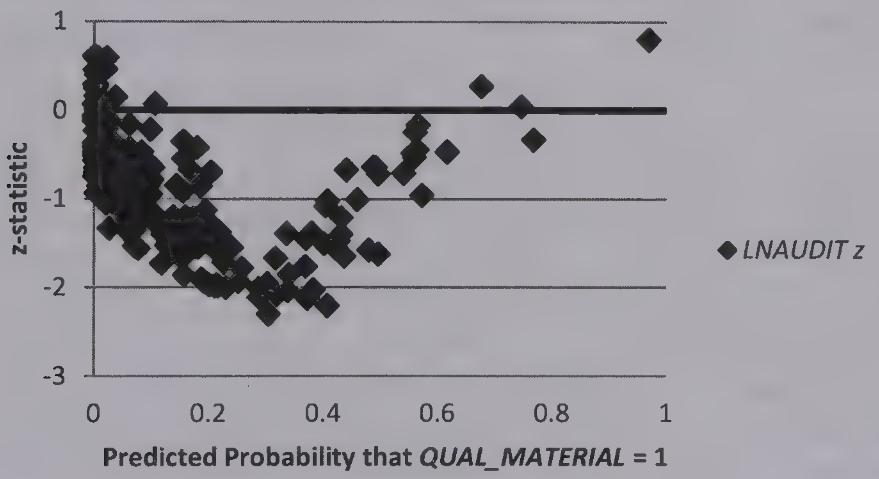
[27] We also tested whether audit committee financial expertise might mitigate managers' incentives to waive qualitatively material misstatements, i.e., the interaction of *ACEXP* × *ANALYST*. In untabled results, we find that this variable is not significant ($p > 0.10$) in our models and other results are essentially unchanged.

FIGURE 2
z-Statistics for *ANALYST* × *LNAUDIT* Interaction Effect

Panel A: Model 1a (93 percent of z-statistics are negative)



Panel B: Model 1b (89 percent of z-statistics are negative)



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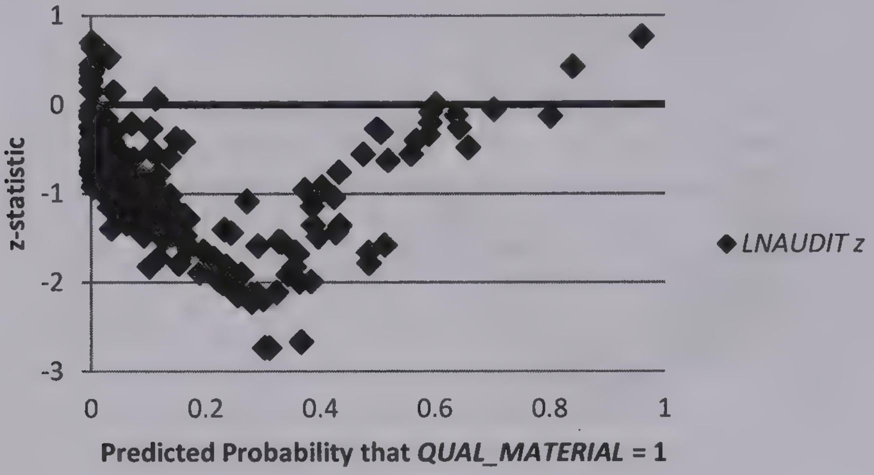
associated with *QUAL_MATERIAL*, and that the number of audit committee meetings, the presence of complex misstatements, and audit firm tenure are positively associated with *QUAL_MATERIAL*.

Models 2 and 3: Quantitative Materiality Tests (*QUANT_MATERIAL_PRETAX* and *QUANT_MATERIAL_NORM*)

We provide further tests of H2 and H4 using two measures of quantitative materiality, namely, pretax net income (Table 5, Panel A) and normal income (Table 5, Panel B). Consistent with the results that obtain using our measure of qualitative materiality, we find that audit fees are negatively related to the likelihood that auditors will allow managers to waive a quantitatively material misstatement in models using the level of audit fees (*LNAUDIT*: $p = 0.04$ and $p = 0.05$ in Panels A and B, respectively), abnormal audit fees (*ABNORMALAUDIT*: $p = 0.07$ and $p = 0.07$), and firm rank of audit fees (*FIRMAUDIT*: $p = 0.02$ and $p = 0.10$).

FIGURE 2 (continued)

Panel C: Model 1c (90 percent of z-statistics are negative)



This figure produced by the "inteff" command in Stata (Norton et al. 2004) plots observation level z-statistics for the *ANALYST* × *LNAUDIT* interaction effects by moving *ANALYST*, *LNAUDIT*, and *ANALYST* × *LNAUDIT* with all other variables at their means. The figure illustrates that Model 1a, 1b, and 1c interaction effects are predominantly negative.

The results also show that audit committee financial expertise is negatively related to the likelihood of managers waiving a quantitatively material misstatement in models using each of the audit fee measures (*LNAUDIT*: p = 0.07 and p = 0.05 in Panels A and B, respectively; *ABNORMALAUDIT*: p = 0.07 and p = 0.05; *OFFICEAUDIT*: p = 0.07 and p = 0.04; and *FIRMAUDIT*: p = 0.07 and p = 0.05). In terms of control variables, the results across Panels A–B of Table 5 show that higher levels of inventory and receivables, higher sales growth (Panel A only), and larger companies are negatively associated with *QUANT_MATERIAL*, and that the presence of complex misstatements, the presence of large blockholders, and a recent loss are positively associated with *QUANT_MATERIAL*.

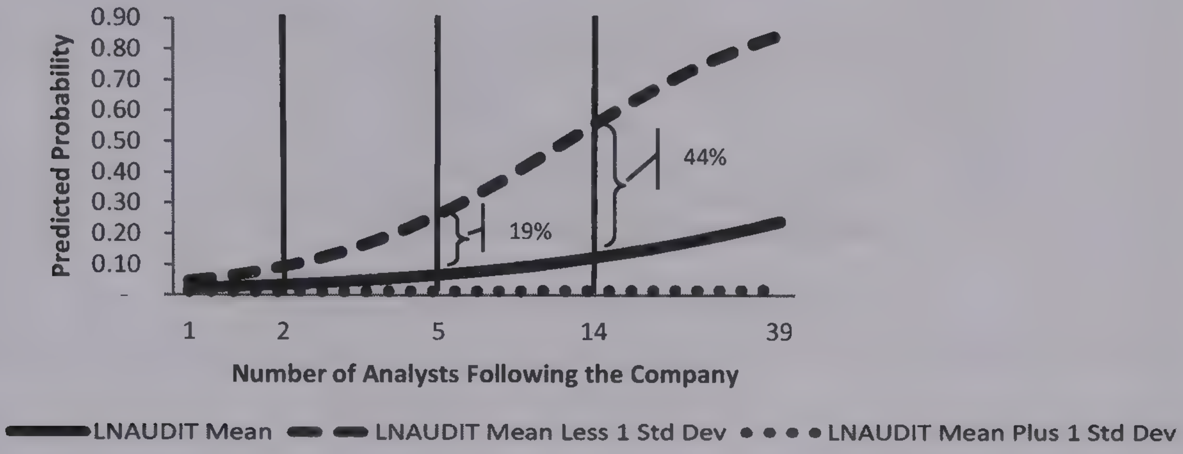
Robustness Checks

Audit Firms, Audit Fees, and Client Quality

The results in Tables 4 and 5 show that our measures of audit fees are negatively related to the likelihood that managers will waive a material misstatement. We interpret this relation as indicating that auditors who receive higher audit fees protect their reputations by decreasing the extent to which they allow managers to waive material misstatements. An alternative explanation is that audit firms that charge higher fees accept higher quality clients that are less likely to have material misstatements. The extant literature generally accepts the definition of a "high-fee auditor" as a Big N auditor (Francis 2004). Given that Big N firms charge higher audit fees than non-Big N firms (Palmrose 1986), we examine this alternative explanation by re-estimating our models in Tables 4 and 5, limiting our sample to Big N client companies. We find essentially the same results in untabulated analyses, supporting our original interpretation.

FIGURE 3
Illustration of *ANALYST* × *LNAUDIT* Interaction Effect

Panel A: Model 1a



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Managerial Compensation

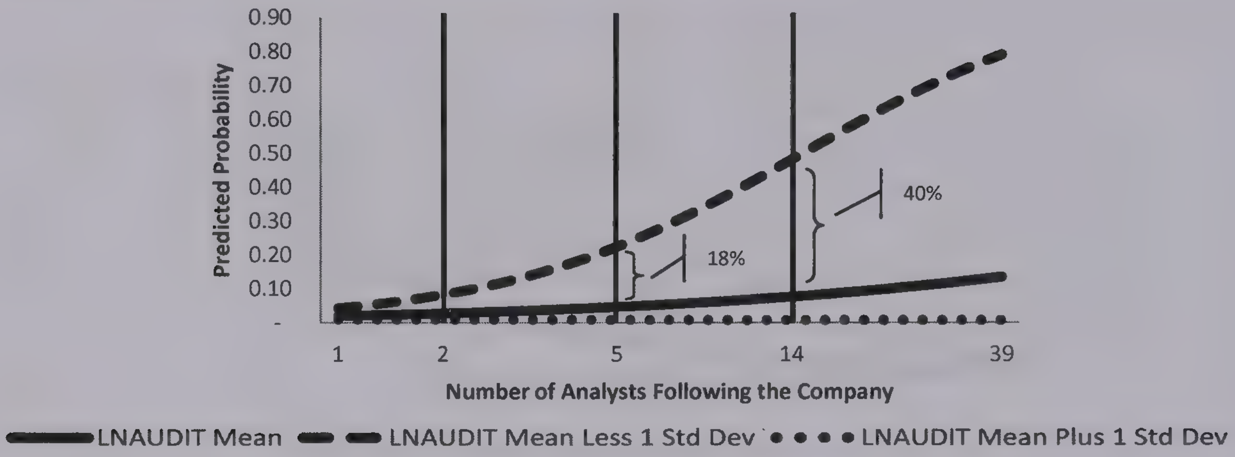
Managers face incentives to meet or beat earnings from their compensation contracts and performance goals (e.g., Burns and Kedia 2006; Efendi et al. 2007). However, Collins et al. (2009) show that companies penalize executives via lower bonuses for both low-quality financial reporting and subsequent restatements. Thus, while compensation contracts provide incentives for aggressive financial reporting, there are also competing incentives for accurate financial reporting (Desai et al. 2006). To examine the effects of compensation contracts on managers' incentives, we re-run the analyses in Table 4 and include the following compensation variables: the ratio of CEO cash bonus to salary, the number of shares owned by the CEO deflated by total shares outstanding, the number of stock options paid to the CEO deflated by total shares outstanding, and the percentage of stock-based CEO compensation. Untabulated results show that these variables are not related to the likelihood of waiving qualitatively material misstatements ($p > 0.10$), with similar results for the hypothesis-testing variables.

V. CONCLUSIONS

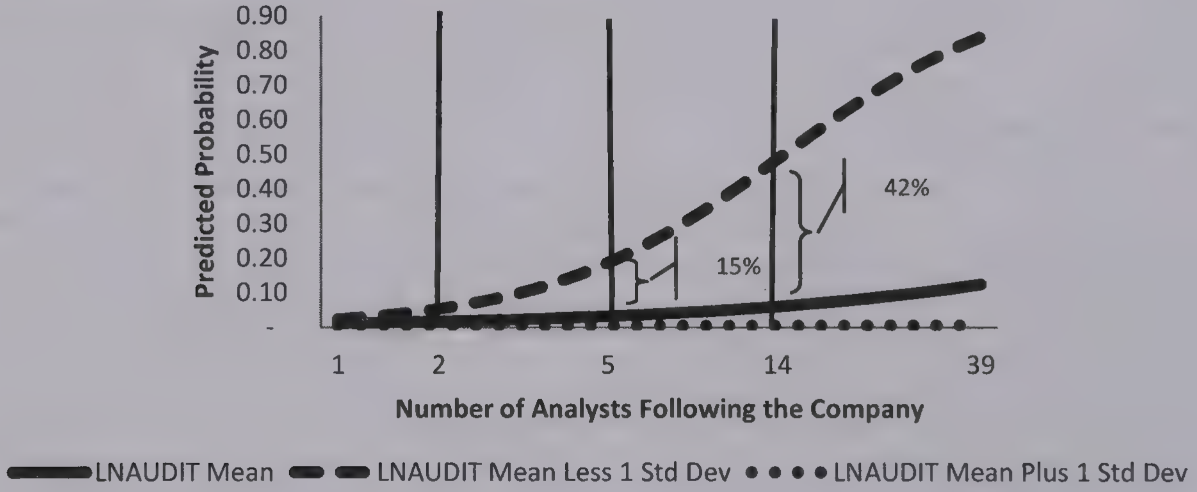
Using data derived from SAB 108 disclosures, we investigate how managers' and auditors' incentives, along with audit committee characteristics, affect qualitative and quantitative materiality judgments about misstatements. We acknowledge certain limitations to our analyses. First, the data include only those disclosures on misstatements that were initially judged immaterial under the regulations in effect at the time of detection and subsequently judged material under SAB 108. There are two other potential misstatement samples: (1) those misstatements that managers and auditors immediately deemed material and corrected upon detection, and (2) those misstatements that were waived when they were detected and that were not subsequently disclosed or corrected under SAB 108 even in instances when managers and auditors judged them to be material. These samples are of obvious theoretical interest, but the data necessary to examine such misstatements are not publicly available. Second, our sample includes a relatively small number of misstatements in relation to the overall number of transactions for public entities during this period, but we note

FIGURE 3 (continued)

Panel B: Model 1b



Panel C: Model 1c



The figures illustrate the effect that increasing *ANALYST*, a logged variable, from its minimum to its maximum at three levels of *LNAUDIT* has on the probability of *QUAL_MATERIAL* = 1. For ease of interpretation, the numbers on the horizontal axis are the unlogged number of analysts following each company. The values represent *ANALYST* at its minimum (1), its mean less one standard deviation (2), its mean (5), its mean plus one standard deviation (14), and its maximum (39). Brackets represent the change that a decrease in *LNAUDIT* from its mean to one standard deviation less than its mean has on the probability of *QUAL_MATERIAL* = 1 at two levels of *ANALYST*.

that other research in this area uses similar samples (e.g., Acito et al. 2009). While SAB 108 disclosure data may be limited to those companies that made such disclosures, such disclosures provide information and insights that would otherwise be unavailable.

We first show that managers are generally more likely to waive qualitatively material misstatements as analyst following increases, but this effect is primarily present when audit fees are relatively low. This finding implies that analyst pressure creates incentives for managers to use aggressive financial reporting in situations in which not correcting a misstatement can allow a company to meet or beat its consensus forecast, and auditors curtail this behavior for high-fee

TABLE 5

Logit Regression Models 2 and 3
QUANT_MATERIAL_PRETAX and *QUANT_MATERIAL_NORM*

Panel A: Model 2 and Quantitative Materiality Using Pretax Income

		LNAUDIT			*ABNORMALAUDIT*			*OFFICEAUDIT*			*FIRMAUDIT*		
Variable[a]	**Pred. Sign**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**	**Coeff.**	**p**[b]	**Econ.**
Constant		6.49	0.02		2.24	0.18		2.61	0.11		3.24	0.07	
FEE[c]	+/− (H2)	−0.42	0.04	−4.8%	−0.40	0.07	−3.0%	−0.71	0.25	−2.1%	−2.30	0.02	−4.4%
ACEXP	− (H4)	−1.01	0.07	−3.1%	−0.97	0.07	−3.0%	−0.96	0.07	−3.0%	−1.01	0.07	−3.1%
ACMTG	+/−	−0.04	0.37	−1.6%	−0.04	0.35	−1.7%	−0.05	0.19	−2.3%	−0.03	0.40	−1.5%
MIS_TYPE	+	1.02	< 0.01	7.1%	1.00	< 0.01	7.0%	1.03	< 0.01	7.2%	1.03	< 0.01	7.2%
TENURE	+/−	−0.41	0.27	−2.0%	−0.39	0.30	−1.9%	−0.39	0.31	−1.9%	−0.41	0.28	−2.0%
BIG_N	+/−	0.32	0.53	1.1%	0.31	0.54	1.1%	0.16	0.74	0.6%	−0.28	0.61	−0.9%
BLOCK	−	1.23	0.03	6.8%	1.24	0.03	6.9%	1.19	0.03	6.5%	1.23	0.03	6.8%
CEOCOB	+	−0.01	0.49	−0.1%	0.00	0.50	0.0%	0.07	0.42	0.4%	0.00	0.50	0.0%
INVREC	+/−	−2.93	< 0.01	−6.5%	−2.94	< 0.01	−6.5%	−2.70	0.01	−6.1%	−2.94	< 0.01	−6.5%
AGE	+/−	−0.04	0.93	−0.2%	−0.03	0.94	−0.2%	0.00	1.00	0.0%	−0.08	0.86	−0.4%
MW	+/−	0.09	0.86	0.3%	−0.16	0.74	−0.6%	0.01	0.99	0.0%	0.08	0.86	0.3%
LITRISK	+/−	0.26	0.56	1.3%	0.26	0.56	1.3%	0.33	0.46	1.7%	0.22	0.64	1.1%
LOSS	+/−	0.87	0.06	4.9%	0.77	0.09	4.3%	0.72	0.13	4.0%	0.85	0.06	4.9%
SEG	+/−	0.28	0.38	3.0%	0.20	0.51	2.1%	0.22	0.48	2.3%	0.26	0.41	2.8%
SALESGROWTH	+/−	−2.33	0.01	−4.9%	−2.32	0.01	−4.9%	−2.13	0.02	−4.6%	−2.36	0.01	−5.0%
ASSETS	+/−	−0.43	0.01	−6.3%	−0.64	< 0.01	−8.4%	−0.59	< 0.01	−7.9%	−0.44	< 0.01	−6.4%
Industry$_j$ and *Year*$_i$	NA												
Quantitatively material n		113			113			113			113		
Quantitatively immaterial n		470			470			470			470		
Pseudo R^2		0.24			0.24			0.24			0.24		

The dependent variable, *QUANT_MATERIAL_PRETAX*, equals 1 if abs(larger of income statement or balance sheet misstatement amount/pretax income using Compustat "pi") is $\geq$ 5 percent, 0 otherwise. All standard errors are clustered by company.

[a] Variable definitions are in Table 2.

[b] All p-values are two-tailed, except for those variables with directional expectations. Economic effects for continuous independent variables represent the effect a one standard deviation increase from the mean has on the predicted probability of the dependent variable being 1 and for indicator variables represent the effect a change from 0 to 1 has on the predicted probability of the dependent variable being 1. The calculations include the constant, as well as all other model variables at their means (Greene 2008; Long and Freese 2005).

[c] *FEE* denotes *LNAUDIT*, *ABNORMALAUDIT*, *OFFICEAUDIT*, and *FIRMAUDIT*, respectively.

(continued on next page)

TABLE 5 (continued)

Panel B: Model 3 and Quantitative Materiality Using Normal Income

		LNAUDIT			*ABNORMALAUDIT*			*OFFICEAUDIT*			*FIRMAUDIT*		
Variable[a]	Pred. Sign	Coeff.	p[b]	Econ.	Coeff.	p[b]	Econ.	Coeff.	p[b]	Econ.	Coeff.	p[b]	Econ.
Constant		4.52	0.03		0.92	0.41		1.37	0.21		1.63	0.15	
FEE[c]	+/– (H2)	–0.36	0.05	–6.5%	–0.35	0.07	–4.0%	–0.24	0.67	–1.0%	–1.42	0.10	–4.2%
ACEXP	– (H4)	–0.95	0.05	–4.1%	–0.93	0.05	–4.0%	–0.97	0.04	–4.2%	–0.96	0.05	–4.2%
ACMTG	+/–	0.01	0.39	1.3%	0.01	0.37	1.4%	0.00	0.87	0.3%	0.00	0.75	0.5%
MIS_TYPE	+	0.82	< 0.01	7.5%	0.81	< 0.01	7.4%	0.81	<0.01	7.4%	0.81	< 0.01	7.4%
TENURE	+/–	–0.17	0.59	–1.2%	–0.15	0.63	–1.1%	–0.15	0.62	–1.1%	–0.18	0.56	–1.3%
BIG_N	+/–	–0.17	0.66	–0.9%	–0.19	0.62	–1.1%	–0.30	0.45	–1.7%	–0.50	0.23	–2.7%
BLOCK	–	0.89	0.02	6.0%	0.90	0.02	6.1%	0.86	0.02	5.8%	0.88	0.02	5.9%
CEOCOB	+	–0.28	0.35	–2.1%	–0.27	0.36	–2.0%	–0.28	0.33	–2.1%	–0.26	0.36	–2.0%
INVREC	+/–	–3.41	< 0.01	–10.6%	–3.44	< 0.01	–10.7%	–3.28	< 0.01	–10.4%	–3.39	< 0.01	–10.6%
AGE	+/–	0.08	0.76	0.6%	0.09	0.75	0.6%	0.07	0.79	0.5%	0.08	0.78	0.5%
MW	+/–	–0.40	0.31	–2.1%	–0.62	0.11	–3.2%	–0.48	0.21	–2.5%	–0.41	0.29	–2.1%
LITRISK	+/–	–0.26	0.47	–1.7%	–0.27	0.46	–1.7%	–0.26	0.49	–1.7%	–0.29	0.43	–1.8%
LOSS	+/–	1.52	< 0.01	14.6%	1.44	< 0.01	13.7%	1.34	< 0.01	12.7%	1.46	< 0.01	13.9%
SEG	+/–	0.34	0.14	5.0%	0.27	0.22	4.1%	0.26	0.26	3.8%	0.30	0.19	4.5%
SALESGROWTH	+/–	–0.71	0.24	–2.6%	–0.72	0.24	–2.7%	–0.68	0.25	–2.6%	–0.69	0.25	–2.6%
ASSETS	+/–	–0.26	0.08	–6.5%	–0.45	< 0.01	–9.9%	–0.41	< 0.01	–9.5%	–0.32	0.02	–7.7%
$Industry_j$ and $Year_i$	NA												
Quantitatively material n		243			243			243			243		
Quantitatively immaterial n		540			540			540			540		
Pseudo R^2		0.22			0.22			0.22			0.22		

The dependent variable, *QUANT_MATERIAL_NORM*, equals 1 if abs(larger of income statement or balance sheet misstatement amount/normal income) is $\geq$ 5 percent, 0 otherwise. All standard errors are clustered by company.

[a] Variable definitions are in Table 2.

[b] All p-values are two-tailed, except for those variables with directional expectations. Economic effects for continuous independent variables represent the effect a one standard deviation increase from the mean has on the predicted probability of the dependent variable being 1 and for indicator variables represent the effect a change from 0 to 1 has on the predicted probability of the dependent variable being 1. The calculations include the constant, as well as all other model variables at their means (Greene 2008; Long and Freese 2005).

[c] *FEE* denotes *LNAUDIT*, *ABNORMALAUDIT*, *OFFICEAUDIT*, and *FIRMAUDIT*, respectively.

clients. Thus, we extend prior research that finds that managers use materiality justifications to strategically achieve desirable financial reporting outcomes (Acito et al. 2009).

We next show that auditors are less likely to allow managers to waive both qualitatively and quantitatively material misstatements as audit fees increase. Thus, our results are consistent with the theory of auditor reputation protection, which suggests that auditors make materiality judgments that are consistent with their incentives to avoid second-guessing by regulators and other third parties. We posit that the mechanism through which this occurs is audit effort. Audit fees that are higher overall, higher than "normal," and higher in terms of audit firm importance provide the audit firm greater ability to obtain the evidence necessary to talk managers out of relatively aggressive financial reporting (Kinney et al. 2004). Thus, while higher audit fees may provide incentives for auditors to yield to client pressures, they also increase the visibility of auditors' judgments in this era of publicly available audit fee data and thus increase auditors' incentives to encourage managers to yield to auditor demands for reputation protection when auditors' exposure to that visibility is high. This interpretation is consistent with Bedard and Johnstone (2004), who find a positive association between audit effort, fees, and earnings management risk, which auditors judge to be commonly associated with a history of exactly meeting analysts' consensus forecasts.

The results show further that managers' incentives associated with analyst following are mitigated by auditors' concerns about their own reputations. Specifically, auditors are less likely to allow managers to waive qualitatively material misstatements as audit fees increase. Indeed, the effects of the pressure created by analyst following on managers' waiving of qualitatively material misstatements are primarily associated with clients having relatively low audit fees. These results imply that, during the post-SOX period, a company's economic importance to the audit firm provides auditors with incentives to invest in reputational capital.

Finally, we provide evidence on the role of audit committee oversight, reporting that audit committees with greater finance and accounting expertise are less likely to allow managers to waive material misstatements compared to those with less expertise. Taken together, the results of this study shed light on the complex interplay between analyst following, the pressure that managers face to manage earnings, the pressure that auditors face to protect their reputations in the post-SOX environment, and the important role that audit committees can play in settings in which managers may act strategically to achieve desired financial reporting outcomes.

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APPENDIX A

DEPENDENT VARIABLE MEASUREMENTS AND CALCULATIONS: AN EXAMPLE

Panel A: Misstatement Quantification

	12/31/2003	12/31/2004	12/31/2005
Change in Misstatement	−525,000	−500,000	−300,000
Cumulative Misstatement Amount	−525,000	−1,025,000	−1,325,000

"Change in Misstatement" refers to the income statement effect of the misstatement in year *t*. "Cumulative Misstatement Amount" refers to the misstatement amount that has accumulated by the end of year *t*.

Panel B: Qualitative Dependent Variable Measurement

	12/31/2003	12/31/2004	12/31/2005
Misstatement Amount [A]	−525,000	−500,000	−300,000
Common Shares Outstanding for EPS [B]	25,000,000	25,000,000	25,000,000
Misstatement Per Share Amount [C]	−0.02	−0.02	−0.01
Actual EPS Compared to Analyst Forecast [D]	0.03	0.01	0.02
Forecast Error if Misstatement Corrected [E]	0.01	−0.01	0.01
Dependent Variable [F]	0	1	0

Model 1 uses the misstatement amount that affects the income statement in year *t* [A]. We determine the per share misstatement amount [C] by dividing the misstatement amount [A] by the shares outstanding used to calculate earnings per share [B]. Then, we compare the amount by which the company either met or beat analysts' consensus forecast [D] to the per share misstatement amount [C] to arrive at the per share amount by which the company would have met, beat, or missed consensus EPS if it had corrected the misstatement in the current year [E]. If the company would have continued reporting meeting or beating analysts' consensus forecast after correcting the error, then the Model 1 dependent variable (*QUAL_MATERIAL*) is 0 [F]. If the company would have reported missing analysts' consensus forecast after correcting the error, then *QUAL_MATERIAL* is 1.

Panel C: Quantitative Dependent Variable Measurement

	12/31/2003	12/31/2004	12/31/2005
Misstatement Amount [A]	−525,000	−1,025,000	−1,325,000
Income Measure [B]	12,000,000	13,000,000	14,000,000
Misstatement as a Percentage of Income Measure [C]	−4.4%	−7.9%	−9.5%
Dependent Variable [D]	0	1	1

Models 2 and 3 use the larger of the misstatement amount that affects the income statement in year *t* or the misstatement amount that has accumulated by the end of year *t* [A]. We determine the relative percentage of the misstatement to the income measure [C] by dividing the misstatement amount [A] by the income measure [B]. If the misstatement amount is greater than or equal to 5 percent or more of the income measure, then the dependent variable [D] (*QUANT_MATERIAL_NORM* or *QUANT_MATERIAL_PRETAX*) is 1, otherwise it is 0.

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Voluntary Disclosure Incentives and Earnings Informativeness

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ABSTRACT: We propose that the value of the earnings reporting process as an information source lies in limiting delays in the release of bad news, either by inducing managers to disclose it voluntarily or by directly releasing the negative news that managers have incentives to withhold. We compare earnings informativeness in bad-news and good-news quarters. Using returns to measure news, we find, consistent with our prediction, that earnings informativeness relative to other sources is higher in bad-news quarters than in good-news quarters. Further, cross-sectional tests indicate that earnings differential informativeness in bad-news quarters is more pronounced when managers do not voluntarily disclose the news, information asymmetry is stronger, and managers are net sellers of stock.

Keywords: *earnings; earnings announcements; earnings informativeness; voluntary disclosure.*

JEL Classifications: *G3; M4; M40; M41; M48.*

Data Availability: *Data are available from Compustat, CRSP, First Call, I/B/E/S, ISSM, TAQ, and Thompson Financial.*

I. INTRODUCTION

There is a significant literature on the incremental information content of earnings announcements (Beaver 1968; Bamber 1987). More recently, studies such as Ball et al. (2011) and Beyer et al. (2010) argue that the primary value of earnings lies less in releasing

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information than in confirming information that has reached the market from alternative sources. Our paper bridges these two (non-mutually exclusive) streams of thought by proposing that the earnings reporting process is geared toward uncovering information that has not as yet been disclosed via alternative sources; in particular, negative information. This "*ex post* settling-up" via earnings is important because it limits managers' ability to delay the release of bad news in two ways. First, the prospect of having to announce earnings at the end of a fiscal period can sometimes induce managers to release bad news voluntarily (Skinner 1997), in which case, the primary role of earnings is confirmation, as in Ball et al. (2011).[1] Second, when managers do not disclose bad news voluntarily, earnings are instrumental in releasing the negative news, and the information content of earnings is important. Overall, the earnings reporting process results in timelier and more precise disclosure of bad news.

We attribute earnings' differential role in revealing bad news relative to good news to managers' voluntary disclosure decisions and the nature of the earnings reporting process. The literature has discussed several possible factors that are likely to provide managers with differential voluntary disclosure incentives with respect to good versus bad news. For example, managers have incentives to preemptively disclose bad news prior to earnings announcements via earnings forecasts over relatively short horizons.[2] Two issues are worth noting in this context. First, short-horizon forecasts are often prompted by the desire to manage the litigation risk and/or the market disappointment arising from the imminent release of the bad news at a forthcoming earnings announcement (Skinner 1994, 1997; Kasznik and Lev 1995; Soffer et al. 2000; Matsumoto 2002; Richardson et al. 2004; Field et al. 2005). Second, besides short-horizon forecasts, voluntary disclosure can also take the form of qualitative disclosures about the firms' operations, press releases about important contracts won or lost, etc. (Miller 2002). Miller (2002) documents that these alternative forms of voluntary disclosures tend to increase (decline) when firm performance improves (deteriorates), suggesting that they are used to convey good news about the firm.[3] In fact, there are a number of reasons discussed in the literature for why managers *delay* disclosures of bad news relative to good news.

Bad-news disclosures are often inherently associated with greater uncertainty in a firm's operations (Li 2006, 2008; Bloomfield 2008; Brown et al. 2009; Ertimur et al. 2011), and tend to be scrutinized more closely by capital market participants. This provides managers with motives to be more precise and careful with their estimates when they disclose bad news, introducing delays in the disclosure. Consistent with this, Graham et al. (2005) document in a survey that top-level executives acknowledge delaying bad-news disclosures to allow themselves time to study and interpret the negative information. Further, negative events often have disproportionately strong implications for stakeholders such as lenders because of asymmetric payoffs in their contracts with the firm (Fama and Miller 1972; Jensen and Meckling 1976). These contracts often rely on earnings performance (Fields et al. 2001; Asquith et al. 2005), increasing the stakeholders' demand for information on the impact of bad news on earnings. As a result, managers may be reluctant to disclose the bad news until they are able to provide a precise estimate of the effect of such news on earnings.

Second, even when the manager is relatively certain about the magnitude of bad news, she may choose to first take corrective action or wait for offsetting good news. In support of this, managers responding to Graham et al.'s (2005) survey admit to delaying bad-news disclosures in the hope that subsequent improvements eliminate the need to make a bad-news disclosure. Third, in some cases, managers can have self-serving motives to delay disclosure of bad news. Kothari et al. (2009) propose that managers' concerns about their careers, compensation packages, and stock-price-based wealth provide them with incentives to disclose good news early and delay bad-news disclosures.

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[1] Our evidence suggests that managers prefer to preemptively disclose negative information rather than wait for the earnings announcement, when the magnitude of the negative news is particularly large.

[2] Managers may also expedite bad news disclosures and delay good news disclosures out of a desire to obtain lower option exercise prices prior to option grants (Yermack 1997; Aboody and Kasznik 2000).

[3] Short-horizon forecasts exhibit a reverse pattern, consistent with such forecasts primarily conveying bad news.

The earnings reporting process plays an important role in the context of delayed voluntary disclosures of bad news. GAAP measurement principles provide guidelines for how to estimate and report losses in future cash flows via earnings components such as warranty and bad debt expenses, asset write-downs, etc. (Basu 1997). These measurement principles provide managers with tools for arriving at more precise estimates of bad news and their impact on earnings. Further, earnings reports are scrutinized by external fiduciary agents such as outside directors, auditors, and regulators. The checks and balances incorporated in the earnings reporting process imply that at least some news that has not been previously disclosed is likely to be released in earnings. Finally, earnings releases are closely scrutinized by investors and financial analysts. In particular, during earnings-related conference calls, analysts and investors question managers about the future implications of current earnings (Tasker 1998; Frankel et al. 1999). The evidence in Matsumoto et al. (2011) indicates that these conference calls are more informative when earnings announcements convey negative news. Further, the greater information content is more a result of scrutiny by capital market participants, such as financial analysts, rather than voluntary disclosures by managers.

In summary, if there are intra-quarter delays in the disclosure of bad news, then the announcement of earnings is expected to reveal at least some of the delayed bad news, with two possible consequences. In certain cases, for example, when the news is particularly negative, managers are prompted by its imminent release at earnings announcements to voluntarily disclose the news prior to the announcement. In the majority of cases when the news is not particularly negative and/or the litigation risk is not high enough to warrant early disclosure, the bad news is conveyed at the time of the earnings announcement. Consequently, when the overall news reaching the market in a quarter is negative, we expect it to be more concentrated around the announcement of earnings at quarter-end than when it is positive, particularly in instances that we observe no issuance of short-horizon forecasts.[4]

In our empirical analysis, we define an announcement quarter as extending from the end of the previous quarter's earnings announcement to the end of the current quarter's earnings announcement. Incremental news released by earnings is captured by returns in the three days around the current quarter's earnings announcement date. We compare the news that reaches the market during the three-day quarterly earnings announcement (QEA) window to that reaching the market during the non-QEA period in the quarter to determine earnings incremental informativeness relative to other information sources. Using a sample of 152,275 firm-quarters over the period 1987–2006, we find that earnings informativeness is higher in bad-news quarters than in good-news quarters.[5] The result is robust to using ranked values of our independent variable to address the issue of outliers, and to controlling for various factors, including firm fixed effects and any potential mechanical bias in our empirical measure. As predicted, earnings differential informativeness in bad-news quarters is concentrated among firm-quarters without short-horizon forecasts.

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[4] Importantly, the role of earnings with respect to negative versus positive information, which we examine empirically, is distinct from the one examined by Basu (1997). We focus on earnings' role in the *release* of bad news relative to good news, while Basu (1997) measures earnings' greater timeliness in *recognizing* bad news that may already have been released by other information sources.

[5] For the overall sample, the ratio of news released around QEAs to that in non-QEA periods is around 18 percent for bad-news periods and around 16 percent for good-news periods; that is, earnings are around 12 percent more informative in bad-news quarters.

Our cross-sectional analysis focuses on examining two situations in which earnings are a particularly important source of bad news. First, we predict that earnings play a more pronounced role in the release of bad news among firms with greater information asymmetry between managers and external investors. In such firms, managers are more likely to delay voluntarily releasing negative news, either because their private information is less observable to investors or because the magnitude of bad news is less measurable pending the completion of the earnings reporting process. Second, in periods in which managers sell stock, disclosures of bad (good) news would lead to more unfavorable (favorable) trade outcomes, making it more likely that managers delay disclosures of bad news relative to those of good news.[6] Thus, we predict that earnings differential informativeness is more pronounced among firm-quarters with net insider sales of stock. Our results support our predictions and indicate significant cross-sectional effects. When information asymmetry is in the highest decile (when there are net insider sales), the informativeness of earnings in bad-news quarters is higher than that in good news quarters by as much as 38 percent (23 percent).

In further tests, we find a non-monotonic pattern in earnings differential informativeness across the fiscal quarters, with a peak in the third quarter and a slight decline thereafter in the fourth quarter. We interpret the results as indicating a rise in earnings differential informativeness as the year-end audit approaches over the first three fiscal quarters. However, more intensified efforts at gathering information by market participants in the fourth quarter in anticipation of the imminent release of audited annual results, and more voluntary disclosures by managers in response possibly diminish the role of earnings as a disclosure medium in the fourth quarter.[7] In sensitivity analyses, we find that earnings' differential informativeness persists after excluding firm-quarter observations in which QEAs are accompanied by management forecasts.

The informativeness of earnings has received considerable attention in recent times. The verification and measurement rules underlying financial reporting, along with the scrutiny by fiduciary agents such as auditors and boards of directors, are generally thought to make earnings numbers untimely relative to other information sources and, hence, less informative (Ball and Shivakumar 2008; Ball et al. 2011; Beyer et al. 2010). Our findings indicate that earnings value as an information source does not arise solely from the absolute amount of news that earnings convey to the market. Instead, earnings' incremental usefulness lies in limiting delays in the release of negative news, either by inducing managers to disclose it voluntarily or by directly releasing the bad news that managers withhold. In either case, earnings' information role as discussed in this paper should be of particular interest to standard-setters as they establish fresh measurement principles under the new conceptual framework.

In Section II, we develop our hypotheses. Section III describes the data, empirical design, and descriptive statistics. Section IV discusses primary results, and Section V presents additional analyses. Section VI concludes.

II. HYPOTHESES DEVELOPMENT

Earnings Differential Informativeness

The literature discusses a number of circumstances under which managers can delay disclosures of bad news relative to those of good news. The earnings reporting process can limit delays in the release of negative news in two ways. First, the prospect of the eventual release of bad news at the time of the earnings announcement can induce managers to voluntarily disclose the bad news; for example, out of concern for litigation risk. To the extent that bad news remains undisclosed, as is possible when perceived litigation risk is low and/or managers' incentives to delay are more pronounced, the news is released at the time of the earnings announcement. Managers' disclosure incentives and the earnings reporting process are discussed in greater detail below, along with our empirical predictions.

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[6] There is considerable evidence that firm insiders possess private information at the time they sell stock (Seyhun 1986; Beneish and Vargus 2002; Ke et al. 2003; Piotroski and Roulstone 2005).

[7] For example, Baginski and Hassell (1990) report that not only do managers issue more forecasts during the fourth quarter, but also that fourth-quarter management forecasts are more influential in generating revisions in analyst estimates of future earnings relative to other quarters.

Greater scrutiny by capital markets upon bad-news disclosures, and additional demand for information on the impact of the bad news on earnings from debtholders (Fama and Miller 1972; Jensen and Meckling 1976), can make managers reluctant to disclose the bad news until they can arrive at more precise estimates of its impact. Second, upon observing bad news, managers can take corrective action or wait for offsetting good news before releasing the news to the market (Graham et al. 2005). Finally, a desire to capture private benefits or sustain higher firm valuations can make managers unwilling to disclose bad news immediately (Sletten 2012), consistent with the "stockpiling" of negative information referred to in Hutton et al. (2009). Such motives to delay bad-news disclosures can arise from managers' career and wealth-related concerns (Kothari et al. 2009), or from the tendency to "hype the stock" prior to capital market events such as seasoned equity offerings (Lang and Lundholm 2000).[8]

Disclosure of bad news that has not yet been released through other channels, including voluntary disclosures, is likely at the QEAs for a number of reasons. First, unlike most voluntary disclosures, such as management forecasts, financial statement information is scrutinized by outside directors and auditors before the earnings release. Auditors' and outside directors' reputations are tied to their ability to uncover adverse information. Failure to do so can result in lawsuits and regulatory penalties in addition to reputation loss, as with Arthur Andersen's failure to highlight irregular accounting practices at Enron in 2001. Second, financial reporting by listed firms is regulated, with costs for firms and managers via regulatory investigations, restatements, and personal penalties if accounting practices deviate significantly from acceptable norms (Dechow et al. 1996; Feroz et al. 1991; Palmrose et al. 2004). The checks and balances imposed on financial statements imply that to the extent that bad news affects contemporaneous earnings, managers' ability to conceal it, particularly across multiple successive periods, is likely limited (Xie 2001; Barton and Simko 2002). Finally, GAAP measurement rules provide a framework for the estimation of future cash flow losses based on managers' private information when reporting earnings, for example, via bad debt provisions, warranty expenses, inventory write-offs, etc. Such conservative measurement rules (Watts and Zimmerman 1976; Watts 2003) can amplify the role of earnings reports in releasing bad news.[9]

Delayed disclosures of bad news relative to good news, GAAP measurement principles and practices, and the checks and balances characterizing the earnings reporting process will result in earnings playing a greater role in the release of bad news than good news. Specifically, we predict that a greater proportion of news reaching the market in a given quarter is released at the time of the earnings announcement when the news is negative than when the news is positive. We use the sign of the quarterly return to proxy for the sign of the news reaching the market during the quarter. Earnings informativeness is measured as the proportion of news reaching the market at the time of the QEA relative to that during the non-QEA period in that quarter. Our first hypothesis can be formally stated as follows:

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[8] In a slightly different context, Miller (2002) points out that the general desire to maintain high valuations makes it more likely for firms to issue disclosures when they are experiencing performance gains than when they are experiencing performance declines.

[9] A substantial literature argues that conservative reporting and auditing practices arise in equilibrium as an offsetting mechanism to any propensity of managers to delay the release of bad news relative to that of good news (Devine 1963; Watts and Zimmerman 1976; Antle and Nalebuff 1991).

> **H1:** Earnings informativeness is higher when the overall news reaching the market during a quarter is negative than when it is positive.

We emphasize that H1 is based on earnings releasing new negative information to investors, rather than on recognizing negative information that investors are already aware of. Basu (1997) describes how one implication of conservatism in reporting and auditing practices is the asymmetric timeliness of earnings in recognizing bad news relative to recognizing good news. The key difference between the differential informativeness of earnings and asymmetric timeliness as investigated by Basu (1997) is that the former attempts to capture the timing of the news release within the quarter, while the latter does not. Specifically, earnings informativeness as measured in this paper is higher when the proportion of news released at the time of the QEA relative to the non-QEA period is greater. However, earnings asymmetric timeliness can be high even if all news in the quarter reaches the market prior to the QEA, but earnings *recognize* the bad news in a timelier manner than the good news. In a study that investigates earnings' incremental information content, McNichols (1988) documents that earnings announcement returns are less positively skewed than returns during similar-length windows in non-announcement periods. However, she also reports that the frequency of negative announcement returns is lower than that of negative non-announcement-period returns. As a result, this evidence is insufficient to conclude whether earnings are a more important source of bad news than good news.

Cross-Sectional Variation

In this section, we discuss cross-sectional predictions arising from H1. Our primary cross-sectional analyses focus on identifying situations in which bad news revelations from information sources other than earnings (e.g., voluntary disclosures) are likely to be limited. We focus on two cross-sectional partitions based on: (1) information asymmetry, and (2) net sale of stock by insiders.

With greater information asymmetry between managers and external investors, investors are less likely to be cognizant of managers' privately observed news, providing managers with greater opportunities to delay the release of negative private information. We expect information asymmetry between managers and investors to be higher among firms with smaller size, lower analyst following and institutional ownership, lower liquidity, and greater idiosyncratic volatility (Collins et al. 1987; Foster and Viswanathan 1993; Bushee and Noe 2000; Jiang et al. 2005). Such firms are also likely to operate in economic environments characterized by greater uncertainty. Greater uncertainty, in turn, can make managers delay disclosures of bad news until the earnings reporting process generates more precise estimates of bad news. In either case, we predict that the greater the information asymmetry, the more pronounced is the role of earnings in disclosing bad news relative to good news.

> **H2:** Earnings differential informativeness during bad-news quarters relative to good-news quarters is higher among firms with greater information asymmetry.

Our primary hypothesis (H1) does not necessarily require that managers are opportunistic or strategic with respect to their own short-term monetary incentives. The delays in bad-news disclosures could arise from perceived benefits of measuring the bad news with greater precision or attempting to reverse the bad news. However, managers' incentives can also be self-serving, as when managers are net sellers of stock in a given period. There is significant evidence in the literature that at the time of insider trades, managers possess undisclosed private information (Seyhun 1986; Noe 1999; Ke et al. 2003), particularly regarding future earnings (Beneish and

Vargus 2002; Piotroski and Roulstone 2005). We expect that managers' incentives to delay disclosures of any negative information they possess are stronger in the quarters in which they are also net sellers of stock. We accordingly test whether earnings' incremental role in releasing bad news relative to good news is more pronounced in firm-quarters with net insider sales.

H3: Earnings' differential informativeness during bad-news quarters relative to good-news quarters is higher in quarters with net insider sales of firm stock.

III. DATA, EMPIRICAL PROXIES, AND DESCRIPTIVE STATISTICS

Data

To construct our sample, we begin with all firm-quarters in Compustat with earnings announcement dates, and sufficient data to calculate market value of equity (MVE), book value, and leverage. We also require daily returns data from CRSP to compute quarterly returns, earnings announcement returns, and idiosyncratic return volatility. Analyst following is obtained from I/B/E/S, while institutional ownership and insider trade data are obtained from Thomson Financial, and are set equal to 0 when not available. Finally, bid-ask data are obtained from the Institute for the Study of Security Markets database (ISSM) for years 1987–1992, and from the NYSE Trades and Quotes database (TAQ) for years 1993–2006. Since common coverage in these databases starts in 1987, our sample includes 152,275 firm-quarters over the 80 quarters from 1987 to 2006, and comprises 10,204 individual firms. In additional tests, we supplement our data with management forecasts obtained from First Call. Since First Call provides little coverage in early years, our additional tests incorporate firm-quarters starting from 1995.

Measuring Informativeness of Earnings Announcements

We measure news using equity returns.[10] Earnings announcement returns (EAR_t) are defined as the market-adjusted buy-and-hold returns over the three days from Day −1 to Day +1, where Day 0 is the quarterly earnings announcement (*QEA*) date. Quarterly returns (RET_t) are defined as the market-adjusted buy-and-hold returns starting two days after the earnings announcement of Quarter $t-1$ and ending one day after the announcement of Quarter t.[11] The mean length of the period over which RET_t is computed is around 63 trading days. Non-earnings-announcement returns ($NEAR_t$) are estimated as $(1 + RET_t)/(1 + EAR_t) - 1$.[12]

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[10] Returns are a comprehensive measure of news released both by earnings and by other sources in the non-earnings-announcement period. Returns around earnings announcements capture both the information released by earnings itself, as well as the additional information managers often release with earnings announcements (including that via conference calls) to facilitate understanding of the implications of earnings information (Chen et al. 2002; Frankel et al. 1999). Further, during the non-earnings-announcement period, returns capture the net information released by various sources: alternative forms of managerial disclosures such as earnings forecasts and non-earnings-related qualitative disclosures, as well as information searches by third parties such as financial analysts.

[11] We require at least 25 days with trading to compute *RET*. The market adjustment involves subtracting the buy-and-hold return on the CRSP value-weighted market index from the corresponding buy-and-hold return of the firm. We also conducted robustness analyses with size and book-to-market adjusted returns, instead of market-adjusted returns. This adjustment involves partitioning firms into five equal groups of size and book-to-market, thus, yielding 25 size and book-to-market portfolios. The adjusted return for a firm is then the daily raw return in excess of the daily return on the matching size and book-to-market portfolio. All our subsequent results, including those we obtain in the cross-sectional tests, are robust to using this alternative adjustment.

[12] We check the robustness of our analysis to two alternative definitions of the earnings announcement window. In the first, we estimate *EAR* over Day −3 to Day +3 with respect to the QEA. In the second, we acknowledge that a longer window is especially suitable for smaller firms, and estimate *EAR* over Day −3 to Day +3 for firms below median size, while using Day −1 to Day +1 for firms above median size. In both specifications, the estimation of *NEAR* is adjusted accordingly. All our results are robust to both of these specifications.

TABLE 1
Distribution of *NEWS_RATIO*

Descriptive Statistics	*NEWS_RATIO*			*Ln(NEWS_RATIO)*		
	Full Sample	***BNEWS* = 1**	***BNEWS* = 0**	**Full Sample**	***BNEWS* = 1**	***BNEWS* = 0**
Percentiles						
0.01	0.478	0.540	0.420	−0.754	−0.611	−0.859
0.05	2.296	2.697	1.999	0.831	0.992	0.693
0.25	12.670	14.400	11.160	2.539	2.668	2.412
0.50	32.800	35.630	29.970	3.491	3.573	3.400
0.75	84.620	86.350	82.360	4.438	4.458	4.411
0.95	451.878	420.694	483.424	6.113	6.042	6.181
0.99	1,132.160	1,121.850	1,136.290	7.032	7.023	7.036
Mean	100.030	97.830	102.320	3.467	3.542	3.414
n of firm-quarters	152,275	77,723	74,552	152,275	77,723	74,552

Table 1 provides descriptive statistics for *NEWS_RATIO* and *Ln(NEWS_RATIO)*.

Variable Definitions:
NEWS_RATIO = 100 ∗ *ABS*(*EAR*)/*ABS*(*NEAR*);
ABS(*EAR*) = absolute value of cumulative market-adjusted returns on trading days −1 to +1 relative to the QEA date;
ABS(*NEAR*) = the absolute value of the cumulative market-adjusted non-earnings-announcement period returns;
Ln(*NEWS_RATIO*) = natural logarithm of *NEWS_RATIO*; and
BNEWS = an indicator variable equal to 1 if overall quarterly return *RET* is negative, and 0 otherwise.

We measure earnings informativeness using a ratio of the news arriving during the QEAs to that arriving during non-QEA periods, denoted as *NEWS_RATIO* and defined as 100 ∗ *ABS*(*EAR*)/*ABS*(*NEAR*). The ratio expresses the absolute value of market-adjusted QEA returns, *ABS*(*EAR*), as a percentage of the absolute value of market-adjusted returns during non-QEA periods, *ABS*(*NEAR*).[13]

We expect *NEWS_RATIO* to be high during quarters when the overall news (measured by quarterly return, *RET*) is negative relative to when overall news is positive. Our prediction is easy to follow if *EAR* and *NEAR* are always of the same sign. When *EAR* and *NEAR* are of differing signs, the analysis is more involved, but *NEWS_RATIO* is still expected to exhibit the same asymmetry. Specifically, for all observations consistent with our hypotheses, *NEWS_RATIO* is expected to be systematically higher in negative-return periods ($RET < 0$) and lower in positive-return periods ($RET > 0$). In contrast, for all observations inconsistent with our hypotheses, *NEWS_RATIO* is expected to exhibit the reverse pattern. Thus, variation in *NEWS_RATIO* is well suited to testing whether earnings informativeness is higher when overall information reaching the market is negative. This is discussed in greater detail in Appendix A.

Table 1 presents the details of the distribution of *NEWS_RATIO* and its logarithmic transformation *Ln*(*NEWS_RATIO*) for the overall sample, as well as separately for bad-news and good-news quarters. For every firm, good-news and bad-news quarters are identified by the sign of overall quarterly return, *RET*, and characterized by the indicator variable $BNEWS_t$, which is set equal to 1 if $RET_t < 0$, and is equal to 0 otherwise. Thus, $BNEWS_t = 1$ corresponds to bad-news quarters, while $BNEWS_t = 0$ identifies good-news quarters.

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[13] *NEWS_RATIO* is unlikely to be affected systematically by post-earnings-announcement drift (PEAD) or the under-reaction of the market to current-period dollar earnings surprises (Foster et al. 1984). PEAD implies that earnings surprises at the time of the QEAs predict returns in the following period; further, most of the "drift" return is realized at the subsequent QEAs (Bernard and Thomas 1989). Thus, in the period of the initial earnings surprise, PEAD is likely to bias our measure of earnings informativeness downward; in the period following the initial earnings surprise, PEAD is more likely to bias the measure upward. The overall effect of drift on *NEWS_RATIO* is, thus, ambiguous, and even more so when we subsequently partition on the sign of the returns.

We winsorize *NEWS_RATIO* at the 1st and 99th percentiles, as it includes some unrepresentative high-magnitude ratios driven by low denominators. However, the sharp difference between mean and median *NEWS_RATIO* in Table 1 for the overall sample and both subsamples implies that the variable remains right-skewed. To further address this issue, we compute the natural logarithm of the ratio, denoted *Ln*(*NEWS_RATIO*), and use this measure in our primary empirical tests. As can be seen from the proximity of mean and median *Ln*(*NEWS_RATIO*), the variable is symmetrically distributed. Finally, as an alternative to computing the natural logarithm of the ratio, we supplement our primary tests with additional regressions in which we use decile ranks of *NEWS_RATIO* (*Ranked NEWS_RATIO*) as the dependent variable.

NEWS_RATIO and *Ln*(*NEWS_RATIO*) for $BNEWS = 1$ exceeds that for $BNEWS = 0$ for all the percentiles listed in the table, except for the 95th and 99th percentiles. The median *NEWS_RATIO* is greater for bad-news quarters, but the opposite is true based on the mean ratio, a result of the variable's right-skewness.[14] Computing the natural logarithm of the ratio smoothes out the distribution, and both medians and means produce similar results; i.e., *Ln*(*NEWS_RATIO*) is greater in bad-news quarters. We conduct and discuss formal tests of significance of the difference in means and medians, as well as interpret the magnitude of *Ln*(*NEWS_RATIO*), in Section IV.

Measuring Information Asymmetry

We are primarily interested in a measure of information asymmetry that captures circumstances in which voluntary disclosures of bad news are less timely. A number of variables have been used in the literature to proxy for the information environment in various settings.

Firms with greater size and larger analyst following are expected to be characterized by less information asymmetry between managers and investors, a consequence of both market participants' greater efforts to uncover information and managers' greater willingness to be forthcoming with information (Collins et al. 1987). Further, there is evidence that institutional ownership is positively associated with firm disclosure quality (Bushee and Noe 2000). Thus, we expect that firms with higher institutional ownership are likely to have lower information asymmetry. Greater firm-level return volatility is generally reflective of greater uncertainty about a firm's operations (see, for example, Jiang et al. 2005). While this uncertainty can affect both managers and investors, it is likely to make any managerial information advantage less detectable *ex post* by external investors. Uncertainty about a firm's operations may also result in the manager requiring more time to arrive at a precise estimate of bad news. Another widely used measure of information asymmetry is the adverse selection component of the bid-ask spread (Foster and Viswanathan 1993; Brennan and Subrahmanyam 1996). It measures the extent to which the bid-ask spread quoted for the firm reflects the market makers' attempts to protect themselves against informed trading.

Instead of using the five variables separately, we use principal components analysis to extract the common variation in these variables. The five variables include: (1) logarithm of firm size, defined as market value of equity (*SIZE*), (2) logarithm of 1 plus analyst following, defined as the number of analysts covering the firm (*NUM_AN*), (3) institutional ownership, or the percentage of outstanding shares owned by institutions (*INST_OWN*), (4) idiosyncratic return volatility (*IVOL*), and (5) the adverse selection component of the bid-ask spread (*Adv_BidAsk*). *IVOL* is computed as the volatility of daily firm returns in excess of market returns in the preceding quarterly period, but excluding the earnings announcement. In computing *Adv_BidAsk*, we follow Brennan and Subrahmanyam (1996), who estimate the sensitivity of price changes to order flow based on a model of price formation developed by Hasbrouck (1991). It is computed as the daily average over the last month of the preceding quarter.[15]

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[14] The relatively few instances in which managers pre-announce negative information are likely to dampen the mean *NEWS_RATIO* for bad-news quarters. This is analyzed in greater detail in Section IV.

The principal components analysis yields a composite measure of information asymmetry between managers and shareholders, denoted *InfoAsymm*. *InfoAsymm* is negatively associated with *SIZE*, *NUM_AN*, and *INST_OWN*, and positively associated with *IVOL* and *Adv_BidAsk*. Thus, higher values of the composite measure *InfoAsymm* represent greater information asymmetry. To facilitate our cross-sectional analyses, we rank firms into deciles of *InfoAsymm* every quarter. Using the ranked variable allows us to measure earnings informativeness within each decile, and to provide a more intuitive interpretation of variation in earnings informativeness across the deciles.

Firm Characteristics: Variable Definitions and Descriptive Statistics

Table 2, Panel A, presents the key descriptive statistics for the sample, including the pooled mean, median, standard deviation, and the 5th and 95th percentiles. All continuous variables are winsorized at the 1 percent level. Additionally, we present variation in these descriptive statistics across good-news ($BNEWS_t = 0$) and bad-news quarters ($BNEWS_t = 1$) in Table 2, Panel B. For the purposes of assessing statistical significance of the differences, we use t-statistics with standard errors clustered by firm and fiscal year-quarter for the means, and Z-statistics from the Wilcoxon rank-sum tests for the medians.

RET is measured over the entire quarter (63 trading days, on average). The non-earnings-announcement period return, *NEAR*, spans, on average, a 60-day trading period, excluding the three days around the QEA date and, hence, corresponds closely to *RET*. Mean magnitudes of *RET*, *EAR*, and *NEAR* are higher than the corresponding medians in both the good-news and bad-news quarters, indicating positive skewness in the magnitudes. Both the mean and median magnitudes of earnings announcement returns, *ABS*(*EAR*), are significantly larger in bad-news quarters than in good-news quarters. The mean and median magnitudes of non-announcement returns, *ABS*(*NEAR*), on the other hand, are significantly lower during bad-news quarters than good-news quarters.

Highlighting that earnings are more likely to convey adverse information during bad-news quarters, negative three-day earnings announcement returns tend to occur significantly more frequently in quarters with negative *RET*. Mean *BAD_EAR*, an indicator variable set equal to 1 if $EAR < 0$, is 59.3 percent in bad-news quarters and 39.8 percent in good-news quarters. Mean number of trading days in the announcement quarter, denoted *TRADE_DAYS*, is between 63–64 days and significantly higher for bad-news quarters. This finding is consistent with evidence in the prior literature that documents that earnings announcements tend to be delayed when there is bad news to be conveyed (Givoly and Palmon 1982; Patell and Wolfson 1982; Chambers and Penman 1984; Cohen et al. 2007).

The bad-news subsample includes firms with significantly lower size, measured using market value of equity (*MVE*). Firms in the bad-news subsample also have significantly lower analyst following and institutional ownership, while exhibiting significantly greater idiosyncratic return volatility. *BTM* is the ratio of the book value of equity to market value of equity, and financial leverage, *LEV*, is computed as the ratio of total debt to total assets. All firm characteristics are measured at the beginning of the period. The descriptive statistics demonstrate that while *BTM* is significantly lower for firms in the bad-news subsample, leverage is similar across the two samples.

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[15] Also, see Ng et al. (2009).

TABLE 2

General Descriptive Statistics

Panel A: Overall Sample

	Mean	Median	Std. Dev.	P5	P95
RET (%)	1.418	−0.450	27.725	−37.887	45.831
EAR (%)	0.185	0.034	8.396	−13.492	14.205
NEAR (%)	1.314	−0.584	26.597	−35.717	43.424
ABS(*RET*) (%)	18.345	12.516	20.836	1.081	53.676
ABS(*EAR*) (%)	5.824	3.813	6.051	0.317	18.778
ABS(*NEAR*) (%)	17.370	11.743	20.184	1.026	51.100
BAD_EAR (%)	49.728	0.000	49.999	0.000	100.000
TRADE_DAYS	63.708	63.000	11.265	44.000	84.000
MVE ($ millions)	2,284.784	373.288	6,615.455	24.602	10,765.055
NUM_AN	7.303	5.000	7.673	0.000	24.000
INST_OWN (%)	43.673	43.175	26.892	2.611	87.344
IVOL	3.293	2.746	2.154	1.107	7.303
Adv_BidAsk	3.238	1.155	8.991	−2.224	15.184
BTM (%)	53.805	44.363	47.385	7.125	133.163
LEV (%)	21.088	17.246	20.196	0.000	59.743
HITECH (%)	24.006	0.000	42.712	0.000	100.000
INSALE (%)	33.227	0.000	47.103	0.000	100.000
BIAS_ADJ	2.990	3.015	1.566	0.381	5.621
n of firm-quarters			152,275		

Panel B: Bad-News and Good-News Subsamples

	Means			Medians		
	BNEWS = 1	*BNEWS* = 0	Difference	*BNEWS* = 1	*BNEWS* = 0	Difference
RET (%)	−16.582	20.183	−36.765***	−12.400	12.680	−25.080***
EAR (%)	−1.671	2.120	−3.791***	−1.276	1.227	−2.503***
NEAR (%)	−14.723	18.033	−32.756***	−11.474	11.501	−22.975***
ABS(*RET*) (%)	16.582	20.183	−3.602***	12.400	12.680	−0.280***
ABS(*EAR*) (%)	6.080	5.557	0.523***	3.929	3.627	0.302***
ABS(*NEAR*) (%)	15.742	19.067	−3.325***	11.786	11.796	−0.010***
BAD_EAR (%)	59.269	39.780	19.489***	100.000	0.000	100.000***
TRADE_DAYS	64.067	63.334	0.733**	63.238	63.050	0.188***
MVE ($ millions)	2,212.590	2,360.048	−147.458*	340.806	368.978	−28.172***
NUM_AN	7.159	7.453	−0.294**	5.525	5.825	−0.300***
INST_OWN (%)	42.662	44.726	−2.064***	40.908	42.868	−1.960***
IVOL	3.446	3.134	0.313***	2.915	2.618	0.297***
Adv_BidAsk	3.281	3.193	0.088	0.915	0.876	0.039**
BTM (%)	51.824	55.869	−4.045***	43.829	45.411	−1.582***
LEV (%)	21.086	21.089	−0.002	16.567	16.637	−0.070
HITECH (%)	24.921	23.052	1.868	0.000	0.000	0.000
INSALE (%)	31.058	35.488	−4.430***	0.000	0.000	0.000
BIAS_ADJ	3.057	2.921	0.137***	3.106	2.971	0.135***
n of firm-quarters	77,723	74,552		77,723	74,552	

(continued on next page)

TABLE 2 (continued)

*, **, *** Represent statistical significance at a minimum 0.1, 0.05, and 0.01 levels, respectively.
Table 2 presents descriptive statistics. Panel A reports descriptive statistics for the full sample of 152,275 firm-quarters from 1987–2006. P5 and P95 are the 5th and 95th percentiles, respectively. Panel B reports means and medians for two subsamples: (1) firm-quarters with negative abnormal returns (*BNEWS* = 1), and (2) firm-quarters with positive abnormal returns (*BNEWS* = 0). For the purposes of assessing statistical significance of the differences, we use t-tests with standard errors clustered by firm and year-quarter for the means, and Z-statistics from the Wilcoxon rank-sum test for the medians.

Variable Definitions:
RET = quarterly market-adjusted returns beginning two days after the quarterly earnings announcement for the previous quarter and ending one day after the final earnings announcement for the current quarter;
BNEWS = an indicator variable equal to 1 if *RET* is negative, and 0 otherwise;
EAR = cumulative market-adjusted returns on trading days −1 to +1 relative to the quarterly earnings announcement;
NEAR = cumulative market-adjusted non-earnings-announcement period returns (spanning all trading days included in *RET* except for the trading days included in *EAR*);
ABS(*RET*), *ABS*(*EAR*), and *ABS*(*NEAR*) = absolute values for *RET*, *EAR*, and *NEAR*, respectively;
BAD_EAR = an indicator variable set equal to 1 if *EAR* < 0;
TRADE_DAYS = number of trading days in the announcement quarter;
MVE = beginning-of-quarter market value of equity;
NUM_AN = beginning-of-period number of analysts following the firm;
INST_OWN = beginning-of-period percentage institutional ownership;
IVOL (idiosyncratic volatility) = volatility of daily firm returns in excess of market returns in the preceding quarterly period, excluding the earnings announcements;
Adv_BidAsk = adverse selection component of the daily bid-ask spread (based on Hasbrouck [1991]) computed as the daily average over the last month of the preceding quarter;
BTM = beginning-of-period book-to-market ratio (book value of equity to market value of equity);
LEV = beginning-of-period leverage measured using ratio of total debt, both long-term and short-term, to total assets;
HITECH = binary indicator variable that is set equal to 1 if the firm belongs to any of the following four-digit SIC industry codes: 2833–2836, 3570–3577, 3600–3674, 7371–7379, or 8731–8734;
INSALE = binary indicator set equal to 1 for firm-quarters with net insider sales, and 0 otherwise. Net insider sales are measured as the sales of stock minus purchases of stock across directors and firm executives identified as officers; and
BIAS_ADJ = natural logarithm of the ratio of cumulative market-adjusted returns during a random three-day window in the quarter relative to the cumulative market-adjusted returns during that quarter outside the window.

Following Field et al. (2005), we define *HITECH* as a binary indicator variable that is set equal to 1 if the firm belongs to the following four-digit SIC industry codes, signifying technology-intensive industries: 2833–2836 (drugs and pharmaceuticals), 3570–3577 (computer and office equipment), 3600–3674 (electrical equipment and electronics), 7371–7379 (software services) or 8731–8734 (R&D services). As the mean value of *HITECH* shows, 24.9 percent of the bad-news firm-quarters and 23.1 percent of the good-news firm-quarters belong to technology-intensive industries. Finally, *INSALE* is a binary 0/1 indicator set equal to 1 if insiders have net sales of stock in a given firm-quarter. Net insider sales are measured as the stock sales minus stock purchases across directors and firm executives identified as officers.[16] Table 2, Panel B, indicates that insider sales are significantly less frequent in bad-news quarters, where their frequency is around 31.1 percent, than in good-news quarters, where their frequency is around 35.5 percent.

For every firm-quarter in our sample, we identify a random three-day window and measure the ratio of news released in that window to that released in the period within that quarter outside of the three-day window.[17] The natural logarithm of this ratio is used in our multivariate analyses to control for possible mechanical bias in our dependent variable, and is denoted *BIAS_ADJ*. This is further discussed in Section IV. As Table 2, Panel B, reports, mean and median *BIAS_ADJ* are significantly higher in bad-news quarters than in good-news quarters. Table 2 demonstrates that significant differences can exist in the characteristics of firms that experience negative versus positive returns, particularly those such as size and book-to-market, which are likely associated with firm risk. To address the issue of systematic differences, we control for these firm characteristics in our regression analyses, discussed below.

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[16] We exclude divisional officers and officers of subsidiary companies in estimating net insider sales.
[17] The random three-day window excludes any of the three days around the QEA.

IV. PRIMARY ANALYSES

Earnings Informativeness and the Sign of News

In our first empirical test of H1, we make univariate comparisons between the mean and median earnings informativeness (*Ln*(*NEWS_RATIO*)) across bad- and good-news quarters. To assess the difference in means, we use two types of t-tests with (1) standard errors clustered by firm and fiscal year-quarter, and (2) bootstrapped standard errors.[18] The difference in medians is evaluated based on the Z-statistic from the Wilcoxon rank-sum test. We report the results of these tests in Table 3, Panel A. Consistent with H1, mean and median *Ln*(*NEWS_RATIO*) are higher for bad-news quarters than for good-news quarters, with the differences being statistically significant at the 1 percent level. For both the $BNEWS = 1$ and $BNEWS = 0$ subsamples, we compute implied *NEWS_RATIO*, which is simply the exponent of the corresponding *Ln*(*NEWS_RATIO*). The implied *NEWS_RATIO* using the mean logged ratio is around 34.5 percent for bad-news quarters and 30.4 percent for good news quarters. The interpretation of this finding is that earnings are 13.5 percent more informative in bad-news quarters than in good-news quarters.

We next shift to multivariate tests. We use the following two regressions to test whether the news released during quarterly earnings announcements (QEAs) relative to that released during non-announcement (non-QEA) periods is greater when the overall returns are negative:

$$Ln(NEWS\_RATIO_t) = \alpha_0 + \beta * BNEWS_t + \alpha_1 * BIAS\_ADJ_t + \varepsilon_t. \tag{1}$$

$$\begin{aligned} Ln(NEWS\_RATIO_t) = {} & \alpha_0 + \beta * BNEWS_t + \alpha_1 * BIAS\_ADJ_t + \alpha_2 * RInfoAsymm_{t-1} + \alpha_3 * INSALE_t \\ & + \alpha_4 * TRADE\_DAYS_t + \alpha_5 * RBTM_{t-1} + \alpha_6 * RLEV_{t-1} + \alpha_7 * HITECH_{t-1} \\ & + \varepsilon_t. \end{aligned} \tag{2}$$

The intercept α_0 in Equation (1) represents the mean *Ln*(*NEWS_RATIO*) for good-news quarters after controlling for *BIAS_ADJ*. The coefficient on *BNEWS*, β, captures the extent to which the *Ln*(*NEWS_RATIO*) ratio is different for bad-news quarters as compared to good-news quarters. Our primary hypothesis (H1) predicts that the *Ln*(*NEWS_RATIO*) is higher when overall quarterly return, *RET*, is negative. In other words, we predict that β is significantly positive.

Equation (1) represents a restricted model in which we include only one control variable, *BIAS_ADJ*. It is possible that variation in *Ln*(*NEWS_RATIO*) across bad-news and good-news quarters arises at least partially from the differing variances of negative quarterly returns and positive quarterly returns.[19] However, differing variances of negative and positive returns should influence the informativeness we observe for any random three-day window within the quarter, not

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[18] The bootstrapped standard errors come from re-sampling 152,275 observations 1,000 times with replacement.

[19] While the computation of *NEWS_RATIO* does not involve *RET per se*, it does involve returns during the non-QEA window in the quarter (*NEAR*). Since *NEAR* is highly associated with *RET* (correlation coefficient of 0.94), systematic differences in the variance of positive versus negative *RET* can mechanically induce systematic differences in *NEWS_RATIO* across the positive-*RET* and negative-*RET* partitions.

TABLE 3

Earnings' Differential Informativeness in Bad-News Quarters and Good-News Quarters

Panel A: Univariate Tests

	Ln(NEWS_RATIO)					
	***BNEWS* = 1**	***BNEWS* = 0**	**Difference**	**t-stat. with S.E. Clustered by Firm- and Year-Qtr.**	**t-stat. with Boot-strapped S.E.**	**Z-stat. from Wilcoxon Rank-Sum Test**
Mean	3.542	3.414	0.128***	3.78	3.82	
Implied Mean Ratios	34.546	30.396	4.151			
Median	3.573	3.400	0.173***			18.28
Implied Median Ratios	35.623	29.964	5.659			

Panel B: Earnings' Differential Informativeness—Multivariate Tests

		Dependent Variable			
	Predicted Sign	***Ln(NEWS_RATIO)*** **Model 1**	***Ln(NEWS_RATIO)*** **Model 2**	***Ln(NEWS_RATIO)*** **Model 3**	***Ranked NEWS_RATIO*** **Model 4**
Intercept		2.771***	3.326***	3.342***	3.877***
		(0.043)	(0.081)	(0.073)	(0.095)
BNEWS	+	0.097***	0.110***	0.120***	0.226***
		(0.029)	(0.028)	(0.028)	(0.046)
BIAS_ADJ		0.216***	0.214***	0.216***	0.256***
		(0.006)	(0.006)	(0.006)	(0.006)
RInfoAsymm			−0.030***	−0.039***	−0.060***
			(0.004)	(0.006)	(0.007)
INSALE			0.072***	0.031**	0.075***
			(0.017)	(0.014)	(0.021)
TRADE_DAYS			−0.006***	−0.006***	−0.005***
			(0.001)	(0.001)	(0.001)
RBTM			−0.002	−0.001	−0.006*
			(0.002)	(0.003)	(0.003)
RLEV			−0.010**	−0.001	−0.019***
			(0.002)	(0.003)	(0.003)
HITECH			0.042***		0.095***
			(0.015)		(0.028)
Firm Fixed Effects		No	No	Yes	No
S.E. Clustered by Year-Qtr		No	No	Yes	No
S.E. Clustered by Firm- and Year-Qtr		Yes	Yes	No	Yes

(continued on next page)

TABLE 3 (continued)

	Model 1	Model 2	Model 3	Model 4
n of observations	152,257	152,257	152,257	152,257
Adjusted R^2	4.78%	5.41%	7.36%	7.22%
Implied Ratios				
BNEWS = 1	17.60	17.83	17.80	
BNEWS = 0	15.97	15.98	15.78	

Panel C: Short-Horizon Forecasts and Earnings' Differential Informativeness

		Dependent Variable: *Ln*(*NEWS_RATIO*)		
		Model 1	**Model 2**	**Model 3**
	Predicted Sign for Models 1 and 3	**Subsample from 1995**	**Subsample of Forecasters (from 1995)**	**Subsample of Non-Forecasters (from 1995)**
Intercept		3.328***	3.017***	3.366***
		(0.092)	(0.161)	(0.090)
BNEWS	+	0.106***	−0.095*	0.144***
		(0.032)	(0.058)	(0.031)
BIAS_ADJ		0.216***	0.239***	0.211***
		(0.007)	(0.008)	(0.007)
RInfoAsymm		−0.031***	−0.035***	−0.036***
		(0.004)	(0.008)	(0.004)
INSALE		0.069***	0.064*	0.070***
		(0.014)	(0.035)	(0.015)
TRADE_DAYS		−0.006***	−0.004	−0.006***
		(0.001)	(0.003)	(0.001)
RBTM		−0.002	−0.011**	−0.001
		(0.002)	(0.005)	(0.002)
RLEV		−0.011***	−0.002	−0.012***
		(0.002)	(0.005)	(0.002)
HITECH		0.042**	0.000	0.056***
		(0.016)	(0.029)	(0.017)
S.E. Clustered by Firm- and Year-Qtr		Yes	Yes	Yes
n of observations		134,658	14,152	119,051
Adjusted R^2		5.48%	6.74%	5.49%
Implied Ratios				
BNEWS = 1		17.78	12.57	18.85
BNEWS = 0		15.99	13.83	16.32

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*, **, *** Represent statistical significance at a minimum 0.1, 0.05, and 0.01 levels, respectively.

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TABLE 3 (continued)

Table 3 presents univariate and multivariate tests of earnings differential informativeness. Panel A includes univariate t-tests for the differences of means and median of *Ln*(*NEWS_RATIO*) between the bad-news and the good-news quarters (*BNEWS* = 1 versus *BNEWS* = 0). t-tests are used for the comparison of the means, and the Z-statistic from the Wilcoxon rank-sum for the comparison of the medians. The first t-statistic is based on standard errors clustered by firm and fiscal year-quarter, while the second is based on bootstrapped standard errors clustered by fiscal year-quarter (obtained from drawing 152,275 observations 1,000 times with replacement). Panel B, Models 1, 2, and 4, report the results of OLS regressions with standard errors clustered by firm and fiscal year-quarter, while Panel B, Model 3, reports OLS regression results with firm fixed effects and clustering of standard errors by fiscal year-quarter. In all the regression models in Panel B (except for Model 4), we use *Ln*(*NEWS_RATIO*) as the dependent variable. In Panel B, Model 4, the dependent variable is the decile rank of *NEWS_RATIO* (*Ranked NEWS_RATIO*). Implied Ratio is the exponent of the predicted *Ln*(*NEWS_RATIO*), obtained by setting all control variables to their means except for *BIAS_ADJ*, which is set equal to 0, and *BNEWS*, equal to 1 or 0. Panel C reports the results of OLS regressions with *Ln*(*NEWS_RATIO*) as the dependent variable, and with standard errors clustered by firm and fiscal year-quarter. Panel C, Model 1, includes the sample of 134,658 firm-quarters between 1995 and 2006. Panel C, Model 2, includes the subsample of 14,152 firm-quarters between 1995 and 2006 that issue forecasts of current earnings following the previous quarter's earnings announcements, or short-horizon forecasts. Panel C, Model 3, includes the subsample of 120,506 firm-quarters between 1995 and 2006 that do not issue short-horizon forecasts. Standard errors are reported in parentheses below coefficients.
All other variables are defined in the notes to Table 2.

Variable Definitions:
Ln(*NEWS_RATIO*) = natural logarithm of *NEWS_RATIO*;
NEWS_RATIO = 100 ∗ *ABS*(*EAR*)/*ABS*(*NEAR*);
ABS(*EAR*) = absolute value of cumulative market-adjusted returns on trading days −1 to +1 relative to the QEA date;
ABS(*NEAR*) = absolute value of the cumulative market-adjusted non-earnings-announcement period returns;
BNEWS = an indicator variable equal to 1 if overall quarterly return *RET* is negative, and 0 otherwise;
RInfoAsymm = decile rank of *InfoAsymm*, which is extracted from a principal-components analysis, and is negatively associated with firm size, analyst following, and institutional ownership, and positively associated with idiosyncratic volatility and the adverse selection component of the bid-ask spread;
RBTM = decile rank of beginning-of-period book-to-market; and
RLEV = decile rank of beginning-of-period financial leverage.

just that including the QEA. Therefore, we control for *BIAS_ADJ*, which is defined as the natural logarithm of the ratio of news released during a random three-day window in the quarter relative to that released during the period in that quarter outside the window.[20]

Equation (2) expands the set of control variables to capture systematic differences in earnings informativeness arising from firm characteristics. *RInfoAsymm* is the decile rank of *InfoAsymm* within every quarter. Greater information asymmetry between managers and shareholders, and the general uncertainty typically characterizing high-information-asymmetry firms, can significantly influence earnings informativeness relative to other sources. For example, Bamber (1987) and McNichols (1988) argue that the role of earnings in conveying information varies with the information environment of the firm, which they capture empirically with firm size.

Given that managers often possess private information, their voluntary disclosures, as well as their financial reporting choices, are likely influenced by their own trading patterns (Noe 1999). Consequently, we control for the effect of *INSALE* on the informativeness of earnings. We control for *TRADE_DAYS*, since longer quarters can allow managers more time to disclose news before the actual earnings announcement and, thus, reduce earnings informativeness.

The nature of the investment opportunity set—in particular, the extent to which it is driven by growth options—can influence the information content of earnings (Collins and Kothari 1989). We control for the investment opportunity set using *RBTM*, the decile rank of the book-to-market ratio, and membership in a technology-intensive industry, denoted *HITECH*. Finally, additional scrutiny of earnings information by debt-holders can influence earnings informativeness; since such scrutiny is likely to be increasing in leverage, we control for *RLEV*, the decile rank of financial leverage.

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[20] Differencing *Ln*(*NEWS_RATIO*) and *BIAS_ADJ* to construct a dependent variable adjusted for bias is problematic. Each term in the difference would be a logged ratio, and differencing would yield the logarithm of a ratio of two ratios, very difficult to interpret economically.

Model 1 in Panel B of Table 3 reports the results of estimating Equation (1) over the entire sample of 152,275 firm-quarters. Models 2 through 4 impose controls for firm characteristics (Equation (2)). In Models 1 through 3, the dependent variable is our primary ratio, *Ln*(*NEWS_RATIO*), and in Model 4, it is the decile rank of *NEWS_RATIO*, along with a decile rank of *BIAS_ADJ* as the control. All models are estimated on the pooled sample using ordinary least squares methodology. Standard errors are clustered by firm and fiscal year-quarter in Models 1, 2, and 4. In Model 3, we include firm fixed effects and cluster standard errors by fiscal year-quarter.

In all four models presented in Table 3, Panel B, the coefficient on *BNEWS* is positive and statistically significant at the 1 percent level. To estimate the economic effect of *BNEWS*, we first compute the implied *NEWS_RATIO* for good-news quarters by setting all control variables except *BIAS_ADJ* to their mean values, and *BNEWS* equal to 0. *BIAS_ADJ* is also set equal to 0, to prevent it from inducing a mechanical bias into our estimate of earnings informativeness. This yields an implied *NEWS_RATIO* of approximately 16.0 percent for good-news quarters in Models 1, 2, and 3, indicating that in good-news quarters, news released during earnings announcements is around 16 percent of net news released during the non-announcement period. Subsequently, we follow a similar procedure to estimate the implied *NEWS_RATIO* for bad-news quarters by setting *BNEWS* equal to 1. The implied *NEWS_RATIO* indicates that in bad-news quarters, news released during earnings announcements is around 18 percent of net news released during the non-announcement period. Consistent with H1, the results further imply that during bad-news quarters, earnings informativeness is 12.0 percent higher than during good-news quarters.

We expect earnings informativeness in bad-news quarters relative to good-news quarters to be more pronounced in the sample of firms without short-horizon forecasts; that is, forecasts of current quarter's earnings issued after the previous quarter's earnings announcements. First Call's coverage on management forecasts has substantially expanded since 1995 (see Anilowski et al. 2007). Therefore, we investigate the difference between firms with and without short-horizon forecasts within the subsample of 134,658 firm-quarters between 1995 and 2006, and report the results in Table 3, Panel C. First, in Model 1, we provide the results of Equation (2) reestimated on the subsample of all firm-quarters between 1995 and 2006. Models 2 and 3 of Table 3, Panel C, include the subsamples of firm-quarters with and without forecasts, respectively, starting in 1995.

In untabulated analyses, we observe that mean (median) quarterly returns (*RET*) for firms issuing short-horizon forecasts are around −5.2 percent (−5.5 percent), while mean (median) quarterly returns for firms not issuing short-horizon forecasts are around 2.3 percent (0.02 percent). Focusing on firms experiencing bad news ($RET < 0$), mean (median) returns for bad-news firm-quarters with short-horizon forecasts are around −22.0 percent (−18.0 percent), while mean (median) quarterly returns for bad-news firm-quarters without short-horizon forecasts are around −16.3 percent (−12.2 percent).[21] Thus, the return data suggest that managers issue short-horizon forecasts in quarters in which the news reaching the market is particularly negative, and litigation risk is presumably more pronounced (Francis et al. 1994; Skinner 1997).

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[21] All reported differences in *RET* between firms with short-horizon forecasts and those without such forecasts are statistically significant at the 1 percent level, using t-statistics with standard errors clustered by firm and year-quarter for the means and Z-statistics from the Wilcoxon rank-sum test for the medians. These results are untabulated.

Model 2 presents results for 14,152 firm-quarters between 1995 and 2006 with short-horizon forecasts.[22] We find that for this subsample, earnings are *less* informative in bad-news quarters than in good-news quarters. The coefficients imply that the ratio of news disclosed during QEAs to that disclosed during non-QEA periods is 12.6 percent in bad-news quarters, as opposed to 13.8 percent in good-news quarters. The results are consistent with short-horizon forecasts preempting bad news at the earnings announcements.

For the 120,506 firm-quarters between 1995 and 2006 *without* short-horizon forecasts, Model 3 indicates strong support for H1. After imposing various controls, the coefficient on *BNEWS* in Model 3 is positive and statistically significant at the 1 percent level. The implied *NEWS_RATIO* for the average firm is 18.9 percent in bad-news quarters and 16.3 percent in good-news quarters. The difference of 2.6 percent points implies that earnings are around 16.0 percent (or 2.6/16.3) more informative during bad-news quarters relative to good-news quarters. Recall that by selection, in the sample of firms without any short-horizon forecasts, litigation risk is not high enough to warrant early public disclosure of impending bad news.

Collectively, the evidence suggests that the prospect of earnings eventually revealing withheld bad news induces managers to preemptively disclose information that is particularly negative, and is, thus, presumably associated with higher litigation risk. In the remaining majority of cases, when the magnitude of bad news is relatively low, it is earnings rather than preemptive short-horizon disclosures that reveal the bad news.[23]

Cross-Sectional Variation in Earnings' Informativeness

We test the cross-sectional variation in earnings' more pronounced role in disclosing bad news relative to good news using the following regression:

$$\begin{aligned} Ln(NEWS\_RATIO_t) = {} & \alpha_0 + \alpha_1{*}BIAS\_ADJ_t + \alpha_2{*}RInfoAsymm_{t-1} + \alpha_3{*}INSALE_t \\ & + \alpha_4{*}TRADE\_DAYS_t + \alpha_5{*}RBTM_{t-1} + \alpha_6{*}RLEV_{t-1} \\ & + \alpha_7{*}HITECH_{t-1} + \beta_0{*}BNEWS_t + \beta_1{*}BIAS\_ADJ_t{*}BNEWS_t \\ & + \beta_2{*}RInfoAsymm_{t-1}{*}BNEWS_t + \beta_3{*}INSALE_t{*}BNEWS_t \\ & + \beta_4{*}TRADE\_DAYS_t{*}BNEWS_t + \beta_5{*}RBTM_{t-1}{*}BNEWS_t \\ & + \beta_6{*}RLEV_{t-1}{*}BNEWS_t + \beta_7{*}HITECH_{t-1}{*}BNEWS_t + \varepsilon_t. \end{aligned} \tag{3}$$

Equation (3) is similar to Equation (2), except that it introduces partitioning variables and allows the intercept and the coefficient on *BNEWS* to vary with the partitioning variables via the interaction terms.

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[22] The proportion of firm-quarters with short-horizon forecasts in our sample is relatively low. Even after the passage of Regulation Fair Disclosure, firm-quarters with short-horizon forecasts constitute only 11 percent of our sample (20 percent value-weighted). Note that this is much lower than the proportion of firm-quarters in our sample that report negative quarterly returns (47 percent, 48 percent value-weighted) or even negative earnings announcement returns (50 percent, 47 percent value-weighted), indicating that a relatively large proportion of firms do *not* preempt bad news.

[23] In additional analysis, we further partition the sample of firms that issue short-horizon earnings forecasts into firm-quarters with management forecasts that convey negative information versus those that convey positive information. As expected, in the sample of firm-quarters with management forecasts conveying negative information, earnings' informativeness in bad-new quarters is lower than that in good-news quarters. However, there is significant evidence of earnings' greater informativeness in bad-news quarters in the sample of firm-quarters with management forecasts conveying positive information. These findings indicate that when negative information is not disclosed in voluntary disclosures, it is released by earnings.

RInfoAsymm $*$ *BNEWS* is included as an explanatory variable to test whether earnings' differential informativeness during bad news periods is higher among firm-quarters characterized by greater information asymmetry (H2). *INSALE* $*$ *BNEWS* tests whether earnings' differential informativeness is higher among firm-quarters including insider sale of stock (H3). Finding significant and positive β_2 and β_3 would, therefore, provide support for H2 and H3, respectively. We also include *TRADE_DAYS*, *RBTM*, *RLEV*, *HITECH*, and *BIAS_ADJ* independently and interacted with *BNEWS* as control variables.

Table 4, Model 1, presents results on cross-sectional variation for the full sample using our main specification. The coefficient on *RInfoAsymm* in Model 1 is significantly negative, implying that earnings informativeness in good-news quarters is declining with our measure of information asymmetry. More importantly, consistent with H2, the coefficient on *RInfoAsymm* $*$ *BNEWS* is significantly positive at the 1 percent level, indicating that earnings *differential* informativeness in bad-news quarters, relative to good-news quarters, is increasing in the degree of information asymmetry. Moreover, consistent with H3, earnings' differential informativeness in bad-news quarters, relative to good-news quarters, is higher when there is net selling of the firm's stock by insiders during the quarter (the coefficient on *INSALE* $*$ *BNEWS* is 0.133, and significant at the 1 percent level). We draw similar conclusions from Model 2 with firm fixed effects. In Model 3, we use decile ranks of *NEWS_RATIO* as the dependent variable, and replace *BIAS_ADJ* with decile ranks of the variable, as well. We note that the coefficients on both variables of interest (*RInfoAsymm* $*$ *BNEWS* and *INSALE* $*$ *BNEWS*) are of the predicted signs and statistically significant at the 1 percent level.

To evaluate the economic magnitude of earnings' differential informativeness for firms with varying levels of information asymmetry and with or without net insider sales, we compute implied ratios based on the coefficients obtained from Model 1, and present the ratios in Panel B of Table 4. We first estimate base case implied ratios, in which we set *RInfoAsymm*, *INSALE*, and *BIAS_ADJ* to 0 and all other explanatory variables to their mean values. The implied *NEWS_RATIO* for firms in the lowest rank of information asymmetry and no net insider sales is 19.7 percent in bad-news quarters, while it is 18.2 percent in good-news quarters. Thus, earnings are 1.5 percent points, or 8.6 percent, more informative in bad-news quarters than in good-news quarters for firms with low information asymmetry and no net insider sales. For firms in the highest rank of information asymmetry (but with no net insider sales), the implied *NEWS_RATIO* is 17.0 percent in bad-news quarters, and 12.3 percent in good news quarters, implying that earnings are 4.7 percent points, or 37.8 percent, more informative in bad-news quarters than in good-news quarters. Differential informativeness of earnings also increases relative to the base case for firms with net insider sales. The implied *NEWS_RATIO* for firms with net insider sales (but in the lowest rank of information asymmetry) is 22.6 percent in bad-news quarters, and 18.3 percent in good-news quarters, implying that earnings are 4.3 percent points, or 23.6 percent, more informative in bad-news quarters than in good-news quarters.

V. ADDITIONAL ANALYSES

Earnings' Differential Informativeness by Fiscal Quarter

To investigate further the role of the earnings reporting process in influencing earnings' differential informativeness, we examine how the latter varies across the four fiscal quarters. In the first three fiscal quarters, auditors only review financial reports, while in the fourth quarter, they perform a complete and thorough annual audit of statements that are subsequently submitted to the Securities and Exchange Commission (SEC). If the year-end audit is primarily responsible for the

TABLE 4

Cross-Sectional Variation in Earnings Differential Informativeness

Panel A: Regression Results

		Dependent Variable		
	Predicted Signs	***Ln(NEWS_RATIO)*** **Model 1**	***Ln(NEWS_RATIO)*** **Model 2**	***Ranked NEWS_RATIO*** **Model 3**
Intercept		3.459***	3.426***	4.144***
		(0.101)	(0.098)	(0.120)
BIAS_ADJ		0.235***	0.240***	0.280***
		(0.007)	(0.006)	(0.007)
RInfoAsymm		−0.043***	−0.049***	−0.085***
		(0.005)	(0.007)	(0.009)
INSALE		0.007	−0.038**	−0.044
		(0.022)	(0.018)	(0.030)
TRADE_DAYS		−0.008***	−0.007***	−0.008***
		(0.001)	(0.001)	(0.001)
RBTM		−0.002	0.002	−0.004
		(0.004)	(0.005)	(0.007)
RLEV		−0.012***	−0.001	−0.023***
		(0.003)	(0.004)	(0.005)
HITECH		0.029		0.100**
		(0.031)		(0.049)
BNEWS		−0.125	−0.086	−0.258
		(0.112)	(0.118)	(0.189)
BIAS_ADJ ∗ BNEWS		−0.044***	−0.044***	−0.050***
		(0.006)	(0.006)	(0.006)
RInfoAsymm ∗ BNEWS	+	0.027***	0.023***	0.050***
		(0.006)	(0.006)	(0.009)
INSALE ∗ BNEWS	+	0.133***	0.140***	0.243***
		(0.022)	(0.021)	(0.036)
TRADE_DAYS ∗ BNEWS		0.003*	0.002	0.006**
		(0.002)	(0.002)	(0.003)
RBTM ∗ BNEWS		0.002	−0.004	−0.001
		(0.005)	(0.005)	(0.009)
RLEV ∗ BNEWS		0.004	0.000	0.008
		(0.004)	(0.004)	(0.007)
HITECH ∗ BNEWS		0.026	0.030	−0.007
		(0.0449)	(0.042)	(0.072)
Firm Fixed Effects		No	Yes	No
S.E. Clustered by Year-Qtr		No	Yes	No
S.E. Clustered by Firm- and Year-Qtr		Yes	No	Yes
n of observations		152,275	152,275	152,275
Adjusted R^2		5.54%	7.64%	7.36%

(continued on next page)

TABLE 4 (continued)

Panel B: Implied Ratios

	Model 1		
	Base Case		
	RInfoAsymm = 0, *INSALE* = 0	*RInfoAsymm* = 9, *INSALE* = 0	*INSALE* = 1, *RInfoAsymm* = 0
BNEWS = 1	19.70	17.00	22.64
BNEWS = 0	18.20	12.33	18.31

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*, **, *** Represent statistical significance at a minimum 0.1, 0.05, and 0.01 levels, respectively.
Table 4, Panel A, Models 1 and 3, present the results of OLS regressions with standard errors clustered by firm and fiscal year-quarter, while Model 2 is estimated using OLS with firm fixed effects and standard errors clustered by fiscal year-quarter. The dependent variable in Models 1 and 2 is *Ln(NEWS_RATIO)*. The dependent variable in Model 3 is the decile rank of *NEWS_RATIO* (*Ranked NEWS_RATIO*). All models include the sample of 152,275 firm-quarters between 1987 and 2006. Panel B reports Implied Ratios, computed as the exponent of the predicted *Ln(NEWS_RATIO)* obtained by setting all control variables to their means except for *BIAS_ADJ*, which is set equal to 0, *BNEWS*, equal to 1 or 0, and *RInfoAsymm* and *INSALE*, which are set to the values reported in the table. Standard errors are reported in parentheses below coefficients.
All other variables are defined in the notes to Tables 1 and 2.

Variable Definitions:
Ln(NEWS_RATIO) = natural logarithm of *NEWS_RATIO*;
NEWS_RATIO = 100 ∗ *ABS(EAR)/ABS(NEAR)*;
ABS(EAR) = absolute value of cumulative market-adjusted returns on trading days −1 to +1 relative to the QEA date;
ABS(NEAR) = absolute value of the cumulative market-adjusted non-earnings-announcement period returns; and
BNEWS = an indicator variable equal to 1 if overall quarterly return *RET* is negative, and 0 otherwise.

release of bad news in earnings, then earnings' differential informativeness should be concentrated in the fourth fiscal quarter.

Table 5 presents the results of estimating earnings' differential informativeness with respect to bad news by fiscal quarter. The coefficient on *BNEWS* is statistically insignificant in the first and second fiscal quarters, and significant at the 5 percent or higher level in the third and fourth fiscal quarters. In magnitude, the coefficient on *BNEWS* at first increases across quarters, reaching its peak in the third fiscal quarter, and then exhibits a slight decline. The coefficients imply that earnings are only around 5.6 percent more informative about bad news in the first fiscal quarter, and that this effect increases to 10.5 percent in the second fiscal quarter; the corresponding percentages are around 15.1 percent for the third fiscal quarter and 13.3 percent for the fourth. Interestingly, earnings informativeness with respect to both good and bad news exhibits a lower magnitude in the fourth fiscal quarter than in the third.

One interpretation of the results is that as the year-end audit approaches, managers release more bad news via the earnings reporting process, causing the observed rise in earnings' differential informativeness over the first three fiscal quarters. However, in the fourth quarter, the imminent release of audited annual results can generate intensified scrutiny of the firm by market participants, even prior to the earnings announcements, prompting managers to provide more voluntary disclosures (Baginski and Hassell 1990). This is possibly responsible for the diminished informativeness of earnings in the fourth quarter, with respect to both bad and good news.

TABLE 5

Earnings' Differential Informativeness by Fiscal Quarter

	Predicted Sign	Dependent Variable: *Ln*(*NEWS_RATIO*)			
		Quarter 1	**Quarter 2**	**Quarter 3**	**Quarter 4**
Intercept		3.622***	3.104***	3.403***	2.855***
		(0.103)	(0.111)	(0.147)	(0.173)
BNEWS	+	0.055	0.100	0.141***	0.125**
		(0.057)	(0.065)	(0.039)	(0.057)
BIAS_ADJ		0.188***	0.213***	0.202***	0.249***
		(0.009)	(0.008)	(0.010)	(0.013)
RInfoAsymm		−0.031***	−0.032***	−0.030***	−0.035***
		(0.005)	(0.008)	(0.007)	(0.007)
INSALE		0.053	0.085***	0.083***	0.075*
		(0.034)	(0.026)	(0.030)	(0.039)
TRADE_DAYS		−0.010***	−0.003**	−0.007***	−0.001
		(0.001)	(0.001)	(0.002)	(0.001)
RBTM		0.000	−0.003	0.002	−0.003
		(0.003)	(0.004)	(0.002)	(0.004)
RLEV		−0.012***	−0.006**	−0.010***	−0.013***
		(0.004)	(0.003)	(0.003)	(0.003)
HITECH		0.083***	0.034	0.039*	0.030
		(0.031)	(0.023)	(0.023)	(0.0291)
S.E. Clustered by Firm- and Year-Qtr		Yes	Yes	Yes	Yes
n of observations		35,529	38,020	38,633	40,093
Adjusted R^2		4.14%	5.10%	4.74%	6.97%
Implied Ratios					
BNEWS = 1		20.18	17.83	19.46	14.70
BNEWS = 0		19.11	16.13	16.90	12.97

*, **, *** Represent statistical significance at a minimum 0.1, 0.05, and 0.01 levels, respectively.

Table 5 presents the results from OLS regressions estimated separately for fiscal Quarters 1–4. Standard errors are clustered by firm and fiscal year-quarter. The dependent variable is *Ln*(*NEWS_RATIO*). Implied Ratio is the exponent of the predicted *Ln*(*NEWS_RATIO*), obtained by setting all control variables to their means except for *BIAS_ADJ*, which is set equal to 0, and *BNEWS*, equal to 1 or 0. Standard errors are reported in parentheses below coefficients.

All other variables are defined in the notes to Tables 1 and 2.

Variable Definitions:

Ln(*NEWS_RATIO*) = natural logarithm of *NEWS_RATIO*;
NEWS_RATIO = 100 * *ABS*(*EAR*)/*ABS*(*NEAR*);
ABS(*EAR*) = absolute value of cumulative market-adjusted returns on trading days −1 to +1 relative to the QEA date;
ABS(*NEAR*) = absolute value of the cumulative market-adjusted non-earnings-announcement period returns; and
BNEWS = an indicator variable equal to 1 if overall quarterly return *RET* is negative, and 0 otherwise.

Earnings Misses

In expanded cross-sectional analyses, we test whether earnings' differential informativeness during bad-news quarters can indeed be attributed to negative information conveyed *directly* by earnings information. The primary example of earnings conveying negative information is when realized earnings fall short of prevailing expectations at the time of the earnings announcement. We define an indicator variable, *MISS*, set equal to 1 when actual earnings fall below earnings expectations. Earnings expectations are based on consensus analyst forecasts if analyst data are available; otherwise, they are based on a seasonal random walk.[24] Our results (untabulated) reveal that earnings' differential informativeness is significantly more pronounced in quarters when earnings fall below market expectations. The finding confirms that negative earnings surprises contribute to the increased informativeness of earnings that we observe in bad-news quarters using our returns-based measure.

Subperiod Analysis

There were two significant regulatory events during our sample period that are likely to have influenced both voluntary disclosure and financial reporting practices: Regulation Fair Disclosure (REG FD), effective since the fourth calendar quarter of 2000, and the Sarbanes Oxley Act (SOX), effective since the third calendar quarter of 2002.[25] In this section, we examine earnings informativeness in three subperiods: (1) before REG FD, (2) after REG FD, but before SOX, and (3) after SOX. The passage of REG FD and SOX was associated with a period of significant turmoil in the capital markets, particularly with the reporting scandals involving audit firms such as Arthur Andersen and a large number of U.S. corporations such as WorldCom, Enron, Qualcomm, etc. These confounding events imply that any observed change in earnings informativeness cannot be exclusively attributed to REG FD and/or SOX, and in that sense, our evidence is descriptive. Nevertheless, it is instructive to learn how earnings informativeness varied across this turbulent period, within which the passage of REG FD and SOX represent two clearly identifiable watershed events.

Table 6 reports the results of subperiod analyses testing for changes in the differential informativeness of earnings announcements with respect to bad news versus good news with the passage of REG FD and SOX. Consequently, we examine three subperiods in our sample. Model 1 of Table 6 includes the first subperiod: all fiscal quarters which, in calendar time, precede the fourth quarter of 2000 (PRE-REG-FD). Model 2 includes the second subperiod: all fiscal quarters which, in calendar time, correspond to or follow the fourth quarter of 2000, and precede the third quarter of 2002 (POST-REG-FD PRE-SOX). Model 3 includes the third subperiod: all remaining fiscal quarters which, in calendar time, correspond to, or follow, the third quarter of 2002 (POST-SOX). We find that the coefficient on *BNEWS* is not statistically significant in the PRE-REG-FD period, but positive and significant at the 1 percent level in the more recent POST-REG-FD and POST-SOX periods.

As reported in Panel B in Table 6, the implied *NEWS_RATIO* in the PRE-REG-FD subperiod for a firm with average values of the control variables is 15.2 percent in bad-news quarters, and 14.8 percent in good-news quarters; this small difference is not statistically significant at conventional levels. Interestingly, in the second subperiod, the implied *NEWS_RATIO* for bad-news quarters rises to 19.3 percent, while for good-news quarters, it decreases marginally to 13.2 percent. The increase in the implied *NEWS_RATIO* from the previous subperiod is significant at the 5 percent level for bad-news quarters. The results suggest that the changes in the information environment

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[24] The consensus analyst forecast is the mean across the final forecast provided by each analyst in a given announcement quarter. Using the median consensus analyst forecast yields similar results.

[25] Gintschel and Markov (2004) and Mohanram and Sunder (2006) conclude that REG FD largely succeeded in its objective of restricting selective voluntary disclosure. Further, Wang (2007) and Chen et al. (2011) document evidence suggesting that REG FD adversely affected the informativeness of voluntary disclosures. With respect to SOX, Cohen et al. (2007) document evidence consistent with firms having less flexibility in reporting earnings-increasing discretionary accruals in the post-SOX period.

TABLE 6

Subperiod Analysis of Earnings Differential Informativeness

Panel A: Regression Results

		Dependent Variable: *Ln*(*NEWS_RATIO*)		
	Predicted Sign	**Model 1 PRE-REG-FD Period**	**Model 2 POST-REG-FD PRE-SOX Period**	**Model 3 POST-SOX Period**
Intercept		3.250***	3.077***	3.709***
		(0.052)	(0.103)	(0.103)
BNEWS	+	0.022	0.377***	0.148***
		(0.029)	(0.047)	(0.030)
BIAS_ADJ		0.229***	0.219***	0.186***
		(0.007)	(0.014)	(0.007)
RInfoAsymm		−0.016***	−0.026***	−0.057***
		(0.002)	(0.007)	(0.005)
INSALE		0.007	−0.022	0.081***
		(0.019)	(0.021)	(0.018)
TRADE_DAYS		−0.007***	−0.005***	−0.007***
		(0.001)	(0.001)	(0.001)
RBTM		−0.001	−0.014***	0.001
		(0.002)	(0.004)	(0.002)
RLEV		−0.006***	−0.004	−0.020***
		(0.002)	(0.003)	(0.003)
HITECH		0.045***	−0.004	0.020
		(0.020)	(0.043)	(0.020)
S.E. Clustered by Firm- and Year-Qtr		Yes	Yes	Yes
n of observations		79,007	23,264	50,004
Adjusted R^2		5.79%	7.05%	5.29%

Panel B: Implied Ratios

	Model 1 PRE-REG-FD Period	**Model 2 POST-REG-FD PRE-SOX Period**	**Difference from PRE-REG-FD Period**	**Model 3 POST-SOX Period**	**Difference from POST-REG-FD PRE-SOX Period**
BNEWS = 1	15.17	19.28	4.11**	22.94	3.66**
BNEWS = 0	14.84	13.22	−1.62	19.79	6.56***

, * Represent statistical significance at a minimum 0.05 and 0.01 levels, respectively.

Table 6, Panel A, presents the results of OLS regressions with standard errors clustered by firm and fiscal year-quarter. The dependent variable is *Ln*(*NEWS_RATIO*). Model 1 includes all fiscal quarters which, in calendar time, precede the enactment of Regulation FD in the fourth quarter of 2000 (PRE-REG-FD). Model 2 includes all fiscal quarters which, in calendar time, correspond to, or follow, the fourth quarter of 2000, and precede the enactment of the Sarbanes-Oxley Act in the third quarter of 2002 (POST-REG-FD PRE-SOX). Model 3 includes all remaining fiscal quarters which, in calendar time, correspond to, or follow, the third quarter of 2002 (POST-SOX). Standard errors are reported in parentheses below coefficients. Panel B presents Implied Ratios, which are computed as the exponent of the predicted *Ln*(*NEWS_RATIO*) obtained by setting all control variables to their means except for *BIAS_ADJ*, which is set equal to 0, and *BNEWS*, equal to 1 or 0.

(continued on next page)

TABLE 6 (continued)

All other variables are defined in the notes to Tables 1 and 2.

Variable Definitions:
Ln(*NEWS_RATIO*) = natural logarithm of *NEWS_RATIO*;
NEWS_RATIO = 100 * *ABS*(*EAR*)/*ABS*(*NEAR*);
ABS(*EAR*) = absolute value of cumulative market-adjusted returns on trading days −1 to +1 relative to the QEA date;
ABS(*NEAR*) = absolute value of the cumulative market-adjusted non-earnings-announcement period returns; and
BNEWS = an indicator variable equal to 1 if *RET* is negative, and 0 otherwise.

around the passage of REG FD made earnings' role in conveying bad news more pronounced POST-REG FD.

Finally, Model 3 indicates that in the POST-SOX subperiod, the implied *NEWS_RATIO* for bad-news quarters is 22.9 percent, significantly higher than in the previous subperiod. The implied *NEWS_RATIO* for good-news quarters increases more sharply to 19.8 percent (relative to 13.2 percent in the second subperiod); this increase is statistically significant at the 1 percent level. The evidence suggests that SOX, along with the events surrounding its passage, enhanced the role of earnings in conveying both good and bad news, but that this effect was stronger with respect to good news.[26] The net effect is a decline in earnings' differential informativeness in bad-news quarters relative to good-news quarters in the POST-SOX period. Importantly, however, the differential informativeness of earnings remains significant in the POST-SOX period.

Management Forecasts Issued along with QEAs

In robustness analyses, we identify those firm-quarters in which QEAs are accompanied by management forecasts of earnings. For these observations, it is not possible to distinguish the part of the information released during the three-day QEA window that is attributable to the current quarter's earnings information from that attributable to the accompanying voluntary disclosure. Consequently, we reestimate our regressions after excluding these observations. The excluded observations constitute around 14.0 percent of the overall sample, and become increasingly prevalent with time. They constitute only 2.6 percent of the sample PRE-REG-FD, around 20.4 percent of the sample in the POST-REG-FD PRE-SOX subperiod, and around 29.0 percent of the sample in the POST-SOX subperiod. Our results on the primary hypothesis and cross-sectional variation are robust to the exclusion of observations in which QEAs are accompanied by management forecasts. Moreover, the general time-series patterns observed in Table 6 are robust to the exclusion, as well. Implied *NEWS_RATIO*s for the restricted sample during bad-news and good-news quarters are, respectively, 14.2 percent and 14.4 percent in the PRE-REG-FD subperiod, 17.8 percent and 12.5 percent in the POST-REG-FD PRE-SOX subperiod, and 20.3 percent and 17.1 percent in the POST-SOX subperiod.

VI. CONCLUSION

The collective evidence in our paper suggests that earnings play a valuable information role by mitigating delays in bad-news disclosures in the large majority of cases when litigation risk is not high enough to warrant preemptive voluntary disclosures. Using returns-based measures to capture news, we find that the ratio of news released during the three days around the QEA date to that released during the non-QEA period is significantly greater when quarterly returns are negative, relative to when they are positive. As expected, our evidence is concentrated among firms that do not issue short-horizon forecasts.

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[26] Consistent with our findings, Ball and Shivakumar (2008) also note the general increase in QEA informativeness with time (particularly post-SOX), although they do not condition on the sign of overall news.

Our cross-sectional tests provide evidence that earnings increased informativeness during bad-news quarters is even greater in firms characterized by higher information asymmetry between managers and shareholders, and in firm-quarters with net insider sales of stock. We also analyze whether earnings' differential informativeness varies by fiscal quarter and in time-series. We find that earnings' differential informativeness is concentrated in the third and the fourth fiscal quarters. Subperiod analysis indicates that earnings' informativeness in bad-news quarters, relative to good-news quarters, becomes more pronounced in the later part of our sample period, particularly after the passage of Regulation Fair Disclosure, and persists in the post-SOX period.

Our paper contributes to the existing literature by providing evidence that earnings information plays a crucial role in limiting delays in the release of bad news to the market. Direct evidence on the news earnings convey is particularly critical at this point in time, as the Financial Accounting Standards Board (FASB) considers new standards, presumably for the purpose of making earnings more informative (Johnson 2005; FASB 2007; Kothari et al. 2010). Our findings indicate that the standard-setters, in drafting the new standards, need to bear in mind that earnings' value as an information source may lie in the *ex post* settling-up role that they play with respect to negative information. In certain cases, as when litigation risk is high, the prospect of releasing negative news at the earnings announcements can prompt managers to disclose such news voluntarily in a timely manner. When factors such as litigation risk are not high enough to induce voluntary disclosures, and managers' incentives to delay bad news disclosures are more pronounced, earnings release the negative information that has not reached the market via alternative sources.

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APPENDIX A

NEWS_RATIO MAGNITUDES UNDER VARIOUS SCENARIOS

	***EAR, NEAR* of the Same Sign**		***EAR, NEAR* of Different Sign**	
	$EAR < 0$ $NEAR < 0$	$EAR > 0$ $NEAR > 0$	$EAR > 0$ $NEAR < 0$	$EAR < 0$ $NEAR > 0$
$RET < 0$				
Consistency with H1	Could be consistent or inconsistent	Not possible	Inconsistent	Consistent
Magnitude of *NEWS_RATIO*	High if consistent, Low if not	Not possible	Low	High
$RET \geq 0$				
Consistency with H1	Not possible	Could be consistent or inconsistent	Inconsistent	Consistent
Magnitude of *NEWS_RATIO*	Not possible	Low if consistent, High if not	High	Low

This appendix tabulates the scenarios under which *NEWS_RATIO* is expected to be relatively high versus relatively low. *NEWS_RATIO* is defined as 100 * *ABS(EAR)/ABS(NEAR)*. *EAR* is the cumulative market-adjusted returns on trading days −1 to +1 relative to the quarterly earnings announcement (QEA) date, *NEAR* represents the cumulative market-adjusted non-QEA period returns (spanning all trading days included in *RET* except for the trading days included in *EAR*). *ABS*(*EAR*) and *ABS*(*NEAR*) represent absolute values for *EAR* and *NEAR*, respectively. *RET* represents quarterly market-adjusted returns beginning two days after the QEA date for the previous quarter and ending one day after the QEA date for the current quarter.

To establish the patterns in *NEWS_RATIO*, first consider cases when *EAR* and *NEAR* are of the same sign. If it is true that earnings play a proportionately greater role in releasing bad news than good news, as our hypothesis states, then for cases when $RET < 0$, *ABS*(*EAR*) is expected to be high relative to *ABS*(*NEAR*). Similarly, when $RET > 0$, our hypothesis would imply that *ABS*(*EAR*) is expected to be low relative to *ABS*(*NEAR*).

Next, consider the scenarios with differing signs on *EAR* and *NEAR*, beginning with the case when $RET < 0$, $EAR < 0$, and $NEAR > 0$. This case is clearly consistent with our hypothesis: all of the negative information is released at the *QEA*s. Since overall returns are negative, by construction, *EAR* will be higher than *NEAR* in magnitude and *NEWS_RATIO* will be high. The scenario where $RET < 0$, $EAR > 0$, and $NEAR < 0$ is inconsistent with our hypothesis: all of the negative information is released during the non-QEA period. *EAR*, by construction, must be lower in magnitude than *NEAR*, since overall returns are negative, generating a low *NEWS_RATIO*. Thus, *NEWS_RATIO* functions well as an empirical proxy by assuming high values for negative-return observations, consistent with our hypothesis, and lower values for those that are inconsistent.

To generate the asymmetry predicted by our hypothesis, *NEWS_RATIO* also needs to assume lower values for all positive-*RET* observations that are consistent with our hypothesis, and higher values for those that are inconsistent. We examine the positive-*RET* cases with *EAR* and *NEAR* of differing signs to ensure that this is true. First, we focus on $RET > 0$, $EAR < 0$, and $NEAR > 0$. This case is consistent with our hypothesis: all of the positive information for the period reaches the market during the *non-QEA* period. Since overall returns are positive, by construction, *EAR* will be lower than *NEAR* in magnitude, and *NEWS_RATIO* will be low. Finally, in the positive-return case that is inconsistent with our hypothesis ($RET > 0$, $EAR > 0$, and $NEAR < 0$), *EAR*, by construction, has to be higher in magnitude than *NEAR*, since overall returns are positive, generating a higher *NEWS_RATIO*. In summary, variation in *NEWS_RATIO* is well suited to testing whether the relative informativeness of earnings is higher when overall information reaching the market is negative.

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Consecutive Earnings Surprises: Small and Large Trader Reactions

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ABSTRACT: Prior research demonstrates that investors respond differently to earnings surprises that are part of a string of consecutive earnings increases or surprises than to those that are not. To shed light on who values these patterns, I compare trading responses of small and large traders to earnings surprises that occur during a series of positive or negative surprises. I find that the relative intensity of small traders' trading response (and, to a lesser extent, that of medium traders) to earnings surprises generally increases as a series progresses. Small traders respond more negatively to the second (third) negative surprise in a series than to the first (second), and more positively for the first three surprises in a positive series. Moreover, I find that announcement-period returns are related to the trading of small and medium traders. These results suggest that less sophisticated smaller traders, responding to earnings series, contribute to previously documented pricing patterns.

Keywords: *earnings string; earnings pattern; earnings momentum; trade imbalance; small and large trade.*

Data Availability: *All data used in this study, with the exception of data obtained from an anonymous discount brokerage firm, are publicly available from the sources indicated in the text.*

I. INTRODUCTION

Prior research indicates that investors respond more strongly to earnings surprises that occur as part of a consistent series than to surprises that do not, but provides no evidence on which investors value these earnings patterns. In particular, Barth et al. (1999) find higher

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price-earnings multiples later in a "string" of consecutively increasing earnings. This paper compares the trading responses of investors who make large trades (large traders), whom research has shown to be sophisticated, and investors who make small trades (small traders), whom research has shown to be relatively naïve, to a similar pattern of earnings surprises that occur during a series of either positive or negative surprises. Since the two classes of investors differ in sophistication, differences in their response also sheds light on whether mispricing or risk drive the pricing patterns.

Following prior literature, I measure investor reactions using New York Stock Exchange Trade and Quote (TAQ) data, splitting trades into categories based on the dollar value of the trade. I classify the sign (buy versus sell) of the trades using the Lee-Ready algorithm (Lee and Ready 1991; Lee and Radhakrishna 2000; Odders-White 2000). This study examines each trade group's reaction to a "series" of consecutive "strong positive" and "strong negative" surprises, defined as the top 30 percent and bottom 30 percent of standardized unexpected earnings, respectively. If small traders exhibit a stronger reaction to consistent earnings patterns than large traders, then we should observe their relative trading response becoming more negative (positive) for a series of negative (positive) surprises.

The results show that small traders' reaction relative to large traders becomes more negative over a series of negative surprises, i.e., for the second surprise versus the first and the third versus the second. While the relative reaction becomes insignificantly less negative for the fourth or later surprises compared to the third, the relative reaction remains significantly more negative than the reaction to the first surprise. Similarly, results show that small traders' relative trade reaction becomes generally more positive for a series of positive surprises, although with an insignificant drop for fourth-and-later surprises. Small traders' relative reaction is significantly more positive for the third positive surprise than for the first or second, and for the fourth-and-later than for the first. I find similar results when comparing medium traders with large traders. In contrast, large traders react similarly to surprises in a series even when earnings surprises are defined using analyst forecasts, which proxy more strongly for large traders' expectations (Bhattacharya 2001; Battalio and Mendenhall 2005), suggesting that the relative-reaction patterns are not due to different earnings expectations of small, medium, and large traders. In contrast to trading during a series, small and large trader reactions to breaks in a series do not differ systematically with the length of the preceding series. Finally, additional analyses verify that small and medium trade imbalances are positively associated with concurrent returns around earnings announcements, even after controlling for large trades and the earnings surprise. These results provide evidence that smaller traders have a preference for consistent performance, as measured by an earnings surprise series, relative to large traders. The results are consistent with "earnings momentum" trading by small and medium traders being one driver of the return patterns documented in prior literature, at least for the first several surprises in a series.

This study contributes to the literature by providing evidence of which traders react positively to a series of consistent earnings surprises. Bhattacharya (2001) and Battalio and Mendenhall (2005) increase our understanding of post-earnings-announcement drift by showing that small traders respond most strongly to random-walk-based earnings surprises. This study increases our understanding of the market reactions to the time-series-pattern in earnings by building on that literature. Prior work that examines market responses to earnings patterns has focused on price- and returns-based tests (e.g., Barth et al. 1999; Kasznik and McNichols 2002; Myers et al. 2007). The present study's examination of the trading patterns of small and large traders has the advantage of assessing how different investors respond to the same announcement. This research design complements prior studies, providing evidence that "earnings-momentum" trading is stronger among smaller, less sophisticated investors.

One drawback to examining small and large trades is that the method is unlikely to be accurate after about 2002, as discussed in more detail in the "Data and Research Design" section. Therefore, I cannot say for certain that the results documented in this paper would extend to recent years. However, the ten-year sample period examined sheds significant light on the basic question of which investors react more strongly to a series of consistent earnings surprises.

The paper proceeds as follows. Section II describes the hypothesis development and related literature. Section III explains the empirical methods and data. Section IV presents the primary results and Section V presents additional analyses. Section VI concludes.

II. HYPOTHESIS DEVELOPMENT

Barth et al. (1999), Kasznik and McNichols (2002), Myers et al. (2007), and Lev et al. (2008) examine pricing multiples and stock returns related to patterns of consistent earnings increases or consistently positive earnings surprises, while Frieder (2008) investigates aggregate trade imbalances. All of these papers suggest that the market values consistent earnings patterns.

However, it is not clear what drives the market premium for consistency. Barth et al. (1999) suggest several possible explanations, which fall into two primary categories: (1) consistent earnings patterns may capture dimensions of growth or risk that the usual proxies for these variables do not capture, or (2) there may be an element of mispricing due to "earnings momentum" trading. Intuitively, an investor is engaged in earnings momentum trading if he/she buys (sells) stocks with positive (negative) earnings "momentum," i.e., multiple consecutive periods of positive (negative) earnings results. These two explanations lead to different predictions for investor trading, as described below.

Small and large traders are likely to differ in sophistication. First, investors making larger trades will invest more in acquiring information as fixed costs are employed to earn a larger dollar return. Conversely, an investor with better information will make a larger trade to exploit that information. Easley and O'Hara (1987) develop a theoretical model predicting that larger trades will incorporate more information and Hasbrouck (1988, 1991) provides empirical evidence consistent with this. Second, large trades tend to be made by institutional investors (Lee and Radhakrishna 2000). Experimental researchers have found that professionals are more successful in avoiding behavioral biases than nonprofessionals in several accounting and finance contexts (Smith and Kida 1991; Frederickson and Miller 2004). Finally, a growing body of empirical work finds that small traders make less sophisticated trading decisions than large traders. Bhattacharya (2001), Battalio and Mendenhall (2005), Lee (1992), and Hirshleifer et al. (2008) find that small traders buy stock following earnings announcements even if the earnings surprise is negative. Bhattacharya (2001) and Battalio and Mendenhall (2005) find that small traders rely more on a seasonal random-walk model to form their earnings expectations, even though analyst expectations impound more information. Odean (1999) and Barber and Odean (2000) show that discount brokerage account investors fail to earn returns to compensate for transaction costs and buy stocks that perform more poorly than the ones they sell. Bhattacharya et al. (2007) find differences between small and large trading around pro forma earnings announcements. Malmendier and Shanthikumar (2007, 2009), using the same trade data as this paper, and Mikhail et al. (2007) find that small investors react more naively to analyst recommendations and earnings forecasts than large traders. In all of these settings, small traders appear to make less sophisticated trading decisions than large traders. Turning to the effect of earnings series, if "earnings momentum" trading, based on behavioral factors, is a driver of the previously documented price patterns, then small, less sophisticated traders are likely to exhibit stronger earnings momentum trading than large traders.

While small and large traders appear to differ systematically in sophistication, they do not seem to differ systematically in their risk preferences. As Lee (1992) discusses, individuals have larger holdings in not only certain high-risk stocks, such as small firms and over-the-counter stocks, but also certain low-risk stocks such as utilities (Lee et al. 1991). Barsky et al. (1997) elicit risk preferences from 11,000 respondents and find no systematic relation between risk preferences and wealth or education. Brunnermeier and Nagel (2008) find that the share of households' liquid assets in risky assets does not vary with changes in wealth, suggesting that individuals' risk aversion remains steady as wealth changes. Finally, it is not clear how the risk aversion displayed by mutual fund managers compares to that of individuals. Prior literature has shown that for mutual funds, incentive fees increase risk-taking by losers but decrease risk-taking by winners (Elton et al. 2003). Absent a systematic difference in risk preferences between small and large traders, if the price patterns for earnings series are due to growth or risk, then we would expect large traders to react at least as strongly as small traders, in contrast to the prediction for "earnings momentum" trading.[1]

Examining small and large trades provides direct information on who responds to patterns of consistent earnings performance. Because small and large investors exhibit different levels of sophistication but similar risk preferences, it also provides insight on whether the pricing patterns are related to earnings momentum trading or firm risk. I state the hypothesis predicted by earnings-momentum trading as follows:

> **H1:** Small traders' net buying (selling) increases as a series of positive (negative) earnings surprises continues, relative to large traders.[2]

Section III discusses the empirical design in more detail. However one important issue is how to define an earnings "series." Barth et al. (1999) and Myers et al. (2007) focus on "strings" of five years of increasing annual and quarterly earnings, respectively. However, these definitions provide only 292 annual and 113 quarterly earnings strings for the sample for which I have trade data. In addition, small and large investors may view small earnings increases differently, making it difficult to attribute a difference in small and large trading to preferences for earnings momentum or differences in earnings expectations. For example, if results show that small traders react more strongly to a string of earnings increases than large traders, then it could be that both groups prefer "consistency" but only small traders view a 1 cent increase as good news. In contrast, strong earnings surprises are likely to be viewed as surprises by *both* investor groups. Thus, I define a "series" as a pattern of one to at least four consecutive quarters of strongly positive (top 30 percent), or negative (bottom 30 percent), earnings surprises. Because this definition of a "series" differs from corresponding definitions used in prior literature, I replicate the primary analysis in Barth et al. (1999) as a first step.

I conduct additional analyses as well. First, I test the relation between small trades and concurrent returns around earnings announcements to verify that small trades could potentially affect returns. Second, I examine trading after a break in a series. Finally, I conduct various robustness checks.

III. DATA AND RESEARCH DESIGN

The sample covers common stock for U.S. firms on the New York Stock Exchange (NYSE) between 1993 and 2002, including 2,723 firms and 59,658 earnings announcements. I take returns from CRSP, earnings data and firm characteristics from Compustat, analyst earnings forecasts from the Institutional Brokers Estimates System (I/B/E/S), and trade and quote data from the NYSE Trade and Quote (TAQ) database.

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[1] To see why sophisticated investors will react at least as strongly as unsophisticated investors if returns are due to changes in risk or growth, consider two possibilities for price-adjustment. First, consider the case where prices adjust immediately to the change in risk signaled by the earnings pattern. In this case, both groups hold stocks that are priced correctly, so we would not expect any differential reaction. Second, consider the case where there is some friction in price-adjustment and prices do not adjust immediately. By reacting (buying or selling), sophisticated investors can earn abnormal returns until prices reach equilibrium levels, so we would expect more sophisticated investors to react more strongly.

[2] The predictions for small and large traders are relative predictions. It may be the case that even large traders "prefer consistency." However, I argue that if the preference for consistency is based on behavioral factors, then small traders will exhibit a *stronger* preference. Since it is almost impossible to determine the correct trade response, I use large traders as the benchmark.

Measuring Earnings Surprise Events

I define the event date using the quarterly earnings announcement date from Compustat. The main measure of earnings surprise is standardized unexpected earnings, which adjusts for expectations without restricting the sample to firms with analyst coverage. This is important as large trades might dominate the generally larger and higher institutional-ownership firms that have analyst coverage. To calculate standardized unexpected earnings (SUE), I estimate the model used in Foster et al. (1984) and Bernard and Thomas (1989) and assume that earnings expectations are based on a seasonal random-walk with drift. The model defines expected earnings as:

$$E(e_t^i) = e_{t-4}^i + \delta^i, \tag{1}$$

where δ^i is the earnings drift for firm i. For each stock, I estimate drift as:

$$\hat{\delta}^i = (1/n)\sum_{j=1}^{n}(e_{t-j}^i - e_{t-j-4}^i), \tag{2}$$

where $n \leq 16$. Unexpected earnings are then standardized by the standard deviation of earnings, as measured by the available subset of the preceding 20 announcements. Thus:

$$SUE_t^i = \frac{e_t^i - e_{t-4}^i - \hat{\delta}^i}{\sqrt{Var(e_t^i)}}. \tag{3}$$

Finally, earnings announcements are ranked by SUE within each year.[3]

I use analyst forecasts to define an alternative measure of earnings surprises. The surprise is the difference between announced earnings-per-share and the most recent monthly median forecast before the announcement, normalized by stock price as of the forecast consensus date, when there are at least four earnings forecasts for the firm. Using this measure reduces the sample to 29,649 earnings announcements.

Defining the Earnings Surprise Series

I denote the position of a given surprise in an earnings surprise series by S. For a given surprise, S = 1 if the surprise is strongly negative (positive), i.e., in the bottom 30 percent (top 30 percent) of earnings surprises and the preceding surprise for that firm was not strongly negative (positive). S = 2 if it is the second surprise of the same type, strongly negative or positive, S = 3 if it is the third, and so on. An earnings surprise is assigned a value of S equal to 0 if it is neither a strongly negative nor strongly positive surprise, i.e., if it is in the middle 40 percent of earnings surprises. Figure 1 provides an example.

Table 1 reports sample statistics for the full sample and subsamples based on S. Between 40 percent and 60 percent of S = 1 and S = 2 surprises are followed by S = 2 and S = 3 surprises, respectively, for both positive and negative series, so that each surprise in the series is likely to be a surprise. The magnitude of standardized unexpected earnings does not vary systematically with S. Firm size and book-to-market do not vary systematically for positive earnings surprise series.

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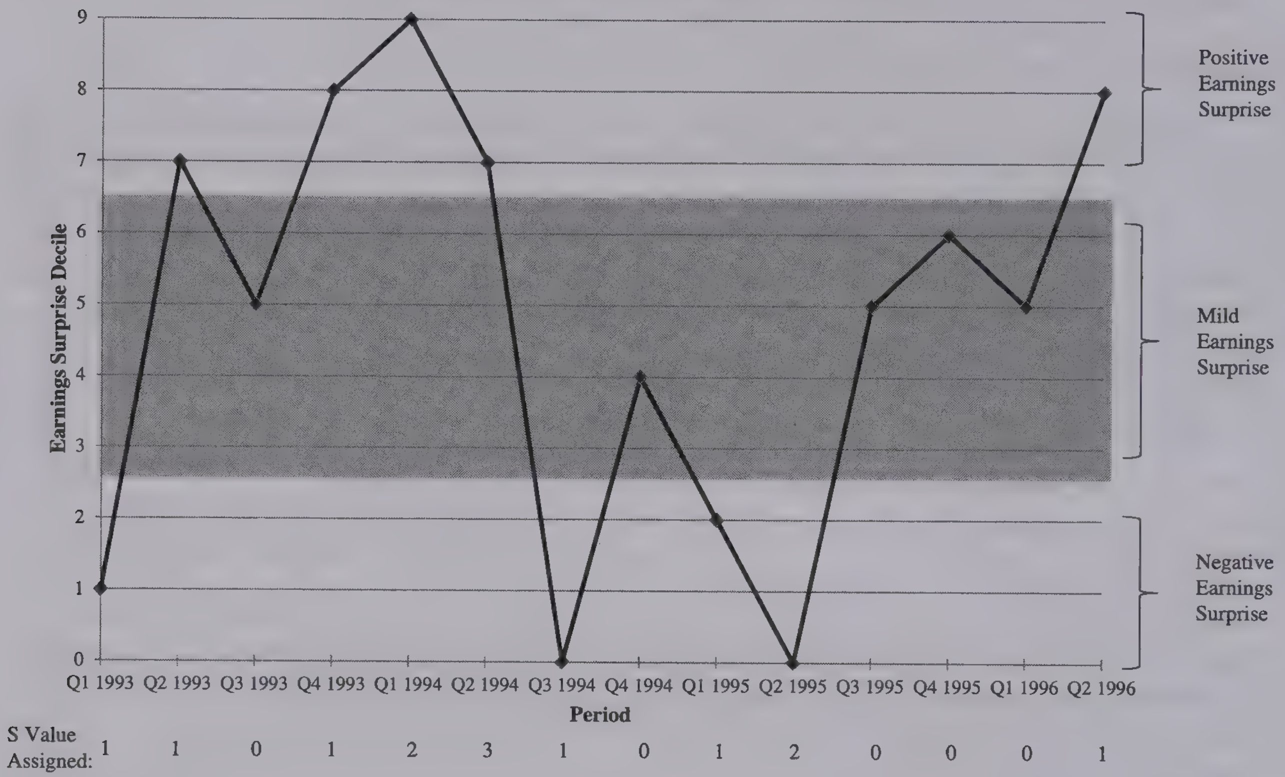
[3] Results are similar when I use a seasonal random-walk without drift and normalization of the unexpected earnings measure by the standard deviation of earnings changes.

FIGURE 1
Defining Earnings Surprise Series



This figure presents an example for the assignment of "S" values, where S marks how far into a series a given surprise falls. Earnings surprises are assigned to deciles (0 through 9) in each quarter, where a decile of 0, 1, or 2 indicates an earnings surprise in the most negative 30 percent, while a decile of 7, 8, or 9 indicates a surprise in the most positive 30 percent. If a surprise is the first in a series of same-type extreme surprises (bottom 30 percent, i.e., deciles 0, 1, or 2, or top 30 percent, i.e., deciles 7, 8, or 9), then it receives a value of S = 1. If it is second in a series, then S = 2 and so on. "Mild" surprises, in the middle 40 percent, i.e., deciles 3 through 6, are assigned S = 0.

However, for a negative series, firm size decreases and book-to-market increases. The change in book-to-market is driven by the decrease in market value. Book value of equity decreases with S for negative series; however, market value decreases more strongly. Given these patterns, I include size as a control variable in the analysis.

Measuring Small and Large Trading

Following Malmendier and Shanthikumar (2007), I restrict the sample to 1993–2002 common stock traded on the New York Stock Exchange (NYSE) to ensure a robust separation of small and large trades. Malmendier and Shanthikumar (2007) discuss some of the potential problems with these methods after 2002. Increased splitting of trades by institutions, internalization, program trading, and more frequent quote updating reduce the efficacy of the small/large trade distinction and the Lee-Ready algorithm.

I classify each NYSE trade from the TAQ data as buyer or seller initiated using the modified Lee and Ready (1991) algorithm recommended in Odders-White (2000). The algorithm involves matching a trade to the most recent quote preceding the trade by at least five seconds. If a price is nearer the bid (ask) price, then it is classified as seller (buyer) initiated. If a trade is at the bid-ask

TABLE 1

Earnings Surprise Sample Statistics

		All Surprises	Middle 40% of SUE	Top 30% of SUE				Bottom 30% of SUE			
			S = 0	S = 1	S = 2	S = 3	S ≥ 4	S = 1	S = 2	S = 3	S ≥ 4
Sample Size		52,421	20,196	6,925	3,029	1,759	5,510	7,100	3,182	1,896	2,824
Size	Mean	4,605	5,156	4,749	4,997	5,171	3,747	4,460	3,981	3,593	2,951
	Median	905	1,012	957	919	966	821	900	821	691	538
	Std. Dev.	16,557	17,779	16,765	18,832	21,049	16,594	14,086	14,094	13,139	9,968
B/M	Mean	0.522	0.514	0.431	0.555	0.524	0.448	0.489	0.647	0.667	0.758
	Median	0.483	0.470	0.485	0.468	0.440	0.393	0.518	0.540	0.568	0.601
	Std. Dev.	4.224	1.097	8.364	0.688	1.026	0.912	7.631	0.803	0.709	0.857
SUE	Mean	−0.282	0.050	1.568	1.600	1.640	1.617	−2.829	−2.156	−2.285	−2.416
	Median	0.050	0.060	0.990	1.120	1.210	1.200	−1.000	−1.200	−1.290	−1.340
	Std. Dev.	27.889	0.245	1.842	1.613	1.379	1.323	73.489	3.666	4.212	4.424

This table presents sample statistics for firm-quarters for NYSE common stock from 1993 through 2002, with matching TAQ trading data for each of the 11 days surrounding the earnings announcement (i.e., trading days [−5, 5]). If a surprise is the first in a series of same-type (top or bottom 30 percent) surprises, then it receives a value of S = 1. If it is the second in a series, then S = 2 and so on. S = 0 indicates a surprise that is not in a top/bottom 30 percent series, i.e., a surprise in the middle 40 percent.

Variable Definitions:
Size = market value and is reported in millions of dollars;
B/M = (book-to-market) ratio of book value of equity to market value of equity; and
SUE = standardized unexpected earnings based on a random-walk with drift earnings expectations model.

midpoint, then it is classified using a "tick test," comparing the trade price to the price of the previous trade.[4] To separate small and large trades I use four cutoffs: \$5,000, \$10,000, \$20,000, and \$50,000.[5]

The analyses in the paper focus on "small" trades of less than \$5,000, "medium" trades between \$5,000 and \$50,000, and "large" trades of at least \$50,000, motivated by Lee and Radhakrishna (2000) who show that dollar-based cutoffs create less noise in separating individuals from institutions than share-based cutoffs and that, for their three-month sample from 1990–1991, a very low (high) cutoff such as \$5,000 (\$50,000) is most effective in separating out individuals (institutions).[6,7]

Calculating Abnormal Trading Measures

This paper focuses on abnormal trade imbalance as the primary variable of interest, calculated following the method of Malmendier and Shanthikumar (2007).[8] Intuitively, if every trade after an announcement were initiated by the buy (sell) side, then the trading reaction to that announcement is extremely positive (negative). To capture this concept, raw trade imbalance is defined as:

$$IMB_{i,x,t} = \frac{buys_{i,x,t} - sells_{i,x,t}}{buys_{i,x,t} + sells_{i,x,t}} \tag{4}$$

for firm i, investor type x, and date t. Following Malmendier and Shanthikumar (2007), I normalize raw trade imbalance by subtracting the non-event-time firm-year mean, and dividing by the standard deviation, with:

$$IMB^{abnormal}_{i,x,t} = \frac{IMB_{i,x,t} - E\left(IMB_{i,x,year(t)}\right)}{\sqrt{Var\left(IMB_{i,x,year(t)}\right)}}, \tag{5}$$

where I exclude trading days −5 through 5 around earnings announcements in calculating $E(IMB_{i,x,year(t)})$ and $Var(IMB_{i,x,year(t)})$. The normalization thus controls for systematic differences in trading behavior for different stocks, trading groups, and times, but does not correct for differences in trading reactions to earnings announcements.

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[4] If the trade occurs at a price that is higher (lower) than the price of the previous trade, then it is classified as buyer (seller) initiated. The original Lee-Ready algorithm employs a "zero-tick" test in the case that a trade is at the bid-ask midpoint and the same price as the previous trade. Odders-White (2000) reports that the "zero-tick" misclassifies trades 40 percent of the time, and recommends leaving this 6 percent of trades unclassified. However, results are robust to using the original Lee-Ready algorithm for a large subsample of the data.

[5] The final dataset contains data from over 640 million trades, with each category containing a similar number of trades: 20.8 percent, 16.4 percent, 16.8 percent, 20.4 percent, and 25.6 percent for trades below \$5,000, between \$5,000 and \$10,000, between \$10,000 and \$20,000, between \$20,000 and \$50,000 and above \$50,000, respectively.

[6] Other papers use similar cutoffs. Lee (1992) uses a cutoff of \$10,000, with \$5,000 and \$20,000 as robustness checks. Bhattacharya (2001) uses dollar cutoffs of \$5,000 and \$50,000. Hvidkjaer (2006) uses firm-size dependent cutoffs ranging from less than \$5,000 to more than \$30,000.

[7] In addition, Griffin et al. (2003) find a strong relation between trade size and trader identity for their May 2000 through February 2001 sample of NASDAQ firms. Malmendier and Shanthikumar (2007) relate small and large trade imbalance to quarterly changes in institutional ownership and find significant correlations in the expected directions. In untabulated analysis, I relate the trade imbalance measures used in this paper to trades made through discount retail brokerage accounts from 1993–1996 and find the expected correlations. Finally, Barber et al. (2009) find significant correlations between trade initiation based on TAQ data and two brokerage data sets. Thus, small (large) trades are most likely to capture individual (institutional) trading.

[8] I modify the method used by Malmendier and Shanthikumar (2007) by excluding the 11 trading days around earnings announcements in calculating the "non-event-time" mean and standard deviation of the raw trade imbalance variables. Malmendier and Shanthikumar (2007) leave all trading days in their sample, as their analysis focuses on analyst buy/sell recommendations, a much more frequent event.

IV. RESULTS

Pricing Patterns for Series of Earnings Surprises

Because my definition of "series" differs from the various definitions of earnings strings used in prior literature, this section first replicates the primary analysis in Barth et al. (1999) for "series." Following Barth et al. (1999), I regress share price at quarter-end on net income per share, the position of a surprise in a series, and control variables for book value of equity, growth in book value of equity, earnings variance, debt-to-equity ratio, an indicator for banks and utilities, and indicators for different years. All control variables except book value of equity are interacted with net income. The key difference from Barth et al. (1999) is in the measure for an announcement's position in a series. This analysis uses indicators for $S = 1$, $S = 2$, $S = 3$, and $S \geq 4$, separately for top and bottom 30 percent surprises, whereas Barth et al. (1999) use an indicator for five consecutive years of increasing earnings, interacted with net income. Table 2 displays the results.

The results indicate that the first surprise in a series does not have an associated increase in price-to-earnings (P/E) multiples. In fact, the coefficient on $I(S = 1)$ is significantly positive (negative) for bottom 30 percent (top 30 percent) surprises, indicating that the market prices firms with $S = 1$ negative (positive) earnings surprises higher (lower) than would be predicted by a linear model of price to earnings, and other included variables, alone. This is not particularly surprising, since we might expect lower P/E multiples for firms with more extreme earnings surprises, where the market may expect those earnings to be less persistent. The key question is whether there are pricing patterns related with the *progression* of a series, as defined in this paper. The test statistics in the right-most column of Table 2 indicate that there are. Share price is significantly increasing (decreasing) for series of strongly positive (negative) SUE, for $S = 2$, 3, and $S \geq 4$, as compared to $S = 1$, 2, and 3, respectively, after controlling for the P/E multiples associated with net income and the other control variables. These results confirm that earnings surprise "series," as defined in this paper are associated with similar pricing patterns to the patterns documented in prior literature.[9]

Small, Medium, and Large Trade Reactions

This section presents results for the trading reactions of the different trade-size groups to a series of strong positive and strong negative surprises, testing H1. The primary specification is as follows:

$$\begin{aligned}\sum_{t-5}^{t+5} IMB^{abnormal}_{s,t} = {} & \beta^t_1 \mathrm{I}(lowSUE_e)*\mathrm{I}(S_e = 1) + \beta^t_2 \mathrm{I}(lowSUE_e)*\mathrm{I}(S_e = 2) + \beta^t_3 \mathrm{I}(lowSUE_e) \\ & *\mathrm{I}(S_e = 3) + \beta^t_4 \mathrm{I}(lowSUE_e)*\mathrm{I}(S_e \geq 4) + \beta^t_5 \mathrm{I}(highSUE_e)*\mathrm{I}(S_e = 1) \\ & + \beta^t_6 \mathrm{I}(highSUE_e)*\mathrm{I}(S_e = 2) + \beta^t_7 \mathrm{I}(highSUE_e)*\mathrm{I}(S_e = 3) + \beta^t_8 \mathrm{I}(highSUE_e) \\ & *\mathrm{I}(S_e \geq 4) + \beta^t_{SUE} SUE_e + \beta^t_{Q4} \mathrm{I}(Q_e = 4) + \beta^t_{Size} \ln(Size_e) + \beta^t_{IO} InstOwn_e \\ & + \beta^t_{TransIO} TransientInstOwn_e + \beta^t_{An} NumAn_e + \beta^t_{\ln(An)} \ln(NumAn)_e \\ & + \sum_{ind \in Industries} \beta^t_{ind} I(Industry(e) = ind) + \varepsilon_{e,t},\end{aligned} \tag{6}$$

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[9] In untabulated analysis, I replicate the Barth et al. (1999) model using interactions between S-value and net income (NI), with qualitatively similar results. Results show a significantly negative coefficient on $NI * I(S = 1)$ and significantly less negative coefficients for $S = 2$ and 3. The difference for $S = 3$ and $S \geq 4$ is insignificant, p-value = 0.11, however the value for $S \geq 4$ is significantly higher than the values for $S = 1$ and 2.

TABLE 2

Pricing Patterns for Series of Strong Positive/Negative Earnings

			Equality of Coefficients	
	Coeff. Est.	t-statistic	Test versus Coeff. On	p-value
I(Bottom 30% SUE) ∗ I(S = 1)	1.182***	5.75		
I(Bottom 30% SUE) ∗ I(S = 2)	−0.359	1.27	I(Bottom 30% SUE) ∗ I(S = 1)	0.00
I(Bottom 30% SUE) ∗ I(S = 3)	−1.273***	3.57	I(Bottom 30% SUE) ∗ I(S = 2)	0.00
I(Bottom 30% SUE) ∗ I(S ≥ 4)	−2.220***	7.19	I(Bottom 30% SUE) ∗ I(S = 3)	0.00
I(Top 30% SUE) ∗ I(S = 1)	−1.705***	8.39		
I(Top 30% SUE) ∗ I(S = 2)	−0.983***	3.43	I(Top 30% SUE) ∗ I(S = 1)	0.00
I(Top 30% SUE) ∗ I(S = 3)	0.130	0.35	I(Top 30% SUE) ∗ I(S = 2)	0.00
I(Top 30% SUE) ∗ I(S ≥ 4)	1.926***	6.91	I(Top 30% SUE) ∗ I(S = 3)	0.00
NI	13.624***	61.75		
BVE	0.891***	151.98		
I(banks and utilities)	−1.165***	6.24		
Growth	−8.424***	19.53		
Earnings Variance	−0.072***	14.03		
Debt/Equity	0.053***	4.38		
NI ∗ I(banks and utilities)	−6.809***	36.68		
NI ∗ Growth	−9.788***	22.16		
NI ∗ Earnings Variance	−0.135***	28.83		
NI ∗ (Debt/Equity)	0.039***	3.47		
Constant	42.050	36.41		
Indicators for Years 1994–2002	Yes			
NI ∗ Indicators for Years 1994–2002	Yes			
R^2	0.6660			
n	39,001			

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*, **, *** Indicate statistical significance of coefficient estimates at the 10 percent, 5 percent, and 1 percent levels, respectively.

This table presents coefficient estimates from robust regressions of share price on Net Income (per share), indicators for S value for negative (bottom 30 percent SUE) and positive (top 30 percent SUE) series, and control variables. If a surprise is the first in a series of same-type surprises (top or bottom 30 percent Standardized Unexpected Earnings), then S = 1. If it is the second, then S = 2 and so on. The sample contains quarterly earnings surprises for NYSE firms from 1993 through 2002. The rightmost two columns provide p-values for one-tailed tests for the differences between coefficients, testing for decreases in coefficients on I(Bottom 30% SUE) ∗ I(S = X) with X and increases for Top 30 percent SUE.

Variable Definitions:
BVE = book value of equity;
NI = net income per share;
I(banks and utilities) = an indicator that takes the value 1 if a firm is a bank or utility;
Growth = annualized growth in BVE from five years prior;
Earnings Variance = standard deviation of earnings changes relative to the same quarter in the prior year, over five years;
Debt/Equity = book value of debt over book value of equity; and
NI ∗ X = interaction of NI and variable X.

where t is the trading day in event-time, e is the specific earnings surprise, and $I(lowSUE_e)$ ($I(highSUE_e)$) is an indicator that the standardized unexpected earnings surprise value for e is in the bottom 30 percent (top 30 percent) for the given period. $I(S_e = (\geq) X)$ is the indicator that the earnings surprise e occurs in position X (occurs in position X or greater) in a series, for $X = 1$, 2, and 3 ($X = 4$), as defined in Section III. The dependent variable is the sum of abnormal trade imbalance over trading days −5 through 5 in event time, to control for potential differences in the timing of small and large trader reactions.[10] The remaining variables are control variables. SUE_e is the standardized unexpected earnings value.[11] $I(Q_e = 4)$ is an indicator for the fourth fiscal quarter. $Ln(Size_e)$ is the log of market value at the end of the fiscal quarter. $InstOwn_e$ is the percentage of shares owned by 13F reporting institutions. Bushee (1998) and Ke and Petroni (2004) provide evidence that "transient" institutional investors are more likely to trade on earnings information and earnings patterns than other institutional investors. Thus, the model includes the percentage of shares owned by transient institutions, $TransientInstOwn_e$.[12] $NumAn_e$ and $\ln(NumAn)_e$ capture the number of analysts reporting earnings forecasts for event *e*. Finally, the model includes industry fixed effects, based on two-digit SIC codes. Coefficients on the variables $I(low/highSUE_e) * I(S_e = X)$ and $I(low/highSUE_e) * I(S_e \geq X)$ reflect the sensitivity of a trade group's trading to the given earnings surprise category, controlling for the other factors. Thus, H1 concerns changes in the differences of β_i coefficients for small and large traders as S_e increases. For example, H1 predicts that small traders' relative reaction will become more negative for a negative series, which implies that $(\beta_{1Small} - \beta_{1Large} > \beta_{2Small} - \beta_{2Large})$.

Table 3 displays results for each trade size group, as well as statistics for the difference in coefficient estimates for trades less than \$5,000 versus trades of at least \$50,000, the focus of this study. Panel A displays regression results, while Panel B provides test statistics for the differences in coefficients for different positions in a series. Figure 2 displays the results for the β_i coefficients graphically. The top (bottom) half of Figure 2 focuses on series of negative (positive) surprises. Figure 2 shows that in terms of the magnitude of reaction, controlling for other factors, the smallest traders react more negatively (positively) for surprises that are later in a negative (positive) series, with the exception of $S \geq 4$ positive surprises. The reactions of larger traders depend much less on a surprises' location within a series. The test statistics in Table 3 report the statistical significance of the numbers plotted in Figure 2.

Looking at the $S = 1$ line in the top half of Figure 2, and at column 6 of Panel A of Table 3, we can see that small traders react significantly more positively than large traders to the first negative surprise in a series, but looking at the $S = 3$ and $S \geq 4$ lines in Figure 2, and column 6, we can see that they react significantly more negatively than large traders to the third and fourth-or-later surprises in a negative series. Looking to Panel B of Table 3, we can see that the relative reaction of small traders versus large traders becomes significantly more negative for $S = 2$ versus $S = 1$ and $S = 3$ versus $S = 2$. The reaction becomes slightly less negative for $S \geq 4$ versus $S = 3$, but the decrease in negativity is not statistically significant, and the $S \geq 4$ relative reaction remains significantly more negative than the $S = 1$ relative reaction. Thus, the results for negative series for $S = 1$, 2, and 3 support H1. The results for $S \geq 4$ are not consistent with H1, but are not statistically significant in either direction.

It may be surprising that small traders react more positively to the first negative surprise in a series than large traders, and that the difference between the two trader groups is insignificant for the second surprise in a negative series. However, one possible interpretation is that small traders have more of a preference for "earnings momentum" than large traders, but small traders start with a weaker reaction to the earnings surprises than large traders at the beginning of a series. Then the "earnings momentum" preference would result in small traders gradually reacting similarly to large traders, and possibly passing them.

Focusing on positive series, column 6 of Panel A shows that small traders react insignificantly more negatively than large traders to the first positive surprise in a series and insignificantly more

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[10] Results are similar using alternative event windows or excluding surprises without trading by the given trade-size group.

[11] I use three alternative models to control for the earnings surprise value, for robustness tests, with similar results. First, I include indicators for each SUE decile, second, standardized rank of SUE, and third, SUE, $I(SUE < 0)$, $SUE * I(SUE < 0)$ and SUE^2.

[12] I thank Brian Bushee for providing the transient institution data.

TABLE 3

Abnormal Trade Reaction to Earnings Surprise Series by Trade-Size Category

Panel A: Regression Results

	Trade Size Category in Thousands of Dollars					Difference
	< 5	**5–10**	**10–20**	**20–50**	**≥ 50**	**< 5 versus ≥ 50**
I(Bottom 30% SUE) * I(S = 1)	0.110	0.094	−0.002	−0.051	−0.105*	0.202**
	(1.29)	(1.27)	(0.02)	(0.79)	(1.75)	(2.00)
I(Bottom 30% SUE) * I(S = 2)	−0.111	−0.062	−0.089	−0.174**	0.075	−0.178
	(0.97)	(0.62)	(0.91)	(1.97)	(0.89)	(1.31)
I(Bottom 30% SUE) * I(S = 3)	−0.329**	−0.013	−0.087	−0.056	0.164	−0.489***
	(2.41)	(0.10)	(0.73)	(0.52)	(1.56)	(3.12)
I(Bottom 30% SUE) * I(S ≥ 4)	−0.468***	−0.307***	−0.333***	−0.187**	−0.107	−0.401***
	(4.06)	(3.01)	(3.42)	(2.03)	(1.15)	(2.73)
I(Top 30% SUE) * I(S = 1)	0.006	0.013	0.147**	0.074	0.124**	−0.106
	(0.08)	(0.18)	(2.14)	(1.14)	(2.01)	(1.06)
I(Top 30% SUE) * I(S = 2)	0.145	0.060	0.149	−0.004	0.193**	−0.028
	(1.30)	(0.60)	(1.57)	(0.04)	(2.20)	(0.19)
I(Top 30% SUE) * I(S = 3)	0.526***	0.374***	0.498***	0.269**	0.201**	0.261
	(3.73)	(3.06)	(4.28)	(2.41)	(1.97)	(1.54)
I(Top 30% SUE) * I(S ≥ 4)	0.215*	0.321***	0.204**	−0.001	0.052	0.174
	(1.86)	(3.27)	(2.09)	(0.01)	(0.60)	(1.22)
SUE	−0.0160	−0.0090	−0.019**	−0.0040	0.0120	−0.030**
	(1.42)	(1.01)	(2.13)	(0.44)	(1.34)	(2.46)
I(Fiscal Quarter 4)	−0.137**	−0.005	0.044	0.059	0.043	−0.166**
	(2.24)	(0.09)	(0.90)	(1.27)	(0.98)	(2.25)
Size	0.038	0.047**	0.006	0.002	0.006	0.056
	(1.35)	(1.98)	(0.27)	(0.11)	(0.30)	(1.54)
Institutional Ownership	−0.002	0.001	0.000	0.000	0.001	−0.003
	(0.97)	(0.94)	(0.05)	(0.25)	(0.79)	(1.55)
Transient Institutional Ownership	0.010***	0.008***	0.014***	0.007**	0.001	0.010**
	(2.83)	(2.78)	(4.72)	(2.43)	(0.19)	(2.29)
Number of Analysts	0.023*	0.014	0.010	0.015	0.002	0.026*
	(1.86)	(1.43)	(0.98)	(1.63)	(0.22)	(1.78)
ln(Number of Analysts)	−0.051	−0.022	0.033	0.012	0.029	−0.107
	(0.75)	(0.39)	(0.58)	(0.21)	(0.55)	(1.28)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	
R^2	0.0046	0.0038	0.0042	0.0034	0.0032	
n	34,013	39,173	39,606	39,661	39,690	

(continued on next page)

positively to the third and fourth. We can see these patterns in Figure 2. The increase in relative reaction is significant for $S = 3$ versus $S = 1$ and 2, and for $S \geq 4$ versus $S = 1$. We again see a decrease for $S \geq 4$ versus $S = 3$, but it is not statistically significant. Thus, the results for positive series for $S = 1$, 2, and 3 are generally supportive of H1, although they are not as strong as for negative series. The results for $S \geq 4$ are not consistent with H1, but are not significant in either direction. Consistent with Figure 2, the results are stronger and more consistent for negative series than for positive series. Including both the results for negative and positive series, I fail to reject H1.

TABLE 3 (continued)

Panel B: Coefficient Differences for Interaction Terms Estimated Above

	Trade Size Category in Thousands of Dollars					Difference
Coeff. Differences Between	**< 5**	**5–10**	**10–20**	**20–50**	**≥ 50**	**< 5 versus ≥ 50**
I(Bottom 30% SUE)						
(S = 2) and (S = 1)	−0.221	−0.156	−0.087	−0.123	0.180	**−0.380**
	(0.08)	(0.17)	(0.43)	(0.21)	(0.05)	**(0.01)**
(S = 3) and (S = 2)	−0.218	0.049	0.002	0.118	0.089	**−0.311**
	(0.18)	(0.74)	(0.98)	(0.34)	(0.48)	**(0.06)**
(S ≥ 4) and (S = 3)	−0.139	−0.294	−0.246	−0.131	−0.271	0.088
	(0.39)	(0.04)	(0.08)	(0.30)	(0.03)	(0.32)
(S = 3) and (S = 1)	−0.439	−0.107	−0.085	−0.005	0.269	**−0.691**
	(0.00)	(0.43)	(0.51)	(0.97)	(0.02)	**(0.00)**
(S ≥ 4) and (S = 2)	−0.357	−0.245	−0.244	−0.013	−0.182	−0.223
	(0.01)	(0.06)	(0.05)	(0.91)	(0.11)	(0.11)
(S ≥ 4) and (S = 1)	−0.578	−0.401	−0.331	−0.136	−0.002	**−0.603**
	(0.00)	(0.00)	(0.00)	(0.18)	(0.99)	**(0.00)**
I(Top 30% SUE)						
(S = 2) and (S = 1)	0.139	0.047	0.002	−0.078	0.069	0.078
	(0.25)	(0.67)	(0.98)	(0.43)	(0.46)	(0.30)
(S = 3) and (S = 2)	0.381	0.314	0.349	0.273	0.008	**0.289**
	(0.02)	(0.03)	(0.01)	(0.03)	(0.95)	**(0.07)**
(S ≥ 4) and (S = 3)	−0.311	−0.053	−0.294	−0.270	−0.149	−0.087
	(0.07)	(0.72)	(0.04)	(0.04)	(0.22)	(0.33)
(S = 3) and (S = 1)	0.520	0.361	0.351	0.195	0.077	**0.367**
	(0.00)	(0.01)	(0.00)	(0.10)	(0.48)	**(0.02)**
(S ≥ 4) and (S = 2)	0.070	0.261	0.055	0.003	−0.141	0.202
	(0.64)	(0.05)	(0.66)	(0.98)	(0.21)	(0.14)
(S ≥ 4) and (S = 1)	0.209	0.308	0.057	−0.075	−0.072	**0.280**
	(0.10)	(0.01)	(0.61)	(0.46)	(0.45)	**(0.04)**

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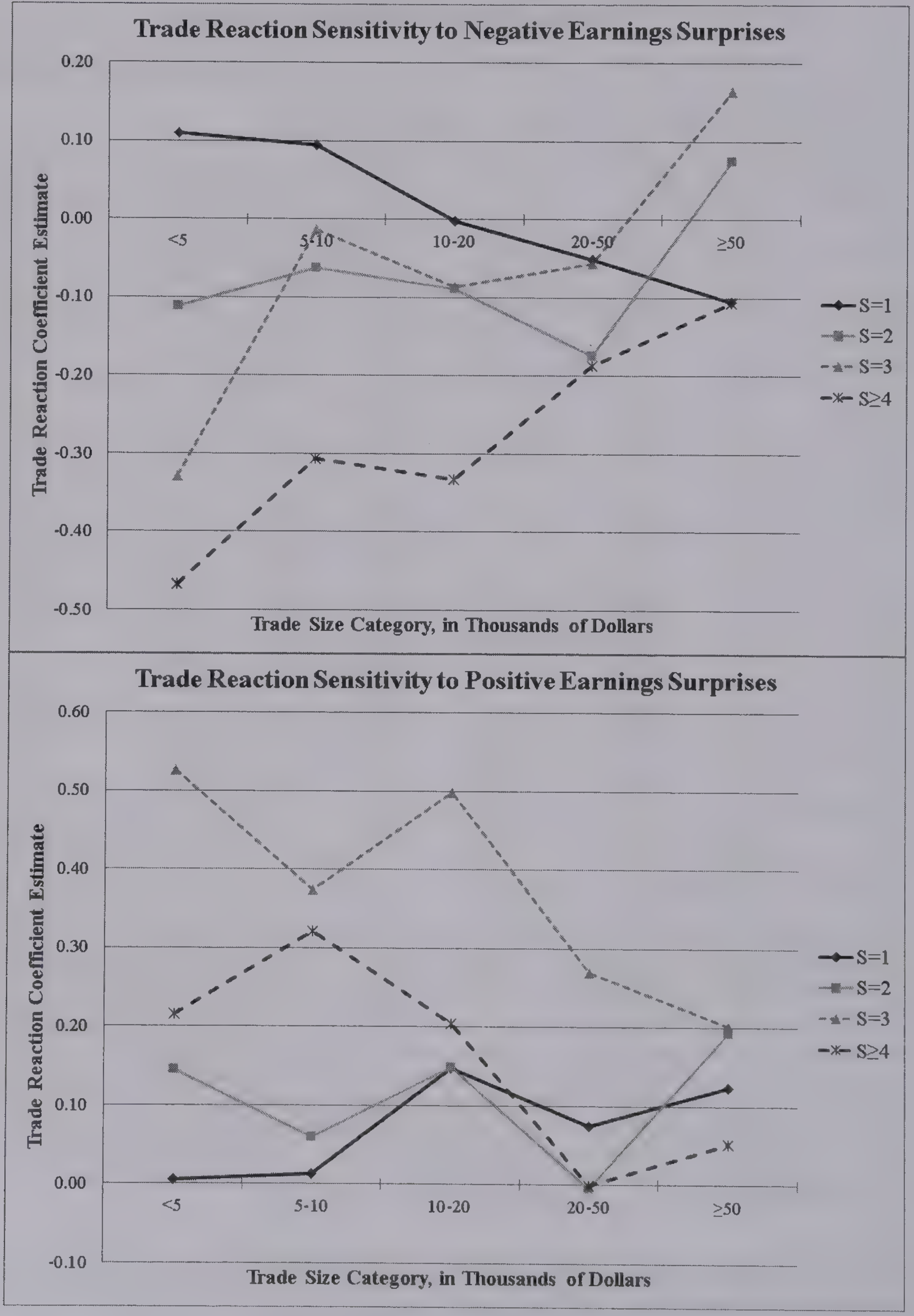
*, **, *** Indicate statistical significance of coefficient estimates at the 10 percent, 5 percent, and 1 percent levels, respectively.

Panel A presents coefficient estimates from regressions of abnormal trade imbalance, summed over trading days −5 through 5, on indicators for S value, separately for surprises in the top and bottom 30 percent of SUE, standardized unexpected earnings, and control variables. If a surprise is the first in a series of same-type surprises (top or bottom 30 percent), then S = 1. If it is the second, then S = 2 and so on. The sample contains quarterly earnings surprises for NYSE firms from 1993 through 2002. "Difference <5 versus ≥50" displays results for the difference between coefficient estimates for trades of <$5,000 and trades of ≥$50,000, estimated using a stacked regression, for the subset of observations with share price less than or equal to $50. For each column the sample is restricted to firm-days on which share price is low enough so that the given trade size is possible with a round lot of 100 shares. t-statistics are in parentheses. Standard errors are robust to heteroscedasticity and arbitrary within-firm correlation.

Panel B displays the differences between coefficient estimates reported in Panel A. p-values for the differences, based on F-tests, are in parentheses. Two-tailed tests are used for columns 1–5, and one-tailed tests are used for column 6. Significant values in Panel B column 6, at the 10 percent level, are shown in bold.

Variable Definitions:
I(Fiscal Quarter 4) = an indicator that takes the value 1 if the given quarter is the fourth quarter of the firm's fiscal year;
Ln(Size) = natural log of market value;
Institutional Ownership = percent of shares owned by 13F reporting institutions;
Transient Institution Ownership = percent of shares owned by institutions that are classified as "transient" following Bushee (1998);
Number of Analysts and ln(Number of Analysts) = number of analysts reporting forecasts of quarterly earnings for the firm in I/B/E/S and ln(Number of Analysts), respectively; and
Industry = as defined by the two-digit SIC code.

FIGURE 2
Trade Reaction to Earnings Surprises in a Series



This figure presents coefficient estimates from regressions of abnormal trade imbalance, summed over trading days −5 through 5, on indicators for S value for positive (top 30 percent) and negative (bottom 30 percent) SUE series, and control variables, as described in Table 3. The figure plots the relevant coefficient estimates from Table 3. Thus each point represents the sensitivity of the given trade-size-group's trade imbalance to earnings surprises with position S in a positive or negative series, controlling for other factors that may drive trade imbalance.

For completeness, Table 3 reports trade responses for each of five trade-size categories. As seen in Figure 2, the results indicate that as trade size grows, the reaction to the first ($S \geq 4$) negative surprise becomes more negative (less negative) in magnitude. Investors' reaction to the first (third) positive surprise is generally, although not monotonically, increasing (decreasing) with trade-size. The pattern for $S \geq 4$ is more mixed. These results, overall, suggest that there is a gradual progression between the behavior of the very small traders and the very large traders. In fact, for the relative reaction of trades between $5,000 and $50,000 versus trades larger than $50,000, three of the six relevant difference-of-differences for negative series are significantly negative and four of the six relevant difference-of-differences for positive series are significantly positive. Thus, it is not just small traders who exhibit a relative preference for "earnings momentum"; medium traders exhibit a preference relative to large traders as well.

The gradual progression is important to evaluating the potential market impact of earnings momentum trading. I directly examine the relation between the return response to earnings surprises and small trading in Section V. However, the appearance of relative earnings momentum trading among medium traders suggests that earnings momentum trading may have a larger price impact than if it appeared only for the smallest traders.

Finally, although Equation (6) includes the earnings surprise value, SUE, as a control variable, it still could be that pooling deciles 0–2 and deciles 7–9 affects results. I modify Equation (6) to include interaction terms between indicators for each SUE decile and S value. Results for the change in the relative reaction of small versus large traders with respect to S are reported in Table 4. The difference-of-difference results reported in Table 4 are analogous to column 6 of Table 3 Panel B. Results are generally consistent. No single decile appears to drive the overall results, nor are there statistically significant results contrary to H1 in any decile. The results by decile, as with the main results, are stronger for bottom 30 percent surprises than top 30 percent, and are in the opposite direction but statistically insignificant for $S \geq 4$ versus $S = 3$ for four of the six relevant deciles. Overall, these results show that the relative reaction of small versus large traders is stronger for earnings surprises later in a series whether those surprises are in the most extreme deciles or are closer to the median.

V. ADDITIONAL ANALYSIS

The Market Impact of Small Trades

Recent research has found that trading by retail investors and small traders is correlated both across investors and across securities (Kumar and Lee 2006; Barber et al. 2009). Because of this, the trades of even very small investors can have a significant impact on the market (see De Long et al. 1990; Baker and Wurgler 2006; Kumar and Lee 2006; Barber et al. 2009). The results presented in Section IV show earnings momentum trading by small and medium traders, relative to large traders, consistent with the previously documented market valuation of earnings growth consistency. This section directly tests the relation between small and medium trading and the concurrent market return around earnings announcements to verify that small and medium traders could be affecting returns.

Table 5 displays results for the relation of returns over days [−5, 5] with trading over the same window. The results show that trades of less than $5,000 exhibit a significant positive relation with concurrent returns. A one standard deviation change in the trade imbalance for trades less than $5,000 corresponds to returns of 0.047 percent over the same window, a change in returns of roughly 7 percent. The second column includes control variables for large trades and SUE (using the standardized rank of SUE, ranging from 0 to 1). While the magnitude of the coefficient on small trades decreases, it remains positive and statistically significant. Trades between $5,000 and $50,000 also relate significantly to concurrent returns. While this analysis does not establish

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TABLE 4

Relative Abnormal Trade Reaction to Earnings Surprise Series, by Earnings Surprise Decile

	Earnings Surprise Decile					
Coeff. Differences Between:	**0**	**1**	**2**	**7**	**8**	**9**
(S = 2) and (S = 1)	**−0.575**	−0.290	−0.313	−0.215	−0.058	**0.531**
	(0.02)	(0.12)	(0.12)	(0.20)	(0.41)	**(0.03)**
(S = 3) and (S = 2)	−0.116	−0.199	**−0.767**	0.295	**0.560**	−0.070
	(0.37)	(0.27)	**(0.02)**	(0.21)	**(0.05)**	(0.42)
(S ≥ 4) and (S = 3)	0.135	−0.132	0.323	0.137	−0.341	−0.014
	(0.34)	(0.34)	(0.20)	(0.36)	(0.16)	(0.48)
(S = 3) and (S = 1)	**−0.691**	**−0.489**	**−1.080**	0.080	**0.502**	**0.461**
	(0.01)	**(0.05)**	**(0.00)**	(0.40)	**(0.05)**	**(0.07)**
(S ≥ 4) and (S = 2)	0.019	−0.331	**−0.444**	**0.432**	0.219	−0.084
	(0.47)	(0.13)	**(0.10)**	**(0.09)**	(0.25)	(0.40)
(S ≥ 4) and (S = 1)	**−0.556**	**−0.621**	**−0.757**	0.217	0.161	**0.447**
	(0.02)	**(0.01)**	**(0.01)**	(0.23)	(0.26)	**(0.05)**

This table presents difference-of-difference results from regressions of small and large trade on indicators for SUE decile interacted with S value as well as control variables defined below and fixed effects for industry, as defined by the two-digit SIC code. If a surprise is the first in a series of same-type surprises (top or bottom 30 percent), then S = 1. If it is the second, then S = 2 and so on. The sample contains quarterly earnings surprises for NYSE firms from 1993 through 2002. The difference between coefficient estimates for trades of <\$5,000 and trades of ≥\$50,000 is estimated using a stacked regression for the subset of observations with stock price less than or equal to \$50. Standard errors are robust to heteroscedasticity and arbitrary within-firm correlation. p-values for the difference-in-differences are shown in parentheses, based on F-tests. One-tailed tests are used. Significant values are shown in bold.

Variable Definitions:
SUE = standardized unexpected earnings;
I(Fiscal Quarter 4) = an indicator that takes the value 1 if the given quarter is the final quarter of the firm's fiscal year;
ln(Size) = natural log of market value;
Institutional Ownership = percent of shares owned by 13F reporting institutions;
Transient Institution Ownership = percent of shares owned by institutions that are classified as "transient" following Bushee (1998); and
Number of Analysts and ln(Number of Analysts) = number of analysts reporting forecasts of quarterly earnings for the firm in I/B/E/S and ln(Number of Analysts), respectively.

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causation, the results are consistent with smaller traders' earnings momentum trading contributing to the pricing patterns observed for series.[13]

Breaks in Series

Barth et al. (1999) show that the higher P/E multiples for earnings strings disappear after a "break" in the string, the first earnings decrease after a string of increases.[14] Examining breaks serves two purposes. First, as a robustness test, if omitted firm characteristics drive the increasing reaction pattern of small traders relative to large traders, then to the extent that these firm characteristics are persistent, they should affect trading in "Break" quarters as well. Second, the test relates to the prior

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[13] Small and medium trades have a correlation coefficient of 0.47. If both small and medium trades are included in a regression of earnings-announcement returns, the coefficient on medium trades remains statistically significant, but the coefficient on small trades becomes insignificant.

[14] In untabulated analyses I estimate tests similar to those in Barth et al. (1999), but for breaks in a series. Consistent with the results for strings in Barth et al. (1999), I find that the premium for series decreases when a break occurs, becoming insignificant for breaks in positive series and decreasing significantly for breaks in negative series.

TABLE 5

Relation between Small and Medium Trade Imbalances and Concurrent Returns around Earnings Announcement Dates

	Returns			
Trade Imbalance, < \$5,000	0.047%*** (5.40)	0.023%*** (2.62)		
Trade Imbalance, \$5,000 to \$50,000			0.044%*** (9.42)	0.017%*** (3.56)
Trade Imbalance, ≥ \$50,000		0.349%*** (34.46)		0.340%*** (34.21)
Standardized Rank of SUE		2.590%*** (18.97)		2.530%*** (19.20)
Constant	0.656%*** (17.00)	−0.649%*** (8.35)	0.646%*** (17.32)	−0.622%*** (8.25)
R^2	0.0007	0.0409	0.0032	0.0415
n	49,640	49,640	52,174	52,174

*, **, *** Indicate statistical significance of coefficient estimates, at the 10 percent, 5 percent, and 1 percent level, respectively.

This table presents coefficient estimates from regressions of cumulative raw returns, summed over trading days −5 through 5 around earnings announcement dates, on abnormal trade imbalance, summed over the same window, a constant, and the standardized rank of the earnings surprise (SUE) ranging from 0 to 1. The earnings surprise sample contains all earnings surprises for NYSE sample firms from 1993 through 2002. t-statistics are in parentheses. Standard errors are robust to heteroscedasticity and arbitrary within-firm correlation.

literature on breaks in strings. Table 6 presents results for Equation (6) augmented with indicators for a "break" in a series. "I(Break from Bottom 30 percent (Top 30 percent) SUE Series, S = (≥)X)" is the first surprise in the top 70 percent (bottom 70 percent) following a negative (positive) series of length X. For parsimony, the table presents only the coefficients on "Break" terms; however, all other variables in Equation (6) are included in the estimation.

The results in Table 6 indicate that small traders' relative reaction to a break does not depend on the length of a series. While small traders' relative reaction versus large traders becomes more negative (positive) for a longer negative (positive) series, their relative reaction at the break does not seem to vary systematically with the length of the series. For negative series, small and large trader reactions to breaks do not differ significantly for any of the four breaks examined. The difference-of-differences are also largely insignificant, with only one of the six being significant: a significant increase in the relative reaction for $S \geq 4$ versus $S = 3$. Small and large trader reactions to breaks in positive series only differ significantly for $S = 2$, and only two of the difference-of-differences are significant: a significant increase from $S = 1$ to $S = 2$, and $S = 1$ to $S \geq 4$. Again, there is no systematic pattern in the relative reaction to the break as S increases.

Overall, these results suggest that, first, omitted firm characteristics are not driving the main results, to the extent that these firm characteristics persist for at least one quarter. Second, these results suggest that small and large traders react similarly when a series breaks. This could be consistent with earnings momentum trading creating an "over-reaction" that is corrected when a series breaks, where small traders are more involved in the over-reaction, but large traders are equally involved in the correction.

Analyst-Based Earnings Surprise

One potential concern with the main results is that large traders' earnings expectations may be captured more accurately with analyst forecasts (Bhattacharya 2001; Battalio and Mendenhall

TABLE 6

Abnormal Trade Reaction to Breaks in Earnings Surprise Series

	Trade Size Category in Thousands of Dollars			Difference
	< 5	5–50	≥ 50	< 5 versus ≥ 50
I(Break from Bottom 30% SUE Series, S = 1)	−0.049	−0.083	−0.100	0.029
	(0.51)	(0.44)	(1.37)	(0.24)
I(Break from Bottom 30% SUE Series, S = 2)	−0.092	−0.235	−0.033	−0.052
	(0.64)	(0.80)	(0.30)	(0.29)
I(Break from Bottom 30% SUE Series, S = 3)	−0.056	0.222	0.183	−0.317
	(0.26)	(0.53)	(1.05)	(1.17)
I(Break from Bottom 30% SUE Series, S ≥ 4)	0.122	0.409	−0.143	0.202
	(0.85)	(1.44)	(1.22)	(1.09)
I(Break from Top 30% SUE Series, S = 1)	−0.141	0.313	−0.169	−0.025
	(0.69)	(0.77)	(1.12)	(0.10)
I(Break from Top 30% SUE Series, S = 2)	0.379	0.037	−0.475*	0.918**
	(1.14)	(0.06)	(1.85)	(2.15)
I(Break from Top 30% SUE Series, S = 3)	0.125	1.322	−0.164	0.337
	(0.23)	(1.34)	(0.37)	(0.49)
I(Break from Top 30% SUE Series, S ≥ 4)	0.299	1.023*	−0.308	0.537
	(1.05)	(1.81)	(1.30)	(1.46)
Terms for (Bottom [Top] 30% SUE) ∗ I(S = [≥]X)	Yes	Yes	Yes	
Control Variables and Fixed Effects	Yes	Yes	Yes	
R^2	0.0048	0.0050	0.0034	
n	34,013	39,661	39,690	

*, **, *** Indicate statistical significance of coefficient estimates, at the 10 percent, 5 percent, and 1 percent level, respectively.

This table presents coefficient estimates from regressions of abnormal trade imbalance, summed over trading days −5 through 5, on indicators for S value, separately for positive and negative series, control variables defined below, and fixed effects for industry, as defined by two-digit SIC code, as in Equation (6), supplemented by indicators for "Break" quarters. If a surprise is the first in a series of same-type (top or bottom 30 percent) surprises, then S = 1. If it is the second, then S = 2 and so on. If a surprise is the first after a series of length S that is not a part of the series, then it is a Break surprise. The sample contains quarterly earnings surprises for NYSE firms from 1993 through 2002. "Difference <5 versus ≥50" displays results for the difference between coefficient estimates for trades of <\$5,000 and trades of ≥\$50,000, estimated using a stacked regression, for the subset of observations with stock price less than or equal to \$50. For each column the sample is restricted to firm-days on which stock price is low enough so that the given trade size is possible with a round lot of 100 shares. t-statistics are in parentheses. Standard errors are robust to heteroscedasticity and arbitrary within-firm correlation.

Variable Definitions:
SUE = standardized unexpected earnings;
I(Fiscal Quarter 4), = an indicator that takes the value 1 if the given quarter is the final quarter of the firm's fiscal year;
ln(Size) = natural log of market value;
Institutional Ownership = percent of shares owned by 13F reporting institutions;
Transient Institution Ownership = percent of shares owned by institutions that are classified as "transient" following Bushee (1998); and
Number of Analysts and ln(Number of Analysts) = number of analysts reporting forecasts of quarterly earnings for the firm in I/B/E/S and ln(Number of Analysts), respectively.

2005). In particular, we may only observe an increasing large-trade response by examining analyst-based surprises. Table 7, Panel A, displays results for earnings surprises and series defined using analyst forecasts. While large traders do appear to react consistently with analyst-based surprises, with positive significant coefficients on I(Top 30 percent Surprise) ∗ I(S = X) for X = 1 and 2, and a

TABLE 7

Alternate Earnings Surprise and Alternate Trade Imbalance Measures

Panel A: Alternate Earnings Surprise

	Analyst-Based Surprise				Positive/Negative SUE			
	< 5	5–50	≥ 50	< 5 versus ≥ 50	< 5	5–50	≥ 50	< 5 versus ≥ 50
I(Negative Surprise)	0.183*	0.843***	−0.234***	0.422***				
* I(S = 1)	(1.85)	(3.92)	(3.08)	(3.59)				
I(Negative Surprise)	0.065	0.282	−0.056	0.065	−0.287***	−0.574***	0.074	−0.361***
* I(S = 2)	(0.38)	(0.85)	(0.50)	(0.33)	(2.76)	(2.67)	(0.89)	(2.80)
I(Negative Surprise)	−0.295	0.266	0.004	−0.261	−0.357***	−0.572**	0.178*	−0.517***
* I(S = 3)	(1.31)	(0.59)	(0.02)	(0.93)	(3.02)	(2.35)	(1.95)	(3.70)
I(Negative Surprise)	−0.074	0.278	−0.209	0.162	−0.420***	−0.596***	0.074	−0.508***
* I(S ≥ 4)	(0.32)	(0.67)	(1.30)	(0.57)	(4.43)	(3.14)	(0.98)	(4.29)
I(Positive Surprise)	−0.162	−0.185	0.336***	−0.569***	−0.211**	−0.350*	0.252***	−0.468***
* I(S = 1)	(1.61)	(0.90)	(4.77)	(4.63)	(2.30)	(1.85)	(3.49)	(4.10)
I(Positive Surprise)	−0.128	0.182	0.330***	−0.467***	−0.209**	−0.468**	0.184**	−0.396***
* I(S = 2)	(0.87)	(0.65)	(3.08)	(2.61)	(2.01)	(2.11)	(2.26)	(2.99)
I(Positive Surprise)	−0.460**	−0.178	0.220	−0.713***	−0.035	−0.136	0.187**	−0.246*
* I(S = 3)	(2.19)	(0.42)	(1.38)	(2.79)	(0.30)	(0.57)	(2.05)	(1.69)
I(Positive Surprise)	0.062	0.379	0.172	−0.128	0.118	0.120	0.151**	−0.019
* I(S ≥ 4)	(0.32)	(1.05)	(1.32)	(0.59)	(1.36)	(0.68)	(2.22)	(0.18)
Control Variables and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0064	0.0054	0.0055		0.0048	0.0046	0.0030	
n	19,648	24,409	24,429		34,013	39,661	39,690	

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TABLE 7 (continued)

Panel B: Coefficient Differences for Interaction Terms Estimated in Panel A

	Analyst-Based Surprise				Positive/Negative SUE			
Coeff. Differences Between	**< 5**	**5–50**	**≥ 50**	**< 5 versus ≥ 50**	**< 5**	**5–50**	**≥ 50**	**< 5 versus ≥ 50**
I(Negative Surprise)								
(S = 2) and (S = 1)	−0.118	−0.561	0.178	**−0.357**				
	(0.50)	(0.12)	(0.14)	**(0.04)**				
(S = 3) and (S = 2)	−0.360	−0.016	0.060	−0.326	−0.070	0.002	0.104	−0.156
	(0.18)	(0.98)	(0.77)	(0.16)	(0.58)	(0.99)	(0.30)	(0.15)
(S ≥ 4) and (S = 3)	0.221	0.012	−0.213	0.423	−0.063	−0.024	−0.104	0.009
	(0.48)	(0.98)	(0.35)	(0.13)	(0.59)	(0.92)	(0.27)	(0.47)
(S = 3) and (S = 1)	0.112	−1.109	0.230	**−0.161**				
	(0.04)	(0.22)	(0.20)	**(0.01)**				
(S ≥ 4) and (S = 2)	−0.139	−0.004	−0.153	0.097	−0.133	−0.022	0.000	−0.147
	(0.61)	(0.99)	(0.40)	(0.39)	(0.23)	(0.92)	(0.99)	(0.14)
(S ≥ 4) and (S = 1)	−0.257	−0.565	0.025	−0.260				
	(0.27)	(0.20)	(0.88)	(0.19)				
I(Positive Surprise)								
(S = 2) and (S = 1)	0.034	0.367	−0.006	0.102	0.002	−0.118	−0.068	0.072
	(0.84)	(0.24)	(0.96)	(0.30)	(0.98)	(0.57)	(0.38)	(0.28)
(S = 3) and (S = 2)	−0.332	−0.360	−0.110	−0.246	0.174	0.332	0.003	0.150
	(0.16)	(0.45)	(0.54)	(0.20)	(0.15)	(0.17)	(0.98)	(0.16)
(S ≥ 4) and (S = 3)	0.522	0.557	−0.048	**0.585**	0.153	0.256	−0.036	**0.227**
	(0.06)	(0.28)	(0.81)	**(0.03)**	(0.17)	(0.24)	(0.67)	**(0.05)**
(S = 3) and (S = 1)	−0.298	0.007	−0.116	−0.144	0.176	0.214	−0.065	**0.222**
	(0.18)	(0.99)	(0.48)	(0.30)	(0.14)	(0.34)	(0.47)	**(0.07)**
(S ≥ 4) and (S = 2)	0.190	0.197	−0.158	**0.339**	0.327	0.588	−0.033	**0.377**
	(0.39)	(0.65)	(0.31)	**(0.09)**	(0.00)	(0.00)	(0.66)	**(0.00)**
(S ≥ 4) and (S = 1)	0.224	0.564	−0.164	**0.441**	0.329	0.470	−0.101	**0.449**
	(0.27)	(0.14)	(0.24)	**(0.03)**	(0.00)	(0.01)	(0.12)	**(0.00)**

(continued on next page)

TABLE 7 (continued)

Panel C: Alternate Trade Imbalance Measures

	a				b			
	< 5	5–50	≥ 50	< 5 versus ≥ 50	< 5	5–50	≥ 50	< 5 versus ≥ 50
I(Bottom 30% SUE) ∗ I(S = 1)	0.049	−0.220	−0.078	0.116	−0.097*	−0.123**	−0.139***	0.031
	(0.60)	(1.36)	(1.34)	(1.20)	(1.67)	(2.37)	(2.82)	(0.41)
I(Bottom 30% SUE) ∗ I(S = 2)	−0.150	−0.490**	0.069	−0.207	−0.262***	−0.306***	−0.035	−0.219**
	(1.35)	(2.24)	(0.85)	(1.56)	(3.33)	(4.30)	(0.50)	(2.07)
I(Bottom 30% SUE) ∗ I(S = 3)	−0.428***	−0.402	0.143	−0.562***	−0.398***	−0.160*	−0.009	−0.415***
	(3.22)	(1.53)	(1.41)	(3.73)	(4.14)	(1.82)	(0.11)	(3.40)
I(Bottom 30% SUE) ∗ I(S ≥ 4)	−0.497***	−0.914***	−0.071	−0.468***	−0.554***	−0.377***	−0.147*	−0.455***
	(4.47)	(4.28)	(0.79)	(3.33)	(6.33)	(4.94)	(1.87)	(3.91)
I(Top 30% SUE) ∗ I(S = 1)	0.036	0.311**	0.077	−0.020	0.066	0.138***	0.131***	−0.074
	(0.46)	(2.00)	(1.29)	(0.21)	(1.12)	(2.66)	(2.63)	(0.94)
I(Top 30% SUE) ∗ I(S = 2)	0.131	0.250	0.118	0.041	0.136*	0.217***	0.123*	0.023
	(1.20)	(1.12)	(1.39)	(0.30)	(1.72)	(3.15)	(1.73)	(0.22)
I(Top 30% SUE) ∗ I(S = 3)	0.521***	1.278***	0.114	0.357**	0.294***	0.511***	0.193**	0.046
	(3.82)	(4.76)	(1.15)	(2.17)	(2.82)	(5.91)	(2.39)	(0.35)
I(Top 30% SUE) ∗ I(S ≥ 4)	0.173	0.511**	−0.007	0.196	0.371***	0.274***	−0.037	0.428***
	(1.54)	(2.25)	(0.08)	(1.40)	(4.27)	(3.81)	(0.51)	(3.63)
Control Variables and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0050	0.0061	0.0028		0.0115	0.0103	0.0058	
n	34,013	39,661	39,690		34,013	39,661	39,690	

(continued on next page)

TABLE 7 (continued)

Panel D: Coefficient Differences for Interaction Terms Estimated in Panel C

	a				b			
Coeff. Differences Between	**< 5**	**5–50**	**≥ 50**	**< 5 versus ≥ 50**	**< 5**	**5–50**	**≥ 50**	**< 5 versus ≥ 50**
I(Bottom 30% SUE)								
(S = 2) and (S = 1)	−0.199	−0.270	0.147	**−0.323**	−0.165	−0.183	0.104	**−0.250**
	(0.11)	(0.28)	(0.10)	**(0.01)**	(0.05)	(0.02)	(0.16)	**(0.01)**
(S = 3) and (S = 2)	−0.278	0.088	0.074	**−0.355**	−0.136	0.146	0.026	**−0.196**
	(0.08)	(0.77)	(0.54)	**(0.03)**	(0.23)	(0.16)	(0.80)	**(0.10)**
(S ≥ 4) and (S = 3)	−0.069	−0.512	−0.214	0.094	−0.156	−0.217	−0.138	−0.040
	(0.66)	(0.10)	(0.08)	(0.31)	(0.17)	(0.03)	(0.20)	(0.40)
(S = 3) and (S = 1)	−0.477	−0.182	0.221	**−0.678**	−0.301	−0.037	0.130	**−0.446**
	(0.00)	(0.53)	(0.04)	**(0.00)**	(0.00)	(0.69)	(0.16)	**(0.00)**
(S ≥ 4) and (S = 2)	−0.347	−0.424	−0.140	**−0.261**	−0.292	−0.071	−0.112	**−0.236**
	(0.01)	(0.12)	(0.21)	**(0.07)**	(0.01)	(0.45)	(0.23)	**(0.05)**
(S ≥ 4) and (S = 1)	−0.546	−0.694	0.007	**−0.584**	−0.457	−0.254	−0.008	**−0.486**
	(0.00)	(0.00)	(0.94)	**(0.00)**	(0.00)	(0.00)	(0.92)	**(0.00)**
I(Top 30% SUE)								
(S = 2) and (S = 1)	0.095	−0.061	0.041	0.061	0.070	0.079	−0.008	0.097
	(0.42)	(0.80)	(0.65)	(0.34)	(0.42)	(0.30)	(0.91)	(0.20)
(S = 3) and (S = 2)	0.390	1.028	−0.004	**0.316**	0.158	0.294	0.070	0.023
	(0.01)	(0.00)	(0.98)	**(0.05)**	(0.18)	(0.00)	(0.48)	(0.44)
(S ≥ 4) and (S = 3)	−0.348	−0.767	−0.121	−0.161	0.077	−0.237	−0.230	**0.382**
	(0.04)	(0.02)	(0.30)	(0.21)	(0.53)	(0.02)	(0.02)	**(0.01)**
(S = 3) and (S = 1)	0.485	0.967	0.037	**0.377**	0.228	0.373	0.062	0.120
	(0.00)	(0.00)	(0.72)	**(0.02)**	(0.03)	(0.00)	(0.48)	(0.20)
(S ≥ 4) and (S = 2)	0.042	0.261	−0.125	0.155	0.235	0.057	−0.160	**0.405**
	(0.77)	(0.37)	(0.25)	(0.19)	(0.03)	(0.51)	(0.08)	**(0.00)**
(S ≥ 4) and (S = 1)	0.137	0.200	−0.084	**0.216**	0.305	0.136	−0.168	**0.502**
	(0.27)	(0.44)	(0.37)	**(0.08)**	(0.00)	(0.09)	(0.03)	**(0.00)**

(continued on next page)

TABLE 7 (continued)

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*, **, *** Indicate statistical significance of coefficient estimates in Panels A and C, at the 10 percent, 5 percent, and 1 percent levels, respectively.

This table presents regression results for several robustness checks and variations on the primary model, Equation (6). The models include control variables defined below and fixed effects for industry, as defined by two-digit SIC code. The dependent variable is abnormal trade imbalance, summed over trading days –5 through 5. If a surprise is the first in a series of same-type (top or bottom 30 percent) surprises, then $S = 1$. If it is the second, then $S = 2$ and so on. The sample contains quarterly earnings surprises for NYSE firms from 1993 through 2002. "Difference <5 versus ≥50" displays results for the difference between coefficient estimates for trades of <$5,000 and trades of ≥$50,000, estimated using a stacked regression, for the subset of observations with stock price less than or equal to $50. For each column the sample is restricted to firm-days on which stock price is low enough so that the given trade size is possible with a round lot of 100 shares. For Panels A and C, t-statistics are in parentheses. Standard errors are robust to heteroscedasticity and arbitrary within-firm correlation. The specific variations are described below. Panels B and D display the differences between coefficient estimates reported in Panels A and C, respectively. p-values for the differences, based on F-tests, are in parentheses. Two-tailed tests are used for columns 1–3 and 5–7, and one-tailed tests are used for columns 4 and 8. Significant values in Panels B and D, columns 4 and 8, at the 10 percent level, are shown in bold.

Panel A presents results for two alternative earnings surprise measures. The column "Analyst-Based Surprise" presents results where the earnings surprise and corresponding surprises series (i.e., top/bottom 30%, $S = 1$, $S = 2$, etc.) are determined based on analyst-forecasts. For columns 1–4, a "negative surprise" ("positive surprise") is defined as a surprise in the bottom 30 percent (top 30 percent) of analyst-based surprises. The column "Positive/Negative SUE" presents results for the main standardized unexpected earnings (SUE) measure, where "S" is defined using all positive (negative) surprises rather than only those in the top (bottom) 30 percent.

Panel C presents results for alternative trade imbalance measures, in which the dependent variable is abnormal trade imbalance with additional adjustments. Column "a" presents results where the dependent variable is abnormal trade imbalance with additional adjustments for response to prior day, week, month, and quarter returns. Column "b" presents results where the dependent variable is abnormal trade imbalance with adjustments for calendar-time effects, prior returns, and prior trade-imbalances.

Variable Definitions:
SUE = earnings surprise value for Panel A, columns 4–6 and Panel C and analyst-based earnings surprise for Panel A, columns 1–3;
I(Fiscal Quarter 4) = an indicator that takes the value 1 if the given quarter is the final quarter of the firm's fiscal year;
ln(Size) = natural log of market value;
Institutional Ownership = percent of shares owned by 13F reporting institutions;
Transient Institution Ownership = percent of shares owned by institutions that are classified as "transient" following Bushee (1998);
Number of Analysts and ln(Number of Analysts) = the number of analysts reporting forecasts of quarterly earnings for the firm in I/B/E/S and ln(Number of Analysts), respectively.

significantly negative coefficient for I(Bottom 30 percent Surprise) $*$ I($S = 1$), there is no evidence of large traders reacting more strongly as either the positive or negative series progresses. Coefficient differences, presented in Panel B, are insignificant. In untabulated analyses, I examine trade reactions for each earnings surprise decile, analogously to Table 4, and find no evidence of large traders reacting more strongly for series.

While small and medium trading is not the focus of this test, I document results for completeness. Focusing on column 4, the difference between small and large trades, there is weaker evidence for an "increasing reaction" for small traders relative to large traders than in the main tests. For surprises in the bottom 30 percent, two of the six relevant differences are significant, and for surprises in the top 30 percent, three of the six differences are significant. Given that the analyst-based expectations model is less likely to capture small trader expectations (Bhattacharya 2001; Battalio and Mendenhall 2005), it is not surprising that I find less evidence of an increasingly strong relative reaction.

Series of Positive and Negative SUE

The primary analysis focuses on strong positive (top 30 percent of SUE) and strong negative (bottom 30 percent) surprises to eliminate the possible confounding effects of "expected" small surprises, which is important when comparing small and large traders who may view small surprises differently. However, prior literature on strings focuses on positive earnings changes (e.g., Barth et al. 1999; Myers et al. 2007; Ke et al. 2003; Ke and Petroni 2004). Thus, this subsection examines trading for series of positive ($SUE > 0$) and negative ($SUE < 0$) earnings announcements. For clarity, I label "S" values defined using $SUE > 0$ and $SUE < 0$ as $S_{+/-}$.

Using this definition, all surprises have a value of at least 1 for $S_{+/-}$. In order to avoid collinearity with the included industry fixed effects, I exclude the term for I(Negative Surprise) $*$ I($S_{+/-} = 1$). The results are reported in Table 7, Panels A and B. There is no evidence of a more negative reaction from small traders, relative to large traders, as a negative series progresses. However, for positive series the difference between small and large trades becomes significantly more positive as the series continues. The difference is monotonically increasing, with statistically significant differences for $S_{+/-} \geq 4$ versus $S_{+/-} = 1$, 2, and 3, and for $S_{+/-} = 3$ versus $S_{+/-} = 1$. Thus, results are qualitatively similar for "positive" series as they are for "strong positive" series, but there is no evidence of increasing reactions for purely "negative" series as there is for "strong negative" series. Given that the prior literature has focused on strings of positive earnings changes, this analysis indicates that the primary results in the paper extend to a definition of series that is closer to that used in prior literature, strengthening the link between the small/large trade results documented in this paper and the price/return results from prior literature.[15]

Alternative Trade Imbalance Measures

This subsection presents two robustness tests using alternative trade-imbalance measures that control directly for prior returns. On average, a firm with prior positive (negative) earnings surprises will have had prior positive (negative) returns. It is possible that a naïve reaction to past returns would affect trading during a series.

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[15] I estimate a model including the interaction terms between $S_{+/-}$ indicators and SUE decile indicators. The results show slightly more evidence of small traders reacting more negatively, relative to large traders, for surprises that are later in a string of negative surprises, with seven of 18 difference-of-difference coefficients for deciles 0, 1, and 2 being significantly negative, and 1 being significantly positive. The results also show slightly weaker evidence for positive series, with four of 18 difference-of-difference coefficients being significantly positive. However, the coefficient patterns are consistent with small traders reacting more strongly, relative to large traders, for surprises later in a positive or negative string, controlling for earnings surprise decile.

For the first measure, I regress abnormal trade imbalance on cumulative abnormal returns over the prior day, week, month, and quarter: trading day intervals [−1], [−5, −2], [−20, −6] and [−60, −21]. Return-adjusted abnormal trade imbalance is defined as the residual from this regression.[16] Table 7, Panel C reports results from estimating Equation (6) using the return-adjusted trade-imbalance measure. Focusing on column 4, the difference between small and large traders, we can see that small traders react increasingly negatively to a series of bottom 30 percent surprises, relative to large traders. Five of the six relevant difference-of-differences in Panel D, column 4 are statistically significant. While the results are not as strong for positive series, they are similar to the main results reported in Table 3, with statistically significant increases in the difference between small and large trader reactions from $S = 1$ and 2, to $S = 3$ and $S = 1$, to $S \geq 4$.

Based on Chordia et al. (2002) and Frieder (2008), I define a second measure that controls for calendar-effects, serial correlation in the trade imbalance variable, and dependence of trade imbalance on prior returns.[17] The right half of Table 7, Panels C and D reports regression results and tests for differences in coefficients, respectively, using this alternative trade imbalance measure. The results are qualitatively similar to the primary results, and similar to the results for return-adjusted abnormal trade imbalance. In untabulated analyses, I replicate Table 4 using both the return-adjusted trade measure and the trade measure adjusted for calendar-effects. Results are similar to those presented in Table 4 and slightly stronger, using these alternative abnormal trade measures.

The results presented in Table 7, Panels C and D further support the results presented in Section IV, i.e., that small traders react more strongly to a series of similar earnings surprises, relative to large traders. While the primary specification in Equation (6) controls for important factors that may drive trade, the results in Panels C and D show that results are robust to controlling explicitly for prior returns, calendar-time effects, and prior trade imbalances.[18] If anything, results for negative series and $S \geq 4$ are more strongly consistent with H1 once I explicitly control for prior returns.

VI. CONCLUSION

This paper documents investor reactions to the time-series of earnings surprises. The results document that smaller traders react generally more negatively relative to large traders as a series of strongly negative surprises progresses, and more positively for the first three positive surprises in a series of positive surprises. These results are robust to many method variations and controls. The paper also documents that the announcement-period market reaction to earnings surprises is significantly related to small and medium traders' concurrent trading. Together, these results are consistent with the price/returns patterns documented in Barth et al. (1999), Myers et al. (2007), and Lev et al. (2008) reflecting stronger "earnings momentum" trading on the part of small and medium traders than on the part of large traders, at least for the first three quarters in a series. Given differences in small and large traders' sophistication, but the lack of systematic differences in their risk preferences, this suggests that the price/return patterns early in a series may be due more to unsophisticated investors than to changes in firm risk.

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[16] The untabulated results indicate that large traders are "momentum" traders, buying more strongly when prior returns have been higher, while small and medium traders appear to be "contrarians," consistent with prior evidence (Grinblatt et al. 1995; Kaniel et al. 2008).

[17] Shanthikumar (2004) describes details of the procedure and insights for calendar-effects in returns.

[18] I conduct a variety of additional robustness tests. The primary results are robust to the inclusion of fixed-effects for each security, to control for any potential variation in trade imbalance across firms. The results are robust to the inclusion of fixed effects for each of the 40 sample quarters as well as the inclusion of fixed effects for each of 100 groups defined by size decile and book-to-market decile.

The small and large investor reaction to earnings series documented here is also relevant to behavioral finance models that predict that investors will react more strongly as similar information is released (Barberis et al. 1998; Daniel et al. 1998; Mullainathan 2002). These models show that increasing investor reactions can explain both short-run momentum and long-run mean-reversal in returns. There is disagreement on whether investors display a preference for consistency in laboratory settings (Bloomfield and Hales 2002; Asparouhova et al. 2009). This paper documents that small traders display a relative preference for consistency in market trading.

The trading evidence provided in this paper shows that, relative to large traders, small traders are more sensitive to earnings information later in a series of similar surprises, particularly for negative series and for the first three quarters of positive series. By stepping beyond aggregate market response measures such as price and returns, these results increase our understanding of the market's reaction to the time-series pattern of earnings information.

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Audit Quality and Auditor Reputation: Evidence from Japan

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ABSTRACT: We study events surrounding ChuoAoyama's failed audit of Kanebo, a large Japanese cosmetics company whose management engaged in a massive accounting fraud. ChuoAoyama was PwC's Japanese affiliate and one of Japan's largest audit firms. In May 2006, the Japanese Financial Services Agency (FSA) suspended ChuoAoyama for two months for its role in the Kanebo fraud. This unprecedented action followed a series of events that seriously damaged ChuoAoyama's reputation. We use these events to provide evidence on the importance of auditors' reputation for quality in a setting where litigation plays essentially no role. Around one quarter of ChuoAoyama's clients defected from the firm after its suspension, consistent with the importance of reputation. Larger firms and those with greater growth options were more likely to leave, also consistent with the reputation argument.

Keywords: *audit quality; auditor reputation; Japan.*

Data Availability: *All data in the paper are publicly available from the sources described in the text.*

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I. INTRODUCTION

High-quality external auditing is a central component of well-functioning capital markets. The accounting literature focuses on two principal forces that motivate auditors to deliver quality—a litigation/insurance incentive and a reputation incentive. Under the first motive, if auditors are legally liable for audit failures, then they have an incentive to deliver high-quality to avoid the costs of litigation. The insurance role arises because investors prefer larger audit firms as these firms can better meet investors' legal claims, thus providing investors' financial recourse against poor audit quality. Under the second motive, auditors have reputational incentives to avoid audit failures because audit quality is valuable to clients and so priced in the market for audit services. Under this view, clients defect to other auditors when an audit firm's reputation for quality deteriorates.

Empirically, it is difficult to separate the effects of litigation/insurance from those of reputation in markets such as the U.S. because the largest audit firms have both the largest litigation incentives and the strongest reputations. We study recent events in Japan where auditors' legal liability is essentially nonexistent. Specifically, we study the case of ChuoAoyama, PricewaterhouseCoopers' (PwC) former affiliate in Japan, which was implicated in a major accounting fraud at Kanebo, a large Japanese cosmetics company. This case provides a good setting for examining the importance of auditor reputation absent the confounding effects of litigation.

In May 2006, regulators in Japan took the unprecedented step of suspending ChuoAoyama's operations for two months as punishment for its role in the Kanebo fraud. Also in response to these events, and at about the same time, PwC adopted a "two-firm strategy" in Japan, under which it undertook to (1) address the audit-quality problems at ChuoAoyama, which it rebranded Misuzu, and (2) establish a new, smaller "high-quality" affiliate in Japan, which it named Aarata. A select group of Japanese clients that included Sony and Toyota, as well as large multinational clients with operations in Japan, moved to Aarata. The revelation in December 2006 of serious accounting irregularities at Nikko Cordial, another prominent ChuoAoyama client, ultimately caused Misuzu to be shut down.

Firms with a reputation for credible financial reporting are likely to change auditors when their audit quality is questioned to avoid the capital market consequences of potentially unreliable financial reporting (Hennes et al. 2011). However, these benefits must be balanced against the costs of switching auditors. First, firms face search costs in identifying and hiring a new audit firm. Second, incumbent auditors develop firm-specific knowledge and expertise about the client that is costly for a new auditor to acquire (DeAngelo 1981).[1] Third, the supply of auditors is constrained in the short run, especially when many firms are looking for new auditors at the same time (Kohlbeck et al. 2008; Ramnath and Weber 2008), as is likely to be the case in our setting.

Prior research finds that local audit office effects are important in explaining auditor attributes such as client dependence (Reynolds and Francis 2000), industry expertise (Ferguson et al. 2003; Francis et al. 2005; Basioudis and Francis 2007), and audit quality in general (Francis and Yu 2009; Chaney and Philipich 2002; Nelson et al. 2008). This suggests that there is a local office effect on audit quality as well as an overall audit firm effect. If audit-quality problems are confined to a particular practice office, then investors are less likely to be concerned about clients of other offices of the audit firm. Our setting provides an unusual instance in which audit quality was perceived to be low across an entire audit firm. As explained below, PwC effectively acknowledged that it could not consistently deliver quality in its Japanese operations by dividing ChuoAoyama into two firms, one of which (Aarata) it characterized as being of higher quality.[2] The view that ChuoAoyama's problems were pervasive was reinforced by the regulator's decision to suspend its operations.

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[1] The "low balling" literature suggests that audit firms price the initial year of an audit engagement below cost to win the engagement with the expectation that the initial losses will be offset by higher fees in the future (Kanodia and Mukherji 1994). Therefore, the higher fees from an auditor change may not be immediately evident.

We find that roughly one-quarter of ChuoAoyama's clients dropped the firm when the extent of its audit-quality problems became apparent, but before it became clear that it would be wound down.[3] Firms with a greater demand for audit quality—larger firms and those with greater growth options—were more likely to leave ChuoAoyama, consistent with our argument that switches were driven by concerns about audit quality.

It would be more difficult to attribute these switches to concerns about audit quality if client firms simply followed their existing audit partners to new audit firms after the suspension was announced. To investigate this, we compare the identity of the audit partners who sign the audit report before and after auditor changes. We first show that there is no overlap in the identity of audit partners who signed audit reports before and after switches for those ChuoAoyama clients that left the firm after the suspension was announced (i.e., those that switched in May, June, and July 2006). In contrast, for those clients that switched auditors after the decision to close the firm in February 2007, there is a good deal of overlap in audit partners who signed audit reports before and after switches. These results are consistent with our interpretation that the switches in 2006 are driven by concerns about audit quality, while those in 2007 are driven by the revelation that the firm would be wound down.

We also use an event study to investigate whether ChuoAoyama's clients suffered declines in equity value as information about the firm's lower audit quality was revealed. We find a statistically significant but small negative reaction to the set of events that collectively revealed the decline in quality. However, this event study likely lacks power because of the relatively long period over which these events unfolded and the associated difficulty of isolating exactly when news about audit quality reached market participants.

Taken as a whole, our evidence supports the view that auditor quality and reputation are important in an economy where litigation does not provide auditors with incentives to deliver quality.[4] Most prior literature finds mixed support, at best, for the importance of auditor reputation as a driver of audit quality (Lennox 1999; Willenborg 1999; Khurana and Raman 2004). Consistent with our findings, Weber et al. (2008) find evidence of client switching when fraud at ComROAD AG raised questions about audit quality at KPMG in Germany, where litigation costs are relatively small. Our findings are stronger than those in Weber et al. (2008), and exploit unusual features of the Japanese setting to further strengthen the interpretation that these results are driven by reputation.

There are both similarities and differences between the events at ChuoAoyama and those surrounding the demise of Arthur Andersen in the U.S. The two sets of events are similar in that Andersen's ultimate demise, which resulted from its failed audit of Enron, was preceded by problems with its audits of Waste Management, Sunbeam, and the Baptist Foundation of Arizona.[5]

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[2] Our characterization of Aarata as the higher quality firm follows the position of PwC's CEO Samuel DiPiazza, who said, "Aarata's limited size reflected the availability of staff who met the firm's performance standards" (*Financial Times* 2007). Our interviews with former personnel of ChuoAoyama, auditors from other audit firms, company managers, academics in Japan, the senior staff of the Japan Institute of Certified Public Accountants, and the Auditing Oversight Board at the Japanese FSA confirm the view that Aarata was the higher-quality firm.

[3] Approximately one-quarter of ChuoAoyama's clients moved to other auditors soon after the suspension was announced. We provide evidence and arguments later in the paper to show that this suspension did not make it likely that ChuoAoyama would subsequently be wound down and that, as of the time of the suspension, most parties viewed the firm as being viable in the long run.

[4] Murase et al. (2010) also examine auditor switching around the time of the ChuoAoyama events. Broadly similar to our findings, these authors find that clients with larger agency costs tended to switch to auditors unaffiliated with PwC, while clients with larger switching costs tended not to change auditors.

[5] As reported by Ball (2009, 292), Andersen settled lawsuits related to these audits for $27 million in the case of Waste Management (including the SEC settlement), $110 million in the case of Sunbeam, and $217 million in the case of the Baptist Foundation of Arizona.

Most notably, Andersen's audits of Waste Management during the 1990s resulted in what was, at the time, an unprecedented settlement with the SEC, which included a $7 million fine and a permanent injunction against further violations. It was this settlement that set the stage for the firm's subsequent criminal indictment in the Enron case.

Prior to the revelation of the fraud at Kanebo, ChuoAoyama had been implicated in accounting frauds at Yaohan Japan, Yamaichi Securities, and Ashikaga Bank. But it was its involvement in the fraud at Kanebo, like Andersen's involvement in the fraud at Waste Management, that resulted in the unprecedented action by regulators. As was the case with Andersen at Waste Management, the regulatory consequences of ChuoAoyama's involvement in the Kanebo fraud were unprecedented but did not threaten the existence of the firm. Consequently, the clients that switched from ChuoAoyama after the suspension was announced, unlike those that left Anderson following that firm's indictment, did not leave because of the imminent closure of the firm. Thus, it is more appropriate to analogize the events in the Kanebo/ChuoAoyama case to those at Waste Management/Andersen than to those of the Enron case. As indicated above, it was the subsequent revelation of accounting irregularities at Nikko Cordial that resulted in the closure of ChuoAoyama.

The more significant difference between the two sets of events, however, is the absence of litigation in the Japanese setting. In the U.S., client losses attributable to Andersen's association with Waste Management could be due to reputational effects, legal consequences, or some combination thereof (as its legal costs mounted, the resources available to meet future legal claims at Andersen were diminished, reducing the insurance value of its audit services).[6] Thus, the Japanese setting provides a substantially cleaner test of the market response to an auditor's loss of reputation.

Our setting is also different from that of Weber et al. (2008) in at least two respects. First, at the time of the events described here, litigation against auditors was largely unavailable to investors in the Japanese market. In contrast, auditors could be sued in Germany although damages were capped at relatively low amounts. Second, in our setting, government regulators clearly signaled the systemic nature of the audit-quality problems at ChuoAoyama by announcing a firm-wide suspension; the severity of the regulator's actions were, at the time, a shock to the Japanese financial community, which had been expecting a modest fine. In contrast, the revelations of audit-quality problems at ComROAD AG in Germany were revealed gradually through the press.

There a number of reasons that the suspension of ChuoAoyama in May 2006 did not raise questions about the firm's survival. First, there was no perception that the suspension was a "death penalty" for ChuoAoyama. The suspension was deliberately timed to begin on July 1, after the conclusion of the annual reporting cycle including annual shareholder meetings in June. Second, the Japanese Institute of Certified Public Accountants discouraged poaching by competitor audit firms (Kyodo News International 2006), an action that would be unlikely if the firm's survival was in jeopardy. Third, there is no evidence that partners were leaving ChuoAoyama as clients defected between May and July 2006. If partners had feared that ChuoAoyama would shut down, we would expect to see departure of audit partners due to career concerns. Fourth, the suspension did not prevent clients from returning to ChuoAoyama when the suspension ended, and most did return. Fifth, the events we describe show that the actual end of Misuzu, the renamed ChuoAoyama, came after the revelation in December 2006 of serious accounting irregularities at another ChuoAoyama client, Nikko Cordial. Finally, our evidence shows that after the first set of client defections from May to July 2006, there was no meaningful client switching until March 2007, when an orderly transition of Misuzu clients to other audit firms began.

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[6] Ball (2009) reports that Andersen began to lose clients in 2000 and the first part of 2001, before the problems at Enron came to light.

The next section provides more details about the Kanebo fraud, the role of ChuoAoyama, a discussion of prior literature, and empirical predictions. Section III describes our sample and provides empirical evidence. Section IV offers a summary and conclusions.

II. THE DOWNFALL OF CHUOAOYAMA AND EMPIRICAL PREDICTIONS

The Kanebo Fraud and the Downfall of ChuoAoyama

In 2004 Kanebo, a longtime ChuoAoyama client, revealed a massive accounting fraud and began an internal investigation that resulted in Kanebo dropping ChuoAoyama as its auditor. Appendix A lists key events in the case. In April 2005, Kanebo revealed that the accounting fraud amounted to an overstatement of income of around 200 billion yen ($1.9 billion) for the fiscal years 1999–2003. After correction, the restated financial statements showed a cumulative loss during that time period of 207 billion yen.[7] While Kanebo was the largest corporate fraud in Japanese history (Hosono 2008), ChuoAoyama had previously been implicated in other accounting frauds, including Yaohan Japan Corp. (1997), Yamaichi Securities (1999), and Ashikaga Bank (2000). In July 2005, three former Kanebo executives were arrested and government prosecutors searched ChuoAoyama's offices. Over the next few months, government prosecutors indicted three ChuoAoyama auditors and ChuoAoyama's board resigned.

PwC took a number of steps to preserve its reputation in the wake of these events. First, late in 2005, Samuel DiPiazza, worldwide head of PwC, traveled to Japan to meet with Japanese regulators, ChuoAoyama executives, and management of important clients, largely to assure them of PwC's commitment to correcting the problems at ChuoAoyama. Second, early in 2006 PwC sent high-level audit personnel from the U.S. and U.K. to take corrective action at ChuoAoyama. In addition to making operational and training improvements at ChuoAoyama, PwC considered forming a new, smaller audit firm that would operate independently of ChuoAoyama. PwC identified four of the firm's Japanese staff, all former Aoyama (Price Waterhouse) partners, as candidates to head the new firm.[8] However, the Japanese leadership of ChuoAoyama resisted this change, arguing that such a drastic step was unnecessary.

During the trial of the Kanebo executives, the accused ChuoAoyama auditors admitted their complicity in the fraud, a revelation that came as a surprise to others in the firm. In late March 2006, the former ChuoAoyama auditors themselves went on trial and pled guilty to the charges. These events made it more difficult for ChuoAoyama leadership to argue that their initial reforms were sufficient. PwC then decided to proceed with its "two-firm strategy" of forming a new, smaller firm, to be known as PwC Aarata, and rebranding the rest of ChuoAoyama as Misuzu Audit Corp. When PwC announced this strategy in May 2006, it indicated that Aarata would audit PwC's international clients in Japan and, in return, PwC would audit the international operations of Aarata's Japanese clients. Most of ChuoAoyama's clients and staff went to Misuzu, which was essentially a reconstituted ChuoAoyama, while a smaller group of clients and, arguably, those of most strategic importance to PwC internationally (Sony and Toyota being the most prominent examples) went to Aarata.[9] Consistent with its higher quality status, Aarata was not subject to sanctions and was allowed to conduct business during the ChuoAoyama suspension period.

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[7] Japanese GAAP first required consolidated financial reporting in the late 1990s. The Kanebo fraud involved the failure to include 14 poorly performing subsidiaries in its consolidated financial statements (Hosono 2008; Hawkins et al. 2010).

[8] ChuoAoyama was formed in April 2000 from a merger between Chuo Audit Corp. (the Coopers & Lybrand affiliate) and Aoyama (the Price Waterhouse affiliate). Chuo was responsible for the three audit failures mentioned in the text. At the time of the merger, Chuo, which contributed 310 partners to the merged firm, was much larger than Aoyama, which contributed 37 partners.

[9] We provide evidence below that most former Aoyama clients still audited by ChuoAoyama in fiscal year 2006 went to Aarata, but only a small fraction of former Chuo clients went to this firm. This is consistent with the suggestion that Aarata was a reconstituted version of Aoyama, the original Price Waterhouse affiliate.

On May 10, 2006, the Japanese Financial Services Agency (FSA) ordered a two-month suspension of the audit operations of ChuoAoyama beginning July 1, 2006.[10] With some exceptions, the rule effectively forced ChuoAoyama to suspend business for two months. ChuoAoyama's clients took one of three actions as a result. First, some firms appointed an interim auditor for the period of the suspension and returned to ChuoAoyama when it resumed business as Misuzu. Second, other firms returned to Misuzu after the suspension without appointing an interim auditor. Third, some firms chose a different auditor and did not return to Misuzu, including approximately 50 firms that switched to Aarata. While companies in Japan are required to have an external auditor at all times, the FSA stated following the suspension order that "realistically speaking we are not sure whether all companies will be able to name a temporary auditor" (*Financial Times* 2006), referring to constraints on the availability of alternative auditors. This suggests that the FSA did not enforce the rule and there existed some level of regulatory forbearance on this issue.[11]

In December 2006, allegations of serious accounting irregularities at Nikko Cordial, another ChuoAoyama client, came to light. To preempt further regulatory action and reputational damage, PwC announced on February 20, 2007 that they would terminate Misuzu. PwC proposed to transfer all staff and clients to other audit firms after fiscal 2006 audits were completed in the spring of 2007.[12]

Previous Literature and Empirical Predictions

Previous literature provides two types of evidence on auditor reputation. Both lines of research rely on the premise that when reputation is important in the audit market, observable declines in audit firm quality will lead to adverse consequences for its clients. One line of research examines auditor switching around events that signal a decline in an audit firm's quality. Lennox (1999) analyzes audit failures in the U.K. from 1987 to 1994 and finds that larger auditors are more likely to be sued, consistent with the liability argument, but that clients generally do not drop auditors following audit failures as the reputation argument would predict. Shu (2000) finds that, consistent with the litigation argument, auditor resignations reflect increases in client litigation risk as well as changes in audit firm characteristics. Also consistent with the litigation argument, she finds that clients tend to move to smaller audit firms after a large auditor resigns, and that there is a significant negative stock price reaction to these events.

Barton (2005) examines auditor switches after the market learned about the scope of Andersen's audit failure at Enron. However, the events at Enron and the associated demise of Andersen occurred over a short period of time, making it difficult to distinguish whether the auditor switches reflected primarily reputation concerns versus being forced by the Andersen closure. Barton (2005) finds that 95 percent of the switches away from Andersen occurred after Andersen was indicted in March 2002.

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[10] In the only previous business suspension order against an audit firm in Japan, Mizuho Audit Corporation, a smaller audit firm, was suspended for one year on October 15, 2002.

[11] Under the Japanese Financial Instruments and Exchange Act, an annual securities report must be submitted within three months of the end of that fiscal year. ChuoAoyama's suspension was timed to start from July 1 as most Japanese companies end the fiscal year on March 31 and complete their annual reports and hold meetings by the end of June. Under the Companies Act in Japan, a company is required to have an auditor at all times. Thus, ChuoAoyama's clients needed to find interim auditors during the two months of the suspension period.

[12] There is some ambiguity about who initiated the closure of Misuzu. Some articles, including those that cite Samuel Di Piazza, then CEO of PwC, characterize this as a decision taken by PwC internationally (*Financial Times* 2007). Other sources indicate that the local management of Misuzu made the decision in order to preempt its loss of clients as a result of the cumulative effect of the accounting scandals (see *Asahi Shimbun* 2007; *Japan Times* 2007). These actions enabled the firm to avoid "losing face" by having to close involuntarily and lay off staff. Thus, soon after the new fraud came to light, Misuzu cooperated with Tohmatsu, ShinNihon, and AZSA (the remaining Big audit firms) to transfer its audit personnel and clients to those other firms. There is little doubt, however, about the basic cause and effect—the revelation of the accounting fraud at Nikko Cordial quickly resulted in the demise of Misuzu.

A second line of research examines the stock price reaction to events that change market perceptions of the quality of services provided by a given audit firm. Menon and Williams (1994) and Baber et al. (1995) examine the reaction of client stock prices to the bankruptcy of Laventhol and Horwath, at the time the seventh largest audit firm in the U.S. Both studies report a significant negative reaction to the announcement, consistent with both the insurance and reputational roles for auditors. Chaney and Philipich (2002) examine the stock price reaction for clients of Andersen when it revealed document shredding related to the Enron audit. They find a significantly negative reaction, which they attribute to Andersen's loss of reputation, although Nelson et al. (2008) contest this interpretation because of confounding news on the event dates. Cahan et al. (2009) investigate the stock price reaction to Enron-related events for the non-U.S. clients of Andersen and find evidence of significantly negative reactions, which supports the importance of reputation. Krishnamurthy et al. (2006) also provide evidence that Andersen clients suffered negative market returns around the time of Enron-related events and relate these returns to cross-sectional measures of audit quality.

To more clearly distinguish the insurance and reputational explanations, Weber et al. (2008) examine an audit failure in Germany, where auditors' legal liability is limited, reducing the viability of the insurance rationale. Consistent with the reputation argument, they find that the stock prices of KPMG clients declined at the time of events that revealed KPMG's involvement in an audit failure at ComROAD. These authors also find that an unusually large number of clients dropped KPMG in 2002, the year of the ComROAD scandal.

The events at ChuoAoyama provide a powerful setting for assessing the importance of auditor reputation, allowing us to extend the findings of Weber et al. (2008). First, litigation against auditors is virtually nonexistent in Japan. This means that there is effectively no insurance role for auditing in Japan.[13] Second, the FSA's decision to suspend ChuoAoyama was unexpected and largely unprecedented.[14] Third, these events unfolded over a relatively extended period, from the first indication of accounting problems at Kanebo in Spring 2004 to early 2007, when Misuzu ceased operations. The separation in time between May 2006, when the FSA announced its suspension and PwC decided to split the firm, and its eventual demise in February 2007 allows us to clearly identify the effects of auditor reputation and separate them from the effects of the firm's closure. Fourth, we have direct evidence that reputation played an important role in these events—PwC intervened quickly and forcefully when it became clear that ChuoAoyama's problems were going to attract the attention of investors and regulators in a significant way. It seems clear that the management of PwC was prepared to sacrifice a large part of its Japanese business to preserve its reputation.[15]

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[13] This is clearly true for Japanese firms that are not listed outside Japan but less true for Japanese firms cross-listed in the U.S. and subject to U.S. securities laws. Litigation in Japan, including securities litigation, is much less prevalent than in Western countries although this is gradually changing (Ginsburg and Hoetker 2006). In spite of an increase in litigation rates since around 1990, expected litigation costs remain lower in Japan than in the U.S. West (2001) provides evidence that the number of shareholder derivative lawsuits in Japan have increased but that settlements are unusual and stockholders lose most of these cases.

[14] Consistent with previous cases, regulators had announced that they would impose some type of sanction against ChuoAoyama. However, most previous sanctions had been relatively inconsequential.

[15] Consider the following quote from Samuel DiPiazza, then CEO of PwC: "In PwC we're not perfect, but I think we have sent the message. In Japan we shut that firm down. We gave up a major amount of businesses, but we did it because we felt that the most important [asset] was our quality in that market to be at the highest level. We feel that we have that today: even if it's smaller, it's a better quality" (*Czech Business Monthly* 2007).

The suspension of ChuoAoyama and PwC's decision to split the firm into two parts signaled to clients and investors that Aarata was of higher quality than ChuoAoyama. Such an unambiguous, firm-wide audit quality signal has not been available in prior research settings. Moreover, by suspending ChuoAoyama, Japanese regulators (perhaps inadvertently) reduced switching costs for its clients. Because firms in Japan are required to have an external auditor at all times, firms were forced to find another audit firm for the suspension period. While client firms could return to ChuoAoyama (Misuzu) when it resumed business, the appointment of interim auditors made it easier for ChuoAoyama's clients to review their contracts, given that they had already incurred some of the switching costs.

Finally, data available to us in Japan permits more detailed analysis of the auditor-switching decision than in Weber et al. (2008). First, Weber et al. (2008) cannot distinguish client switching due to supply-side effects, such as KPMG reducing its exposure to risky clients, from demand-side effects, in which clients switch to maintain a reputation for financial reporting quality. Weber et al. (2008) find that smaller firms and those that recently completed IPOs are more likely to switch auditors, but both of these characteristics are also consistent with KPMG's attempt to avoid riskier clients after the scandal. In contrast, the spin-off of Aarata meant that ChuoAoyama was already facing the loss of clients and so unlikely to reject other clients.[16] Second, we use the date of auditor changes to isolate changes that occurred after the suspension in May 2006 but before the Nikko Cordial fraud came to light in December of that year. Thus, we can more confidently associate client switching with the reputational impact of the events at ChuoAoyama. Finally, in Japan audit partners sign audit reports in their own names as well as those of their audit firms. This allows us to examine the extent to which clients follow their audit teams from one audit firm to another, which is an alternative explanation for switching that is less consistent with improving audit quality. While the audit partner names are also disclosed in Germany, Weber et al. (2008) do not use that information.

To assess the extent to which evidence from these events supports the importance of auditor reputation, we first analyze auditor changes during the period in which these events unfold. If auditor reputation is important, then we expect client firms to switch auditors when the incumbents are revealed to be of low quality. Second, we analyze the market reaction to the events that led to the FSA's suspension of ChuoAoyama. If reputation is important and we have identified these events correctly, then the costs of lower audit quality should be observable as declines in the stock prices of ChuoAoyama's clients.

III. EMPIRICAL ANALYSIS

Sample and Descriptive Statistics

We sample all firms listed on the First and Second Sections of the Tokyo Stock Exchange (TSE) in February 2008, a total of 2,199 firms. To mitigate possible survivor bias, we add firms that delisted during or after January 2004, increasing the sample by about 200 firms. We identify sample firms' auditors from Japanese securities filings (*yukoshoken hukoksho*) from fiscal 2001 through fiscal 2007.[17]

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[16] Our understanding is that when clients started leaving, ChuoAoyama/Misuzu pursued a market share maximization approach that was also consistent with the desire to retain existing partners and staff.

[17] In Japan, most companies end the fiscal year on March 31. We use the Compustat convention to label firm/years; for example, we refer to the fiscal year ended March 31, 2007 as fiscal 2006 or F2006.

Analysis of Market Share

We report the number of publicly listed firms audited by Big and non-Big auditors in Panel A of Table 1;[18] from fiscal years 2001 through 2007 the annual average is about 2,150 firms.[19] Panel A statistics show that there is a high degree of audit market concentration in Japan. The Big auditors cover between 81 and 84 percent of the market in our sample period when measured by the number of clients and 92 to 95 percent when weighted by client firm size.

For each of the Big audit firms, Panel B of Table 1 reports the time-series distribution of the number of client firms while Panel C provides market share based on the client firm market capitalization. Panel B shows that in F2001 and F2002 four Big audit firms dominate the market. There is a shift in F2003, when Asahi (Andersen) combined with AZSA and retained the AZSA name. The numbers for F2004 are similar.

The problems at Kanebo and ChuoAoyama came to light in 2004. The problems became more serious in the middle of 2005, when government prosecutors arrested executives from Kanebo and auditors from ChuoAoyama and searched ChuoAoyama's offices. This means that any switching away from ChuoAoyama could have begun in F2005 (ending March 30, 2006). However, the number of ChuoAoyama clients stays essentially unchanged in F2005, as seen in Panel B where ChuoAoyama audited 471 clients in F2004 and 469 clients in F2005.

The suspension of ChuoAoyama and the split into Misuzu and Aarata was announced in May 2006, allowing companies to decide whether to switch auditors for F2006 before their annual stockholder meetings in June 2006. Panel B of Table 1 shows that in F2006 the total of ChuoAoyama (7 clients), Misuzu (303 clients) and Aarata (52 clients) is 107 less than the 469 clients audited by ChuoAoyama in F2005, implying that a significant number of firms moved away from ChuoAoyama as these events unfolded. The other Big audit firms were the primary beneficiaries. Because the decision to close Misuzu did not occur until February 2007, it seems reasonable to interpret the F2006 audit changes as a response to concerns about audit quality rather than changes forced by the termination of Misuzu.

The F2006 to F2007 loss of clients for Misuzu are more likely forced by its closure and, therefore, more difficult to attribute to reputational concerns. The closure was announced in late February 2007, and all but 15 of the 303 Misuzu clients in F2006 had left by F2007.[20] Misuzu terminated operations following completion of audits for F2007. To summarize, these data show that the number of publicly listed ChuoAoyama clients was essentially unchanged in F2005 but declined significantly from 469 in F2005 to 362 (i.e., 7 in ChuoAoyama, 303 in Misuzu, and 52 in Aarata) in F2006 and even further to 75 (15 in Misuzu and 60 in Aarata) in F2007. We attribute the

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[18] We use the terminology "Big" auditors to refer to local affiliates of the large international audit networks. These are ChuoAoyama/Misuzu/Aarata (PwC), Asahi (Arthur Andersen), AZSA (KPMG), ShinNihon (Ernst & Young), and Tohmatsu (Deloitte). Non-Big auditors are all other audit firms.

[19] We limit the sample in Table 1 to observations with non-missing market capitalization data to make the sample more comparable to that used in subsequent empirical analyses. The number of observations in later tables may be lower due to the availability of data on all the control variables.

[20] The 15 client firms remaining with Misuzu in fiscal year 2007 are firms with fiscal years that end after March 31, for which the fiscal 2007 year-end concludes in calendar 2007, so that Misuzu could complete the fiscal 2007 audit before it closed on July 31 of that year. Our subsequent Table 4, Panel B provides statistics on the eventual auditor in F2007. Of the 303 firms audited by Misuzu, only 49 firms move to Non-Big audit firms. All other clients move to a Big auditor. We are unable to find the subsequent auditor for 50 firms because we are unable to find the relevant filings.

TABLE 1

Market Share Analysis of Japanese Audit Market

Panel A: Distribution of Clients across Time

Fiscal Year	Big Auditor	Big Auditor Percent by Number of Clients	Big Auditor Percent by Size of Client	Non-Big Auditor	Non-Big Auditor Percent by Number of Clients	Non-Big Auditor Percent by Size of Client	All Audit Firms
F2001	1,565	81.2%	93.6%	363	18.8%	6.4%	1,928
F2002	1,757	82.0%	93.4%	386	18.0%	6.6%	2,143
F2003	1,820	82.1%	93.6%	396	17.9%	6.4%	2,216
F2004	1,852	82.9%	94.4%	382	17.1%	5.6%	2,234
F2005	1,871	83.8%	95.1%	361	16.2%	4.9%	2,232
F2006	1,857	83.3%	95.3%	372	16.7%	4.7%	2,229
F2007	1,723	81.2%	92.4%	399	18.8%	7.6%	2,122
Total	12,445	82.4%	94.1%	2,659	17.6%	5.9%	15,104

Panel B: Time Series Distribution of Number of Clients across Big Auditors

	Big Auditor								
	PwC-Related Firms			Other Big Audit Firms					
Fiscal Year	Chuo-Aoyama	Misuzu	Aarata	Asahi	AZSA	Shin Nihon	Tohmatsu	Non-Big Auditor	All Audit Firms
F2001	396	0	0	324	0	465	380	363	1,928
F2002	452	0	0	348	4	515	438	386	2,143
F2003	464	0	0	12	373	518	453	396	2,216
F2004	471	0	0	0	395	525	461	382	2,234
F2005	469	0	0	0	410	532	460	361	2,232
F2006	7	303	52	0	443	573	479	372	2,229
F2007	0	15	60	0	473	674	501	399	2,122

Panel C: Time-Series Distribution of Clients Weighted by Market Capitalization

	Big Auditor								
	PwC-Related Firms			Other Big Audit Firms					
Fiscal Year	Chuo-Aoyama	Misuzu	Aarata	Asahi	AZSA	Shin Nihon	Tohmatsu	Non-Big Auditor	All Audit Firms
F2001	26.68%	0.00%	0.00%	19.48%	0.00%	28.06%	19.42%	6.36%	100.0%
F2002	25.16%	0.00%	0.00%	19.22%	0.60%	28.36%	20.04%	6.62%	100.0%
F2003	27.27%	0.00%	0.00%	0.08%	20.85%	26.40%	19.03%	6.37%	100.0%
F2004	26.28%	0.00%	0.00%	0.00%	21.07%	26.98%	20.12%	5.55%	100.0%
F2005	24.16%	0.00%	0.00%	0.00%	23.95%	25.74%	21.23%	4.92%	100.0%
F2006	0.09%	11.71%	8.83%	0.00%	25.63%	26.04%	23.02%	4.68%	100.0%
F2007	0.00%	0.16%	9.33%	0.00%	29.73%	28.10%	25.13%	7.55%	100.0%

(continued on next page)

TABLE 1 (continued)

Panel D: Turnover Rate in Big and Non-Big Auditor

Fiscal Year	Big Auditor Excluding ChuoAoyama and Misuzu	Non-Big Auditor	ChuoAoyama and Misuzu
F2002	0.9%	8.0%	0.5%
F2003	2.5%	3.1%	1.3%
F2004	0.6%	8.1%	1.5%
F2005	1.2%	7.6%	1.7%
F2006	1.2%	10.7%	23.7%
F2007	1.9%	10.6%	92.5%

Panel E: Month of Switch to Final Auditor by ChuoAoyama Clients

Month Switched to Final Auditor	A: Leave ChuoAoyama, Do Not Revert to Misuzu	B: Choose Interim Auditor, Revert to Misuzu	C: No Interim Auditor, Revert to Misuzu	D: All ChuoAoyama Clients
Feb-06	5			5
Mar-06	0			0
Apr-06	5			5
May-06	38			38
Jun-06	25			25
Jul-06	82			82
Aug-06				0
Sep-06				0
Oct-06				0
Nov-06				0
Dec-06		1		1
Jan-07		0	1	1
Feb-07		3	2	5
Mar-07		5	1	6
Apr-07		28	5	33
May-07		132	66	198
Jun-07		12	5	17
Jul-07		12	14	26
Aug-07		4	4	8
Sep-07		2	1	3
Total	155	199	99	453

In Panel A Big Auditor refers to the following audit firms, with their affiliations to worldwide audit networks in parentheses: ChuoAoyama/Misuzu/Aarata (PwC), Asahi (Andersen), AZSA (KPMG), ShinNihon (Ernst & Young), and Tohmatsu (Deloitte). Non-Big Auditor refers to all other audit firms. Size of client is measured by market capitalization at the end of the fiscal year. The count of companies in all panels includes only those firms for which market capitalization data are available.
Panel D presents the auditor turnover rate for the sample firms. Change of auditor from ChuoAoyama to Misuzu or Aarata is not counted as an auditor change. Big Auditor refers to local affiliates of the large international audit networks. These are ChuoAoyama/Misuzu/Aarata (PwC), Asahi (Arthur Andersen), AZSA (KPMG), ShinNihon (Ernst & Young), and Tohmatsu (Deloitte). Non-Big Auditor refers to all other audit firms in Japan.

Panel E presents the number of ChuoAoyama clients that switched during each month in 2006 and 2007 to their final choice of auditor, which we define as the auditor for fiscal year 2007. The sample in Column A is all clients that leave ChuoAoyama and do not revert to Misuzu. Column B consists of clients that reverted to Misuzu but hired an interim auditor during the suspension. Column C is the sample of ChuoAoyama clients that reverted to Misuzu but had no interim auditor during the suspension period. Column D is all ChuoAoyama clients, i.e., the sum of columns A, B, and C.

movement away from ChuoAoyama from F2005 to F2006 to concerns about audit quality, supporting the importance of reputation effects.[21]

By F2007, when the auditor changes forced by the termination of Misuzu had largely occurred, the market share of the remaining Big auditors was 81.2 percent (Panel A of Table 1), only marginally below the level of 83.8 percent in F2005. The size-weighted shares reported in Panel C of Table 1 show that the market share of Big auditors is even more pronounced. Non-Big auditors capture their highest share of the market in F2007 at only 7.6 percent.

Panel D of Table 1 reports the auditor turnover rates in Japan for the sample time period. In all six years of the sample period auditor turnover rate for the Big auditors excluding ChuoAoyama and Misuzu in Japan is low, ranging from 0.6 percent in F2004 to 2.5 percent in F2003, with most years around 1 percent. The turnover rate for non-Big auditors is higher, ranging from 3.1 to 10.7 percent. The turnover rate for ChuoAoyama through fiscal 2005 is similar to that of the other Big audit firms. However, turnover for ChuoAoyama increases substantially to 23.7 percent in fiscal 2006, consistent with Panels B and C. This suggests there is a shift away from ChuoAoyama as its problems became more evident. Turnover rate for ChuoAoyama/Misuzu in F2007 is much higher again, at 92.5 percent, but this is due to the winding up of the firm.

The decision to terminate Misuzu followed the Nikko Cordial scandal, which was first revealed in December 2006, seven months after the suspension order against ChuoAoyama in May 2006. There is no indication that the closure of Misuzu was anticipated before the Nikko Cordial became public. Supporting this, there are no client departures from Misuzu between the end of the suspension on September 1, 2006 and December 17, 2006 when the Nikko Cordial scandal became public. Table 1, Panel E provides further information on the timing of auditor switches. We are able to find the exact auditor change announcement for 453 of the 469 ChuoAoyama clients identified in Table 1, Panel B. Table 1, Panel E shows that client switching after the suspension occurs in May, June, and July of 2006. The only departure during December occurs late in the month, after the Nikko Cordial announcement. The next major batch of departures occurs in May 2007, after Misuzu announced its impending closure in February 2007. This suggests that the earlier departures from ChuoAoyama were *not* due to fears that the firm would cease operations after the suspension announcement in May 2006.

To test whether the F2006 auditor switches away from ChuoAoyama are unusually frequent, we estimate a logit model of factors that explain auditor changes. The control variables are drawn from previous research on auditor switches and include firm size (log of total assets), growth (percentage change in total assets), leverage, change in leverage, profitability (ROA), a loss dummy, U.S. listing, *keiretsu* inclination, auditor industry expertise, earnings quality as measured by accruals, whether the firm completed an M&A transaction in the preceding two years, and industry fixed effects.[22] We provide details of data sources and variable definitions in Appendix B. The *keiretsu* inclination variable measures whether and to what extent these firms are part of the large corporate groups common in Japan (e.g., Aoki et al. 1994; Hoshi and Kashyap 2001).

We include dummy variables for whether the client is a ChuoAoyama client (*CA*), for fiscal year 2006 (*F2006*), and for the interaction of these two dummies (*CA_F2006*). The interaction variable is our primary interest because it measures the extent to which client firms switch away from ChuoAoyama in fiscal 2006, the period in which we argue that auditor reputation drives switching.[23]

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[21] To provide qualitative evidence on the reasons provided by clients for leaving ChuoAoyama, we examined the auditor change disclosure filed by these companies after the suspension announcement. The filing document known as *konin kaikeishi tou no idou* (change (transfer) of certified public accountant) is obtained from the TSE Timely Disclosure Network (TDnet). In a random sample of 40 companies that we examined, companies either offered no reason for the change or boilerplate language that the change was due to the suspension. We concluded that this disclosure is not useful for our analysis.

[22] For example, see DeFond (1992), Francis and Wilson (1988), Barton (2005), Weber et al. (2008), and Landsman et al. (2009).

We report the results of these regressions in Table 2. There are fewer observations in the multivariate tests in Table 2 than in Table 1 because we require data on all of the control variables.[24] In the Column (1) estimation we do not treat a move from ChuoAoyama to Aarata as a change, while in the Column (2) estimation we do treat these observations as changes.

The estimate on *CA_F2006* indicates that the likelihood of an auditor change is higher in fiscal 2006 when ChuoAoyama was the incumbent auditor. We report both the regular logit marginal effect and the Ai and Norton (2003) marginal effect estimate for this variable. In the first specification, the Ai and Norton marginal effect on *CA_FY2006* is 0.23 with an associated Z-statistic of 6.72 after applying the Ai and Norton (2003) and Norton et al. (2004) methodology. This implies, other variables held at their means, that a ChuoAoyama client is 23 percent more likely to switch auditors in fiscal year 2006, an effect that we attribute to reputation. The associated main effects show that ChuoAoyama client firms are, in general, less likely to switch away from ChuoAoyama than from other audit firms (marginal effect −0.01), but more likely to switch in F2006 (marginal effect of 0.01) than in other years. These effects are, however, smaller and less significant than that for the interaction variable.

The results for the interaction term become stronger in the Column (2) specification in Table 2, where we treat moves to Aarata as auditor changes, with the Ai and Norton marginal effect of 0.34 (Z-statistic = 8.52). These results support the notion that there was an unusually high likelihood of switching away from ChuoAoyama in F2006, when doubts about the quality of that firm's audit practice manifested themselves in a significant way.

Determinants of Auditor Switching for Former ChuoAoyama Clients

We next examine the determinants of the choice to switch auditors to investigate our prediction that audit quality drives these switches. To do this, we use actual dates of the auditor changes to classify the 469 publicly listed ChuoAoyama clients in fiscal 2005 into three groups based on their auditor choices for fiscal 2006. The three principal groups are:[25]

1. 99 firms that did not use an interim auditor during the suspension and reverted to ChuoAoyama/Misuzu at the end of the suspension on September 1.
2. 199 firms that used an interim auditor for the period of the suspension and reverted to ChuoAoyama/Misuzu at the end of the suspension.
3. 155 firms that appointed a new auditor before the suspension began and continued to use that auditor after the suspension ended.

We view these choices as implying different relative levels of concern for audit quality. Firms in group (1) return to ChuoAoyama (Misuzu) after the suspension without using an interim auditor.

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[23] Because the conventional logit coefficients on interaction variables do not provide a statistical test of whether the economic interaction of interest is statistically significant (Ai and Norton 2003; Norton et al. 2004; Greene 2009), we provide the estimated mean marginal effect for this variable along with the corresponding Z-statistic at the bottom of Table 2. We examined, but do not report, the graphical analyses suggested by these authors that plots the estimated interaction effects for various levels of the predicted probabilities. The interaction effect for *CA_F2006* dummy is positive and statistically significant for all relevant levels of the predicted probability. These graphs are available upon request.

[24] We exclude the changes away from ChuoAoyama/Misuzu after fiscal 2006 because these switches are likely forced by the decision to shut down Misuzu... Note that the sample size in Column (2) is lower than in Column (1) because when we exclude the 52 Aarata observations as an auditor change, one of the industry fixed effects drops out because there is no variation in auditor change in that industry.

[25] Of the remaining 16 firms, nine had previously used two auditors and dropped ChuoAoyama during the suspension, and seven firms lack the requisite data.

TABLE 2

Auditor Change Logit Regressions
Changes Away from ChuoAoyama

$$
\begin{aligned}
AuditorChange_{i,t} = {} & \alpha_0 + \alpha_1 CA_{i,t-1} + \alpha_2 F2006 + \alpha_3 CA_{i,t-1}{*}F2006 + \alpha_4 Log\ Total\ Assets_{i,t-1} \\
& + \alpha_5 \%\Delta TotalAssets_{i,t-1} + \alpha_6 Leverage_{i,t-1} + \alpha_7 \Delta Leverage_{i,t-1} \\
& + \alpha_8 ROA_{i,t-1} + \alpha_9 Loss_{i,t-1} + \alpha_{10} US\ Listing_{i,t-1} \\
& + \alpha_{11} Keiretsu\ Inclination_{i,t-1} + \alpha_{12} Industry\ Expert\ Auditor_{i,t-1} \\
& + \alpha_{13} Abs\ Disc\ Accruals_{i,t-1} + \alpha_{14} M\&A_{i,t-1} + Industry\ Fixed\ Effects + \varepsilon.
\end{aligned}
$$

	Column (1) Excludes Moves to Aarata			Column (2) Includes Moves to Aarata		
Variable	**Coeff.**	**Z-statistic**	**Marginal Effects**	**Coeff.**	**Z-statistic**	**Marginal Effects**
Constant	−2.37	−2.08**		−2.70	−6.59***	
CA	−0.52	−2.26**	−0.01	−0.55	−2.38**	−0.01
F2006	0.34	2.08**	0.01	0.33	2.01**	0.01
CA_F2006	2.91	10.18***	0.25	3.47	12.30***	0.37
Log Total Assets	−0.08	−1.54	−0.00	−0.02	−0.36	−0.00
%ΔTotalAssets	0.02	1.42	0.00	0.02	1.09	0.00
Leverage	−0.15	−0.46	0.00	−0.21	−0.65	−0.00
ΔLeverage	0.12	0.11	0.00	0.21	0.20	0.00
ROA	−0.03	−1.69*	−0.00	−0.03	−1.75*	−0.00
Loss	0.33	1.63	0.01	0.28	1.38	0.01
US Listing	0.41	1.61	0.01	0.40	1.73*	0.01
Keiretsu Inclination	−0.03	−0.59	−0.00	−0.06	−1.17	−0.00
Industry Expert Auditor	−0.64	−4.93***	−0.01	−0.55	−4.52***	−0.01
Abs Disc Accrual	2.22	4.10***	0.05	2.40	3.95***	0.05
M&A	0.23	1.68*	0.01	0.18	1.36	0.00
Industry Fixed Effects	Included			Included		
Observations	10723			10798		
Pseudo R^2	0.1362			0.1937		
Ai and Norton Marginal Effect for *CA_F2006*		6.72***	0.23		8.52***	0.34

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*, **, *** Indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-sided tests).

AuditorChange takes the value 1 when the auditor in the next fiscal year is not the same as in the current fiscal year. Data are from fiscal year 2001 to fiscal year 2006 (inclusive). Auditor change from ChuoAoyama to Misuzu is not counted as a change. Change from ChuoAoyama to Aarata is not considered a change in Column (1) and is counted as a change in Column (2). Z-statistics are based on robust standard errors. Marginal effects are computed at the means of the independent variables except for dummy variables, where it is the change in value from 0 to 1. The Ai and Norton marginal effects and Z-statistics on the interaction term are calculated following Norton et al. (2004).

All other variables are defined in Appendix B.

Variable Definitions:

$AuditorChange_t = 1$ if the audit firm changes from year t–1, 0 otherwise;

CA = 1 if ChuoAoyama is the prior year audit firm, 0 otherwise;

F2006 = 1 if fiscal year is 2006, 0 otherwise;

CA_F2006 = 1 if ChuoAoyama is the audit firm in F2005 and the year is F2006, 0 otherwise;

$\%\Delta TotalAssets_t$ = percentage change in total assets from year t–1; and

$\Delta Leverage_t$ = change in leverage from year t–1.

These firms operated for two months without an external auditor and therefore were apparently relatively less concerned about audit quality. As discussed earlier and in footnote 11, regulators in Japan did not strictly enforce the requirement that firms need to have an external auditor at all times likely due to the supply disruption caused in the audit industry due to the ChuoAoyama suspension. Firms in group (2) return to ChuoAoyama/Misuzu after using an interim auditor for the period of the suspension. The use of an interim auditor indicates that the firm has the ability to comply with regulations and market expectations relating to audit quality, thus signaling the demand for higher-audit quality than the first group of firms, although our results continue to hold if we combine groups (1) and (2).

The third category is composed of 155 firms that switch to a new auditor for fiscal 2006, including the 145 firms that switch after the suspension was announced and the ten firms that switch before that time. Considering the substantial costs to switch auditors, the firms in this group likely value audit quality most highly relative to firms in the other two groups.

We consider the auditor switches to Aarata as changes that enhanced audit quality for a few reasons.[26] First, its key management and most staff came from Aoyama, which was seen to have higher-audit quality than Chuo, a judgment that interviewees repeatedly made to us. Second, its clients came disproportionately from Aoyama. Of the 52 former ChuoAoyama clients that shifted to Aarata at its inception on July 1, 2006, 14 had been clients of Aoyama (the Price Waterhouse affiliate) prior to the April 2000 merger of Chuo and Aoyama, 11 were previously clients of Chuo (the Coopers & Lybrand affiliate), while 18 were previously clients of other firms (data are unavailable for the remaining nine firms). Aarata took a much larger fraction of former Aoyama clients than it did former Chuo clients, consistent with information we gathered in our interviews that Aarata was staffed principally by former Aoyama personnel.[27] Because Chuo was substantially larger than Aoyama, the numbers above imply that the majority of former Aoyama clients still with ChuoAoyama in fiscal 2005 (14 of 23 firms, or 60.9 percent) moved to Aarata. Only a small fraction (11 of 307 firms, or 3.6 percent) of former Chuo clients still with ChuoAoyama in fiscal 2005 moved to Aarata.

To investigate the auditor choice more formally, we fit an ordered logit model of these firms' auditor decisions for fiscal 2006. Using the groups described above, the dependent variable *Switching* is set to 0 for firms that went back to ChuoAoyama/Misuzu without using an interim auditor, 1 for firms that went back to ChuoAoyama/Misuzu after using an interim auditor, and 2 for firms that switched away from ChuoAoyama/Misuzu. The signal of the importance of audit quality conveyed by the above choices makes the dependent variable *Switching* an ordinal ranking of audit quality. Table 3 presents the results of this estimation. In the first estimation in Column (1) of Table 3 we drop all Aarata clients and in the second estimation in Column (2) we set the dependent variable to 2 for firms that moved to Aarata (a higher-quality audit alternative).

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[26] In a May 10, 2006 press release PwC announced the formation of a new firm, Aarata, that "will adopt international best practices . . . will meet high standards of audit quality . . . [and have a] high level of oversight by PwC." Further, DiPiazza, then PwC's CEO, stated that Aarata's limited size reflected the availability of staff who met the firm's performance standards. Finally, when announcing Misuzu's closure on February 20, 2007, DiPiazza stated: "We would have hoped the Japanese profession evolved to a higher level of quality over the years. It did not. Cultural differences cannot be used as an excuse for lower quality" (*Financial Times* 2007). See also footnotes 2 and 13.

[27] There are at least three non-mutually exclusive explanations for the way clients switched to Aarata: (1) PwC encouraged some firms to move to Aarata to maintain the relationship given the size and visibility of these clients (e.g., Sony, Toyota, non-Japanese multinationals such as Unilever); (2) firms that were originally clients of Aoyama followed their external audit team to Aarata; (3) firms for which audit quality is especially important, perhaps because of their size/visibility or links overseas (foreign sales, investors, listings, or some combination thereof), made the switch due to audit-quality concerns. These explanations are all consistent with these firms valuing audit quality relatively highly.

TABLE 3

Ordered Logit Estimation of the Likelihood that ChuoAoyama Clients during the Period of Suspension: (1) Do Not Report an Interim Auditor, (2) Switch to an Interim Auditor and Then Revert to Misuzu, or (3) Switch to a Final Audit Firm (do not revert back to Misuzu after suspension ends)

	(1)	No Aarata		(2)	Includes Aarata	
Variables	**Coeff.**	**Z-statistic**	**Odds Change**	**Coeff.**	**Z-statistic**	**Odds Change**
Log Total Assets	0.29	2.00**	1.55	0.32	2.53**	1.69
Std Dev Returns	0.17	0.82	1.13	−0.01	−0.04	0.99
Number of Segments	0.15	1.54	1.22	0.12	1.35	1.17
Market-to-Book	0.23	2.20**	1.46	0.19	2.13**	1.37
Leverage	−0.09	−0.12	0.98	−0.18	−0.26	0.96
ROA	−0.01	−0.17	0.97	−0.01	−0.28	0.97
Annual Returns	−0.00	−0.43	0.95	0.00	0.23	1.03
Foreign Ownership	0.01	0.33	1.06	0.00	0.04	1.00
Overseas Sales Ratio	−0.01	−1.12	0.85	−0.01	−0.96	0.87
US Listing	0.49	0.95	1.12	0.42	0.88	1.11
Keiretsu Inclination	0.12	1.25	1.16	0.07	0.77	1.09
Owner Manager	−0.97	−3.13***	0.63	−0.73	−2.55**	0.71
Industry Expert Auditor	0.18	0.25	1.10	0.53	0.87	1.30
Abs Disc Accrual	5.55	1.80*	1.23	3.74	1.27	1.14
M&A	0.40	1.54	1.20	0.24	0.96	1.12
Suspension Announcement Returns	−0.12	−0.03	1.00	−0.64	−0.18	0.98
Firm Age	−0.00	−0.66	0.91	−0.00	−0.25	0.96
Audit Fees	−0.41	−0.76	0.90	−0.50	−0.94	0.87
Constant Cut 1	0.90	0.70		0.69	0.58	
Constant Cut 2	3.43	2.66***		2.92	2.46**	
Industry Fixed Effects	Included			Included		
Observations	386			433		
Pseudo R^2	0.1155			0.107		

*, **, *** Represent significance at the 10 percent, 5 percent, and 1 percent levels, respectively, (two-sided tests).
The dependent variable, *Switching*, takes the value of 0 if the firm does not have any interim auditor during the suspension, 1 if it adopts an interim auditor and goes back to Misuzu after the suspension ends, and 2 if it adopts a final auditor and does not revert to Misuzu. Column (1) excludes companies that move to Aarata as the final auditor after the suspension, whereas Column (2) includes companies that move to Aarata. All independent variables are measured for fiscal year 2005. Robust Z-statistics are reported. The "Odds Change" column presents the value of the change in odds for one standard deviation change in the value of the independent variable measured as exp(b∗SD of X), i.e., the change in odds for a one standard deviation increase in X.

For the estimation in Table 3 we use largely the same set of independent variables as in Table 2 and add additional variables that we collected for the ChuoAoyama sample to capture cross-sectional variation in the demand for audit quality. All variables are defined in Appendix B. We include firm size (*Log Total Assets*), risk (*Std Dev Returns*), and firm complexity (*Number of Segments*) as larger, riskier, and more complex firms need higher-quality audit effort. Firms with greater growth options (*Market-to-Book*) are likely to have greater information asymmetry between management and investors and, hence, are likely to demand higher-audit quality. *Leverage* is included to assess demand for high-quality auditing from debt holders. Firms with better performance as measured by *ROA* and *Annual Returns* may have lower demand for high-audit quality. However, Landsman et al. (2009) find that firms with poor performance are more likely to change auditors.

We include three variables that measure the extent of foreign interaction of the clients to test if non-Japanese investors and stakeholders drive demand for higher-quality auditing. These variables are *Foreign Ownership*, *Overseas Sales Ratio*, and *US Listing*. We expect firms belonging to a Keiretsu (*Keiretsu Inclination*) and those with a dominant owner-manager (*Owner Manager*) to have a lower likelihood of switching from ChuoAoyama since concentrated owners are an alternate monitoring channel that is a substitute for high-quality auditing. On the other hand, greater agency costs present in these ownership structures between the different classes of owners (concentrated versus public shareholders) might drive a demand for higher-audit quality.

Clients may be reluctant to switch auditors if ChuoAoyama were the industry expert auditor, so we add *Industry Expert Auditor* as a determinant of the switching decision. Landsman et al. (2009) suggest that firms with high accruals entail greater audit risk, suggesting a need for higher-quality audits; hence, we include a measure of absolute discretionary accruals (*Abs Disc Accrual*). Similarly, firms that recently complete a merger or acquisition (*M&A*) also entail greater audit risk, suggesting need for higher-quality auditors. We include the stock returns on the suspension announcement date (*Suspension Announcement Return*) to examine if companies that suffered a more negative market reaction would be more likely to leave ChuoAoyama. Finally, industry fixed effects allow for possible systematic differences at the industry level.

We report the results of these regressions in Table 3. In both estimations, *Log Total Assets* and *Market-to-Book* are both positively related to *Switching* at significance levels of 5 percent or less.[28] This shows that larger firms and growth firms are more likely to move from ChuoAoyama to higher-quality auditors. These effects are economically significant—a one standard deviation increase in firm size leads to a 55 percent (69 percent) increase in the switching likelihood in Column (1) (Column (2)), as seen in the changes in the odds ratios presented in the table. A similar change in market-to-book ratio leads to a change of 46 percent (37 percent) in Column (1) (Column (2)). In both estimations, we find a reliably negative coefficient on *Owner Manager* defined as firms where the President or Chairman are among the top 10 shareholders, implying that firms with a dominant manager-shareholder, reflecting potential managerial entrenchment, are less likely to be concerned about audit quality. As an alternative to the entrenchment explanation, owner-managers could be less concerned about audit quality if they serve as a substitute monitoring mechanism, lowering investor demand for auditing. No other variables are consistently significant.[29] To the extent that client firm size, market-to-book, and owner management are associated with a demand for higher-audit quality, these results support our hypothesis that the auditor changes are explained by differential demands for audit quality. However, we do not observe a similar effect for measures of risk and firm complexity.

As a robustness check, we test an alternate logit specification for which we define firms that reverted to Misuzu as one group irrespective of whether they hired an interim auditor, with the dependent variable taking the value 0 for clients that reverted to Misuzu and 1 if they switched auditors. Results (not tabulated) are similar to the ordered logit results. Size, market-to-book, and owner-manager continue to be significant with the same signs. In addition, firm complexity as

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[28] We also estimated ordered logit models where the dependent variable is coded as 0 for firms that revert to Misuzu after the suspension, 1 for firms that switched to Aarata, and 2 for firms that switched to a firm other than Aarata (non-PwC). Results are similar to those reported here.

[29] In Column (1) companies with lower accounting quality as measured by absolute value to total discretionary accruals are more likely to switch auditors, suggesting that clients with potentially higher audit risk (Landsman et al. 2009) preferred to switch to a higher-quality auditor.

measured by number of segments becomes statistically significant, suggesting that more complex firms exhibit demand for higher-quality auditing.

Also consistent with the above results, Aarata's clients were two to three times larger based on total assets, sales, or market capitalization than those that stayed with Misuzu and 50 to 100 percent larger than those that moved to other audit firms. Aarata's clients also have higher levels of foreign ownership, overseas sales, and U.S. listings.

Finally, because it is unlikely that ChuoAoyama rejected larger clients in the aftermath of its suspension, our results are consistent with a demand-side effect in which client firms drive switching decisions. This contrasts with the results in Weber et al. (2008), who find that smaller firms and those with recent IPOs switched auditors, a result that is also consistent with a supply-side effect in which KPMG rejected smaller clients to reduce audit risk.

Client Switching and Audit Personnel Moves

We attribute the changes away from ChuoAoyama in 2006 to a decline in ChuoAoyama's audit quality. An alternative interpretation is that audit clients simply followed their audit teams from one audit firm to another. Blouin et al. (2007) provide evidence that a significant number of Andersen clients followed their audit teams to new audit firms following Andersen's demise in 2002 to minimize switching costs. Given the closeness of relationships between auditors and clients in Japan, we expect this to be an important phenomenon in Japan as well. On the other hand, if the goal of changing auditors is to improve quality, then it is likely that clients would require a different audit team.

In Japan, audit reports are signed by individual audit partners, which allows us to identify the partner(s) responsible for the audits. We report the results of analyzing the auditor signatory data in Table 4. In Panel A, for the set of client firms that switch away from ChuoAoyama between F2005 and F2006, including those that went to Misuzu and Aarata, we report data on the audit partner(s) who signed these firms' audit reports in fiscal 2005 on behalf of ChuoAoyama and then in fiscal 2006 on behalf of the new audit firm. We then classify the firm as either having at least one common signatory across years—which we interpret as implying the same audit team at the new firm—or not.[30] The results show that there was a strong tendency for clients that stayed with PwC, at either Misuzu or Aarata, to have at least one common signatory, consistent with these audit firms having been formed from ChuoAoyama. Of the 50 Aarata clients that came from ChuoAoyama, 38 (76 percent) had signatories in common; of the 281 Misuzu clients, 239 (85 percent) had signatories in common. In strong contrast, none of the other switches had signatories in common with the fiscal 2005 ChuoAoyama auditors. The data thus clearly reject the interpretation that switches away from ChuoAoyama in fiscal 2006 were due to clients following their audit teams to new audit firms.

In Panel B of Table 4 we repeat the analysis for firms with signatory data available for fiscal years 2005 and 2007. The results are very different for these auditor changes. Here we find significant overlap in signatories for all the fiscal 2007 audit firms: 54 percent for Aarata, 35 percent for AZSA, 53 percent for ShinNihon, 27 percent for Tohmatsu, and 27 percent for other audit firms collectively.[31] This is due to the movement of audit teams from Misuzu to

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[30] In a given year, firms typically have one, two, or three auditor partners sign the report. Japan has mandatory auditor rotation (of audit partners within firms) so we do not expect these percentages to equal 100 percent even when there is no turnover.

[31] The number of firms in Table 4, Panel A differs from that in Panel B due to data constraints, either because we do not find the relevant filing or because some firms delist between fiscal years 2006 and 2007.

TABLE 4

The Relation between Audit Signatories at ChuoAoyama in Fiscal Year 2005 and Those at Subsequent Audit Firms in Fiscal Years 2006 and 2007

Panel A: Continuation of at Least One Audit Partner from ChuoAoyama to the Successor Audit Firm between Fiscal Years 2006 and 2005

	Same Partner in Next Audit Firm as in ChuoAoyama				
Successor Audit Firm	**No Common Signatory**	**Percent of Total**	**At Least One Common Signatory**	**Percent of Total**	**Total**
Aarata	12	24%	38	76%	50
AZSA	29	100%	0	0%	29
Misuzu	42	15%	239	85%	281
Shin Nihon	37	100%	0	0%	37
Tohmatsu	23	100%	0	0%	23
Other Auditors	19	100%	0	0%	19
Total	162	37%	277	63%	439

Panel B: Continuation of At Least One Audit Partner from ChuoAoyama to the Successor Audit Firm between Fiscal Years 2007 and 2005

	Same Partner in Next Audit Firm as in ChuoAoyama				
Successor Audit Firm	**No Common Signatory**	**Percent of Total**	**At Least One Common Signatory**	**Percent of Total**	**Total**
Aarata	25	46%	29	54%	54
AZSA	53	65%	29	35%	82
Misuzu	0	0%	7	100%	7
Shin Nihon	77	47%	88	53%	165
Tohmatsu	45	73%	17	27%	62
Other Auditors	36	73%	13	27%	49
Total	236	56%	183	44%	419

The table provides descriptive statistics on how many ChuoAoyama clients were audited in fiscal years 2006 or 2007 by the same audit partners in the successor audit firms as the audit partner who audited them in ChuoAoyama in fiscal year 2005. Audit partners are the signatories to the auditor report. For companies with multiple signatories, any one of the signatories being present in the next audit firm is counted as a common audit partner. The number of firms in Panel A differs from that in Panel B due to data availability, either because we do not find the relevant filing or because some firms delist between fiscal years 2006 and 2007.

other firms, consistent with the process we describe above for the winding up of Misuzu, under which audit team/client pairings tended to move to new audit firms together. It also reinforces our conclusion that the changes from fiscal years 2006 to 2007 are systematically different from those in fiscal years 2005 to 2006.

Event Study Analysis

Following previous research, we next analyze the market reaction to events associated with downward changes in the market's beliefs about an auditor's quality (e.g., Menon and Williams 1994; Baber et al. 1995; Chaney and Philipich 2002; Weber et al. 2008). Because there is no significant litigation risk for auditors in Japan, any abnormally negative returns to ChuoAoyama clients at the time of these events are likely due to either changes in expectations about audit quality or switching costs, although switching was not forced at the time of these events. To identify relevant events, we search Factiva for media articles related to the Kanebo fraud or otherwise discussing ChuoAoyama, Misuzu, or Aarata during the period outlined in Section II. Important events are listed in Appendix A.[32]

Table 5, Panel A provides mean and median market-adjusted returns for the three-day window centered on the event dates in Appendix A. Events 2, 4, 7, 9, 10, and 11 exhibit a statistically significant negative mean and median market reaction although the magnitudes are generally quite modest. The exception is event 9 with a mean return of −2.56 percent when PwC sent auditors from the U.S. and U.K. to address the problems at ChuoAoyama. Surprisingly, events 1 and 5 exhibit a small positive mean reaction. Following previous research, to address cross-sectional correlation among the contemporaneous daily stock returns of the ChuoAoyama client firms, we form a portfolio of these firms and estimate an adjusted market model regression using the time-series of portfolio returns.[33] Specifically, we estimate:

$$Return_t = \alpha_0 + \beta_1 Return_{TSEINDEX,t} + \theta_k Event_{k,t} + \varepsilon_t \tag{1}$$

where $Return_t$ is the return on day t to an equal-weighted portfolio of ChuoAoyama client firms, $Return_{TSEINDEX,t}$ is the return on the TSE Topix index for day t, and $Event_{k,t}$ is a dummy variable equal to 1 during the three-trading-day window centered on each of the 11 events, $k = 1,\ldots,11$. We estimate the regression over the 798 trading days from January 1, 2004 through March 31, 2007, which includes the full set of events.

We estimate Equation (1) for each of the events individually as well as for the combined set of events, and report results in Table 5, Panel B. The results provide little evidence of any significant effect on most event dates. The coefficients on the event dummies are small and insignificant for 10 of the 11 events. Consistent with the univariate results, the only exception is event 9, which shows a return of −0.88 percent (t = −3.56). When we combine the events, the coefficient on the overall event dummy is −0.15 percent (t = −1.94). This evidence offers modest support, at best, for the reputation argument. We also tested robustness of these results for a shorter window of time from June 1, 2004 to May 21, 2006 with 492 trading days and obtained similar results.

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[32] We validate our set of events by comparing the dates to those in three other studies by Japanese authors. See Sakuma (2009), Numata and Takeda (2010), Saito and Takeda (2011). We find substantial overlap in the dates, which provides assurance that we have identified the important events. These studies do not overlap with our work except in our event study analysis.

[33] We follow an approach similar to the multivariate regression model (MVRM) used previously by Schipper and Thompson (1983) and Bernard (1987). Under this approach, which Schipper and Thompson (1983; hereafter, ST) refer to as the joint GLS estimator, the time-series of individual firms' returns is used to compute return cross-correlations. That is, the portfolio of returns is a weighted average of all the returns, where the weights are calculated based on the inverse of the variance-covariance matrix estimated from firm-by-firm regressions. To be effective, this procedure requires a large number of time-series observations relative to the number of sample firms (Bernard 1987, 6). Because we have a relatively large number of firms compared to the number of time-series observations, we use a variant of the joint GLS approach also used by ST where we use unweighted averages of the firm-level returns, which assumes that the true covariance matrix is a scalar times the identity matrix. When we estimate the Table 5 regression specifications using the joint GLS estimator, the overall return is −0.06 percent (t = −2.00).

TABLE 5

Market Reaction to Significant Events in the Kanebo/Chuo Aoyama Scandal

Panel A: Descriptive Statistics of Returns of ChuoAoyama Clients on the Event Dates

	Market-Adjusted Returns for ChuoAoyama Clients (in Percentage)			
Events	**Mean**	**t-statistic**	**Median**	**Z-statistic**
Event 1	0.74	4.44***	0.30	3.52***
Event 2	−0.66	−2.56**	−0.85	−6.30***
Event 3	−0.18	−1.56	−0.04	−1.57
Event 4	−0.42	−2.36**	−1.05	−6.37***
Event 5	0.23	1.69*	−0.12	−0.55
Event 6	0.06	0.41	−0.56	−2.88***
Event 7	−0.85	−4.49***	−1.48	−8.32***
Event 8	0.22	1.18	0.21	0.65
Event 9	−2.56	−11.75***	−1.90	−10.80***
Event 10	−0.53	−3.41***	−1.21	−5.90***
Event 11	−0.30	−2.27**	−0.22	−2.18**

(continued on next page)

TABLE 5 (continued)

Panel B: Event Study Analysis of Reactions to Events Related to Revelation of Fraud Using Schipper and Thompson (1983) Methodology

$$Return_t = \alpha_0 + \beta_1 Return_{TSE\ INDEX} + \theta_k Event_{k,t} + \varepsilon_t.$$

Variable	Event 1	Event 2	Event 3	Event 4	Event 5	Event 6	Event 7	Event 8	Event 9	Event 10	Event 11	All Events (1–11)
Constant	0.03*	0.03*	0.03*	0.03*	0.03*	0.03*	0.03*	0.03*	0.03**	0.03*	0.03*	0.03**
	(1.69)	(1.83)	(1.80)	(1.79)	(1.75)	(1.75)	(1.82)	(1.75)	(2.00)	(1.79)	(1.82)	(2.12)
TSE Index	0.86***	0.87***	0.86***	0.87***	0.86***	0.86***	0.87***	0.86***	0.86***	0.87***	0.86***	0.87***
Returns	(58.43)	(58.51)	(58.39)	(58.46)	(58.45)	(58.43)	(58.47)	(58.45)	(58.80)	(58.44)	(58.35)	(58.61)
Event i	0.31	−0.28	−0.16	−0.12	0.05	0.06	−0.24	0.03	−0.88***	−0.13	−0.22	−0.15*
	(1.26)	(−1.12)	(−0.63)	(−0.49)	(0.18)	(0.25)	(−0.95)	(0.11)	(−3.56)	(−0.51)	(−0.88)	(−1.94)
Observations	798	798	798	798	798	798	798	798	798	798	798	798
Adjusted R^2	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.81	0.81

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*, **, *** Indicate that the returns are significantly different from 0 at the 10 percent, 5 percent, and 1 percent levels of significance, respectively.

Panel A presents the mean and median market-adjusted returns for ChuoAoyama clients over the three-day window centered on the event dates tabulated in Appendix A. Market adjustment is done using TSE Index for the market index. All returns are in percents.

Panel B presents the coefficients and t-statistics (in parentheses) of the following regression. The event dates are presented in Appendix A and the variable definitions and data sources in Appendix B. TSE Index returns are computed using the Tokyo Stock Price Index (TOPIX). The sample size of 798 comes from returns on all trading days between January 1, 2004 and March 31, 2007.

If the reputation argument matters more for larger, more prominent firms in Japan, then we might expect the event date results to be stronger for clients listed on the First Section TSE (larger, more prominent firms).[34] These results (not tabulated) are similar to those we report in Table 5, with an overall return of −0.15 percent (t = −2.09).[35] Overall, there is little evidence that any of these events is associated with economically significant abnormal event performance.

To summarize, the event study results provide little evidence that the former clients of ChuoAoyama suffered any material decline in value on the dates that we identify. Numata and Takeda (2010) also conduct market reaction tests on the ChuoAoyama event dates. That paper includes four of our event dates—events 2, 3, 4, and 11—and does not follow the Schipper and Thompson (1983) approach to the event study, instead carrying out a market model test for a sample of only First Section companies. They report results similar in magnitude and significance to our results in Panel A of Table 5 and conclude that the significant negative reaction indicates a reputational penalty. However, the multivariate regression approach following Schipper and Thompson (1983) and our larger set of dates weakens the estimates in our tests. Our results are in contrast to those in Weber et al. (2008), who find a statistically significant negative return for two out of their three event dates and for the combination of event dates. However, it is difficult to reach very strong conclusions based on this evidence, given the relatively long time period over which concerns about ChuoAoyama's low audit quality were revealed. It could also be that these events had negative implications not only for ChuoAoyama clients, but also for firms, in general, in Japan, especially those on the TSE First Section, so that market-adjusting the returns removes the effect we are looking for.[36]

IV. SUMMARY AND CONCLUSIONS

In May 2006, the Japanese FSA took the unprecedented step of suspending the operations of ChuoAoyama, the PwC affiliate in Japan, for two months for its role in a major accounting fraud at Kanebo. Even as the suspension was announced, PwC was taking its own actions to remedy the apparent shortcomings of its Japanese unit. First, it brought in high-level personnel from overseas to revamp ChuoAoyama's audit operations in an attempt to improve audit quality. Second, it set up a new, smaller Japanese affiliate, PwC Aarata, which was positioned as a high-quality audit firm. PwC used this firm to audit important Japanese clients like Toyota and Sony, as well as the Japanese operations of large multinational clients such as Unilever. Third, when another major fraud came to light after ChuoAoyama resumed business as Misuzu, PwC shut down Misuzu, ceding a large part of its Japanese business to competitors. These actions make it clear that PwC viewed the audit-quality issues at ChuoAoyama as potentially damaging to its international reputation, and support our view that an auditor's reputation for quality is of first-order importance.

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[34] As another robustness check, we also examine the raw and market-adjusted returns for these firms in short windows around each of these events, as well as for the subset listed on the First Section of the TSE. The market-adjusted returns to some of these events are significantly negative and significantly more negative than those for the non-ChuoAoyama clients. The magnitudes of these abnormal returns and the differences in abnormal returns, however, are quite small. These results are somewhat stronger when we restrict attention to stocks listed on the TSE First Section. Perhaps the strongest evidence in favor of an effect is the mean negative raw (market-adjusted) return of −3.14 percent (−0.64 percent) for First Section ChuoAoyama clients over a three-day window centered on the FSA suspension announcement (event 11), although the corresponding mean returns for non-ChuoAoyama clients are also negative, at −2.59 percent (−0.10 percent). While statistically significant, differences between these amounts are small in economic magnitude.

[35] As a further robustness check, we also perform these tests using returns for a portfolio of all non-PwC clients in our sample as the benchmark rather than the market index returns (not reported in tables). There is, again, little evidence of systematically negative returns, with an overall return of −0.03 percent (t = −1.48).

[36] For First Section TSE stocks the mean raw return for the ChuoAoyama clients on the suspension announcement date is −3.14 percent and for other TSE stocks is −2.59 percent, which is some evidence of this possibility.

We use these events to provide evidence on the importance of an auditor's reputation for quality. Previous studies using U.S. data have difficulty distinguishing the relative importance of an auditor's market-driven incentive to maintain a reputation for delivering quality audits versus the possibility that auditors are subject to potentially large, even catastrophic, legal liability for defective audits. Because litigation concerns are negligible in Japan, the litigation argument can be effectively ruled out. Consequently, we focus on whether these events provide evidence that supports the importance of auditor reputation.

Our results are largely consistent with the importance of reputation effects. We find evidence that a relatively large number of ChuoAoyama's clients left the firm for other auditors as the seriousness of ChuoAoyama's quality problems became evident. The rate of client turnover at ChuoAoyama in fiscal year 2006, before it became apparent that the firm would be shut down but after audit-quality questions had been raised, was substantially higher than would otherwise be expected, consistent with clients leaving once the firm's reputation for quality was seriously diminished. Moreover, we find that the likelihood of switching is higher for larger clients and clients with higher market-to-book ratios, characteristics associated with a demand for higher-audit quality, and lower for firms with greater managerial ownership, indicating a lower demand for audit quality in such firms. Clients that moved to Aarata were also larger, with higher market-to-book ratios, a greater extent of cross-listing, and higher foreign ownership. These switches are not the result of clients following their audit teams to new auditors. Our event study results weakly support the auditor-quality argument, but are likely to lack power because questions about ChuoAoyama's audit quality were revealed over an extended period.

Our conclusions are subject to two caveats. First, we find that clients switched away from ChuoAoyama in large numbers in Spring 2006, just after Japanese regulators announced the two-month suspension and PwC formed Aarata. While we interpret these events as being a clear and undeniable signal of audit-quality problems at ChuoAoyama, we cannot know for sure what drove these switches. It is possible that the suspension caused firms to switch auditors for reasons unrelated to audit quality. Second, our analysis presumes that audit quality is important to Japanese companies. While we believe this to be the case, especially over the past two decades as Japanese capital markets have evolved to be more like their Western counterparts, it is possible that audit quality is, in general, less important in Japan.

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APPENDIX A

Significant Events in the Kanebo/ChuoAoyama Scandal, Including Events Used in Event Study Analysis

	Date	Event
Event 1	07/09/2004	Kanebo drops ChuoAoyama as auditor
Event 2	10/28/2004	Kanebo reports that the preliminary internal investigation has found fraud. Investigation continues.
	03/31/2005	End of Fiscal 2004
Event 3	04/13/2005	Results of fraud investigation announced by Kanebo. Will restate last five years of financial statements.
Event 4	07/29/2005	Three former Kanebo executives are arrested. CA offices are searched.
Event 5	08/18/2005	Indictments brought against ChuoAoyama auditors.
Event 6	09/13/2005	Four auditors from CA who audited Kanebo are arrested.
Event 7	09/19/2005	Top executives from CA are questioned by prosecutors.
Event 8	10/03/2005	CA partners step down. Three CA auditors indicted. FSA indicates it will impose penalties on CA.
Event 9	02/20/2006	PwC sends team of auditors from U.K. and U.S. to revamp ChuoAoyama.
Event 10	03/30/2006	ChuoAoyama accountants plead guilty to falsifying Kanebo financials.
	03/31/2006	End of Fiscal 2005
Event 11	05/10/2006	CA suspension announced; PwC announces two-firm formula with creation of a new, higher quality firm.
	03/31/2007	End of Fiscal 2006

APPENDIX B

Variable Definitions

Variable Name	Definition	Source
Switching	Ordinal ranking of audit quality that takes the value 0 for firms that went back to ChuoAoyama/Misuzu after the suspension without using an interim auditor, 1 for firms that went back to ChuoAoyama/Misuzu after using an interim auditor, and 2 for firms that switched away from ChuoAoyama/Misuzu.	Company Filings
Market Cap	Market value of equity in USD millions.	Worldscope
Total Assets	Total assets in USD millions.	Worldscope
Log Total Assets	Natural log of total assets.	Worldscope
Market-to-Book	Market value of equity/book value of equity.	Worldscope
Leverage	Long-term debt divided by total assets.	Worldscope
Net Income	Net Income in USD millions.	Worldscope
ROA	Net income divided by total assets.	Worldscope
Loss	Indicator variable that takes the value 1 if net income is less than 0, and 0 otherwise.	
Annual Returns	Fiscal year stock returns.	Datastream
Foreign Ownership	Percent ownership of the firm's equity by foreign entities.	JCH, 2004
Overseas Sales Ratio	Ratio of sales outside Japan to total sales, expressed in percentage.	JCH, 2004
Number of Segments	Number of distinct divisions for which shares of total sales are reported.	JCH, 2004
Firm Age	Years since the firm was founded.	JCH, 2004
US Listing	All U.S. listings, including those via stock exchange, portal, and the OTC.	Citibank ADR database
Std Dev Returns	Firm-level standard deviation of daily stock returns in 2005.	Datastream
Industry	Industry affiliation of the company.	Tokyo Stock Exchange
TSE Index	The Tokyo Stock Price Index (TOPIX) is a composite index of all common stocks listed on the first section of the Tokyo Stock Exchange.	Datastream
Keiretsu Inclination	*Keiretsu Inclination* measures the closeness to the *keiretsu*, with higher numbers indicating a closer inclination. There are two types of *keiretsu*, horizontal (also known as "main bank groups") and vertical; for horizontals, the data are from Part III of the book, and for verticals, from Part II. The most recent edition of the source (2001) provides information as of 2000. This variable ranges from 0 for companies that are not part of a *keiretsu* to 4 for companies with the highest level of inclination ("nucleus" group companies). The measured inclination is based on five factors: (1) the characteristics and historical background of the groups and/or the company; (2) sources and amount of bank loans; (3) board of directors sent by and/or sent to nucleus and/or other group companies; (4) the company attitude toward the group; and (5) the company's connections with other groups and/or non-group companies.	Industrial Groupings in Japan

(continued on next page)

APPENDIX B (continued)

Variable Name	Definition	Source
Audit Fees	Audit fees paid to ChuoAoyama for FY 2005 in USD millions.	Company filings
Owner Manager	Indicator variable that takes the value 1 if the president or chairman of the company are among the top ten shareholders of the company, and 0 otherwise. Measured only for ChuoAoyama clients.	JCH, 2004
Industry Expert Auditor	Indicator that takes the value 1 when the audit firm audits greater than 20 percent of clients (weighted by total assets) in a given industry, and 0 otherwise.	Datastream
Abs Disc Accrual	Absolute value of total discretionary accruals estimated as per DeFond and Subramanyam (1998).	Datastream
M&A	Indicator variable that takes the value 1 when the firm has completed an M&A transaction in the prior two years, and 0 otherwise.	Thomson SDC
Suspension Announcement Returns	Stock returns on three-day window around the date the ChuoAoyama suspension was announced (May 10, 2006).	Datastream

The table provides definitions and data sources for all the variables in subsequent tests.
JCH = Japan Company Handbook.
USD = United States Dollar.

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Does Mandatory IFRS Adoption Improve Information Comparability?

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ABSTRACT: This study examines whether the mandatory adoption of International Financial Reporting Standards (IFRS) in the European Union significantly improves information comparability in 17 European countries. We employ three proxies—the similarity of accounting functions that translate economic events into accounting data, the degree of information transfer, and the similarity of the information content of earnings and of the book value of equity—to measure information comparability. Our results suggest that mandatory IFRS adoption improves cross-country information comparability by making similar things look more alike without making different things look less different. Our results also suggest that both accounting convergence and higher quality information under IFRS are the likely drivers of the comparability improvement. In addition, we find some evidence that cross-country comparability improvement is affected by firms' institutional environment.

Keywords: *IFRS adoption; information comparability; institutional environment.*

Data Availability: *Data are available from commercial providers (Worldscope, DataStream, and I/B/E/S).*

I. INTRODUCTION

Demand for internationally comparable accounting information has increased significantly in recent years due to rapid growth in cross-country investment. One reflection of this trend is the widespread adoption of International Financial Reporting Standards (IFRS), including the mandatory adoption of IFRS in the European Union (EU) in 2005. Because mandatory IFRS are some of the most important financial reporting regulations in recent years, many studies have examined the various effects of their adoption (e.g., Capkun et al. 2008; Daske et

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al. 2008; Armstrong et al. 2010; Horton and Serafeim 2010; Clarkson et al. 2011). However, to the best of our knowledge, the effect of IFRS adoption on cross-country information comparability has not been thoroughly examined, although better comparability has been commonly cited as one of the main benefits of IFRS adoption. The purpose of this study is, thus, to provide more empirical evidence on this issue.

Information comparability is "the quality of information that enables users to identify similarities in and differences between two sets of economic phenomena" (Financial Accounting Standards Board [FASB] 1980, 9; International Accounting Standards Board [IASB] 2010, A36). The Financial Accounting Standards Board further states that "greater comparability of accounting information, which most people agree is a worthwhile aim, is not to be attained by making unlike things look alike any more than by making like things look different" (FASB 1980, 42). These statements assert that there are two equally important facets of information comparability: the similarity facet, which indicates whether firms engaged in similar economic activities report similar accounting amounts, and the difference facet, which indicates whether firms engaged in different economic activities report dissimilar accounting amounts. Because improvement in one facet of comparability does not automatically lead to improvement in the other facet, the overall benefit of IFRS adoption on cross-country information comparability is contingent on whether adoption improves both facets of comparability, or at least improves one without impairing the other.

It is intuitively appealing that mandatory IFRS adoption improves the similarity facet of cross-country information comparability, and some empirical evidence from prior studies is consistent with this intuition (e.g., Ashbaugh and Pincus 2001; Chi 2009; Barth et al. 2011; DeFond et al. 2011). However, our view is that more research is required before a firm conclusion can be drawn, as prior studies either use input-based comparability measures, such as accounting rule variability and the number of accounting rules used on an announcement date (e.g., Chi 2009; DeFond et al. 2011), or rely on samples of voluntary adopters that likely have different reporting incentives from those of mandatory adopters (e.g., Ashbaugh and Pincus 2001; Barth et al. 2011). More importantly, the effect of IFRS adoption on the difference facet of cross-country comparability has not been addressed at all. This facet of comparability is important for mandatory IFRS adoption because "an overemphasis on uniformity may reduce comparability by making unlike things look alike" (IASB 2010, A36). For example, if IFRS allow fewer accounting choices than local accounting standards, then their adoption could force firms to treat different economic transactions in a more similar way, thus, diminishing the difference facet of comparability.[1] In this study, we test the effect of IFRS adoption on both facets of cross-country information comparability.

Following previous comparability studies (e.g., Bradshaw et al. 2009; Barth et al. 2011; DeFond et al. 2011; De Franco et al. 2011), we refer to firms in the same industry as similar firms and those in different industries as different firms.[2] We compare the cross-country information comparability of the similarity facet among similar firms from different countries, and we compare the cross-country information comparability of the difference facet among different firms from different countries, across the pre- and post-IFRS periods. Because increased cross-country comparability after IFRS, if any, could be affected by accounting convergence and/or higher quality financial information under IFRS than under local accounting standards, we also examine the two facets of within-country comparability to identify the possible drivers underlying the comparability improvement. As IFRS adoption generally reduces firms' accounting choices (e.g., Ashbaugh and Pincus 2001; Hoogendoorn 2006), an increase in both cross- and within-country comparability would suggest that both accounting convergence and higher quality information are the underlying drivers, whereas only an increase in cross-country comparability would suggest that convergence is the likely driver.

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[1] As an example, some local accounting standards allow firms to use the acquisition method for acquisitions and the pooling method for mergers. However, IFRS allow only the acquisition method for all business combinations that are not under common control. As such, firms are forced to account for acquisitions and mergers in the same way.

[2] The underlying logic is that firms in the same industry have similar operational properties and face similar economic shocks, whereas firms in different industries may have different operational properties and face different industry-specific shocks. This definition is also supported by the common practice of analysts using firms in the same industry as benchmarks when analyzing a firm's financial statements.

We employ three proxies for information comparability. The first is the similarity of accounting functions measure developed by De Franco et al. (2011).[3] With this approach, accounting is essentially the mapping of economic transactions to financial statements, and information comparability can be defined as the similarity of firms' accounting functions that translate economic transactions into accounting data. The second proxy is the degree of information transfer, as measured by the association between the earnings surprise of an announcing firm and the contemporaneous stock price movements of other firms. The intuition underlying this measure is that an earnings announcement by a firm conveys information that has not previously been publicly available, and investors can abstract such information and adjust stock prices for other firms with comparable accounting. The third proxy is the similarity of the information content of earnings (ICE) and the information content of the book value of equity (ICBV), as measured by the long-window association between stock price and earnings and the book value of equity. Because ICE and ICBV capture the extent to which accounting earnings and the book value of equity reflect a firm's economic performance, firms that engage in similar economic activities should have a similar ICE and ICBV if their accounting systems are comparable.[4]

To test our research questions, we use data from 17 European countries in which listed firms must use IFRS to prepare their consolidated financial statements since 2005. We find that the comparability of accounting information for similar firms from different countries is significantly greater in the post-IFRS period (2005–2007) than in the pre-IFRS period (2002–2004), using all of the three comparability measures. However, using the measure of the similarity of accounting functions and information transfer,[5] we find that there are no discernible comparability changes among different firms from different countries across the two periods. Further, the comparability change in the similarity facet is greater than that in the difference facet. These results, thus, suggest that IFRS adoption improves the similarity facet of cross-country information comparability without discernibly impairing the difference facet of comparability. For within-country analysis, we find that the similarity facet of comparability increases for the accounting function measure, but not for the information transfer measure. However, the increase in the similarity facet of within-country comparability does not statistically differ from that of cross-country comparability using both measures. We, thus, interpret our results as consistent with the view that both accounting convergence and information quality play an important role in the observed IFRS effects.

As a supplemental test, we examine whether the improvement in the similarity facet of cross-country comparability is affected by firms' institutional environment. We classify a firm's institutional environment by the common law versus code law origin of the legal system in its home country, because a country's legal origin can proxy for a variety of institutional features, such as financial reporting incentives and the effectiveness of legal enforcement. We find that among similar firms from countries with the same legal origin, nearly all of the comparability measures increase after IFRS adoption. However, among similar firms from countries with different legal origins, only one comparability measure increases. Overall, these results provide some evidence that the institutional environment in the home countries of firms influences the effect of IFRS adoption on information comparability.

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[3] De Franco et al. (2011) also develop another comparability measure that captures the similarity of two firms' economic events and accounting functions at the same time. Because we examine whether IFRS adoption improves the similarity of accounting functions, we do not use that measure here.

[4] We summarize comparability measures for each test in Figure 1.

[5] As explained in "Similarity of ICE and ICBV" in Section III, the similarity of ICE and ICBV measure is not applicable for the sample of different firms from different countries and the sample of similar firms from the same country.

This study contributes to the literature by providing empirical evidence on the association between mandatory IFRS adoption and information comparability. We address the impact of IFRS adoption on both the similarity and difference facets of comparability, whereas previous comparability studies address only the former. Our results suggest that mandatory IFRS adoption has an overall benefit in enhancing investors' ability to compare firms with similar fundamentals without discernibly reducing their ability to distinguish firms with different fundamentals. This study also extends the literature on the importance of institutions by documenting that IFRS adoption is more likely to improve comparability among firms with similar institutional environments. All of the findings may have practical implications for regulators and investors in countries considering IFRS adoption.

Section II discusses the literature and the differences between our study and previous studies. Section III describes the information comparability measures employed, and Section IV presents the empirical tests and their results. Section V presents supplemental and sensitivity tests, and Section VI concludes.

II. MOTIVATION AND LITERATURE REVIEW

Globalization in the last two decades has significantly increased the economic interaction among countries, which, in turn, has created demand for more internationally comparable accounting information. There are several potential benefits associated with enhanced information comparability. For example, both the FASB and the International Accounting Standards Board argue that more comparable information enables global markets to operate with less friction.[6] Several studies also suggest that greater information comparability facilitates international transactions and minimizes exchange costs (e.g., Turner 1983; Weber 1992; Choi et al. 1999).

The importance of comparable financial information in the global economy has led to growing research interest in information comparability. De Franco et al. (2011) develop two measures of accounting comparability and test their construct validity. They also use U.S. data to examine the benefits of comparability, finding that comparability is positively associated with analyst following and forecast accuracy, and negatively associated with forecast optimism and dispersion. Bradshaw et al. (2009) examine the association between accounting method variability (as a proxy for information comparability) and analyst characteristics using U.S. data, finding that a lower level of information comparability is associated with greater analyst forecast error and dispersion.

Several recent studies examine issues related to accounting comparability resulting from IFRS adoption. Using data from 26 countries from 1995 to 2006, Barth et al. (2011) find that the value relevance of earnings and equity book value is more comparable among non-U.S. firms after the application of the International Accounting Standards than when local accounting standards were used. Beuselinck et al. (2007) investigate the determinants of earnings comparability as proxied by the association between accruals and cash flows for the period 1990–2005 using data from 14 EU countries, finding that the association is not affected by mandatory IFRS adoption. DeFond et al. (2011) examine the benefits of increased accounting uniformity as a proxy for comparability, using data from 14 European countries. They find that foreign mutual fund ownership increases among mandatory adopters in countries with strong implementation credibility. Li (2010) cites enhanced comparability as the likely mechanism behind the cost of equity reduction in the EU after IFRS adoption. Wu and Zhang (2010) find an increased use of relative performance evaluation based on foreign peer firms' accounting information among European firms in the post-IFRS period.

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[6] These arguments appeared in news releases by the FASB on February 27, 2006, and by the IASB on February 22, 2006.

Our study differs from these recent studies in two important ways. First, we address both the similarity and difference facets of comparability, whereas the previous studies only address issues related to the former. Second, we examine a different research question and, thus, use different comparability measures, sample periods, and sample firms. For example, whereas we examine the relation between comparability and mandatory IFRS adoption in 17 European countries, Barth et al. (2011) examine the comparability of mandatory and voluntary non-U.S. IFRS adopters and U.S. firms. Beuselinck et al. (2007) rely on a non-price-related comparability measure and data from the year of switch only (2005), whereas we use three price-related measures and three post-IFRS years of data.[7] Li (2010) and DeFond et al. (2011) use input-based and non-firm-specific comparability measures, whereas we employ output-based and firm-specific measures.[8]

III. SAMPLES AND COMPARABILITY MEASURES

Initial Sample

We collect data on listed firms in Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, The Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom from the Worldscope database. The number of firms included in the Worldscope database for each country is presented in Table 1, Panel A. We exclude financial, insurance, and real estate firms (SIC codes 6000–6999) from the sample because they have special operating properties and are subject to additional regulations. We also exclude firms that adopted IFRS before 2005 and firms that do not report consolidated financial statements from the sample. Finally, we restrict the sample to firms with a fiscal year ending in December to ensure that each firm has the same sample period. Table 1, Panel B, summarizes the sample selection procedure and the final sample size.

Comparability Measures and Related Samples

Similarity of Accounting Functions

This comparability measure was developed by De Franco et al. (2011), who argue that accounting is essentially the mapping of economic transactions to financial statements, and that accounting comparability can, thus, be defined as the similarity of accounting functions to translate economic transactions into accounting data. We first estimate each firm's accounting function by applying the following equation:

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[7] Price-related comparability measures capture comparability from the perspectives of both investors and analysts. Because investors and analysts together are likely to be the largest groups using reported accounting information to make investment decisions, it is important to examine comparability from their perspectives.

[8] Following De Franco et al. (2011), we classify comparability measures that are computed using reported accounting data into the output-based category, and those that are based on accounting methods or accounting policies into the input-based category. For the dichotomy of firm-specific and non-firm-specific, measures that are computed using firm-level data are classified into the category of firm-specific, and those computed using industry- or country-level data are classified into the category of non-firm-specific.

TABLE 1
Sample Composition by Country and Initial Sample Procedures

Panel A: Sample Composition by Country from the Worldscope Database

Country	Number of Firms
Austria	98
Belgium	153
Denmark	191
Finland	127
France	818
Germany	966
Greece	293
Ireland	64
Italy	281
Luxembourg	42
The Netherlands	171
Norway	206
Portugal	60
Spain	143
Sweden	419
Switzerland	273
United Kingdom	1,951
Total	6,256

Panel B: Initial Sample Selection Procedures

Number of firms obtained from Worldscope	6,256
Exclusions:	
Financial institutions, insurance companies, and real-estate firms	(1,538)
Fiscal year not ending in December	(1,359)
Firms that voluntarily adopted IFRS before the mandatory requirement	(573)
Firms without consolidated accounts	(224)
Total number of firms in the initial sample	2,562

This table presents the sample composition by country and the initial sample selection procedure. Panel A presents the sample composition by country for the 6,256 firms collected from the Worldscope database. Panel B presents the initial sample selection procedure, which gives 2,562 firms.

$$ROA_{it} = \alpha^i + \beta^i RET_{it} + \varepsilon_{it}, \tag{1}$$

where ROA_{it} is the return on assets (an accounting performance measure) of firm i in period t, and is calculated as net income divided by total assets, and RET_{it}, a proxy for economic events, is the stock return of firm i in period t. The coefficients (α^i and β^i) represent the estimated accounting function of a firm. Using semiannual data, we estimate each firm's accounting function in the pre- and post-IFRS periods separately.

The similarity of accounting functions of firm i and firm j is then computed as follows. First, we translate firm i's economic activity into accounting ROA using its own accounting function (α^i and β^i) and the corresponding firm j's accounting function (α^j and β^j) for each semiannual year. Thus, we obtain the two expected ROAs, $E(ROA)^i_{it}$ and $E(ROA)^j_{it}$, and the absolute value of their difference for each semiannual year.[9] This process yields six absolute value *ROA* differences for the pre-IFRS period (2002–2004) and six differences for the post-IFRS (2005–2007) period. Second, we translate firm *j*'s economic activity into accounting *ROA* using its own accounting function (α^j and β^j) and firm *i*'s accounting function (α^i and β^i), and obtain the two expected *ROA*s, $E(ROA)^j_{jt}$ and $E(ROA)^i_{jt}$, and the absolute value of their difference for each semiannual year. Similarly, there are six differences for the pre-IFRS period and six differences for the post-IFRS period.[10] Third, we calculate the mean of the 12 differences in the pre-IFRS period (post-IFRS period) as the proxy for the information comparability of firms *i* and *j* in the pre-IFRS period (post-IFRS period). We multiply the mean by -1 so that a higher value represents greater comparability.

To generate a sample for a test of the similarity facet of cross-country comparability, we form pairs of firms from the same industry, based on the three-digit SIC code, but different countries. We rank the firms in each industry based on their total assets in 2006. For the largest firm, we choose a foreign firm with the closest total assets as its pair. We also require the ratio of the smaller value of total assets to the larger value in a pair to be greater than 50 percent. This procedure generates 133 pairs of firms. To form a sample to test the difference facet of cross-country comparability, we form pairs from different industries and countries. We require one firm in the pair to be a manufacturing firm (one-digit SIC code of 2 or 3) and the other to be a service firm (one-digit SIC code of 7 or 8).[11] For each manufacturing firm, we choose a service firm in a different country that is closest in terms of total assets. The ratio of the smaller value of total assets to the larger value must be greater than 50 percent. This procedure results in 148 pairs of firms. We employ the same procedure to form pairs from the same country, and generate 61 pairs of similar firms and 125 pairs of different firms.

Degree of Information Transfer

Previous studies have found associations between the information released by announcing firms and the returns of other firms in the same industry and country. For example, such information transfer has been documented with respect to the news in the earnings announcements (Firth 1976; Foster 1981; Clinch and Sinclair 1987; Han and Wild 1990; Hramnath 2002), stock split announcements (Tawatnuntachai and D'Mello 2002), management earnings forecasts (Baginski 1987; Han et al. 1989), and corporate security offerings (Szewczyk 1992). Such transfers occur because an announcement by a firm conveys information that has not previously been publicly available, and the stock market responds by reevaluating the value of non-announcing firms and adjusting their share prices accordingly. An important condition for information transfer through earnings announcements is comparable accounting earnings. If earnings are not comparable, then an earnings announcement by a firm is of little value in predicting the value of other firms, resulting in a low degree of information transfer. As such, the degree of information transfer reflects the level of information comparability.

We measure an earnings surprise for an announcing firm as the difference between reported earnings and *ex ante* expected earnings. The *ex ante* expected earnings are proxied by the mean analyst earnings forecast in the month immediately before the earnings release, which is obtained from the I/B/E/S database.[12] We calculate the abnormal stock returns of a non-announcing firm in the three days around the earnings release of the announcing firm (Day −1 to Day +1) using the following model:

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[9] $E(ROA)^i_{it} = \alpha^i + \beta^i RET_{it}$ and $E(ROA)^j_{it} = \alpha^j + \beta^j RET_{it}$.

[10] $E(ROA)^i_{jt} = \alpha^i + \beta^i RET_{jt}$ and $E(ROA)^j_{jt} = \alpha^j + \beta^j RET_{jt}$.

[11] Because we assume that similar (different) firms face similar (different) economic shocks and have similar (different) operational properties, we use the three-digit SIC code to define similar firms to ensure that these firms are fundamentally similar, and use the one-digit SIC code to define different firms to ensure that they are fundamentally different.

[12] Only annual earnings surprises are used in this test because the data on announcement date for semiannual or quarterly earnings are not available for most sample firms.

$$U_{it} = RET_{it} - (\alpha^i + \beta^i RET_{mt}), \tag{2}$$

where U_{it} is the abnormal stock return of firm i on day t, and RET_{it} and RET_{mt} are the stock return of firm i and the market return of firm i's domestic market on day t, respectively.[13] We estimate the coefficients α^i and β^i separately for each fiscal year using data from Day −185 to Day −6, where Day 0 is the earnings announcement date of the announcing firm. The cumulative abnormal return of a non-announcing firm is the sum of its abnormal returns on the three days surrounding the earnings release date of the announcing firm. To reduce the cross-sectional correlation of prediction errors across clusters of non-announcing firms, we follow Baginski (1987) and use the mean cumulative abnormal return of all of the non-announcing firms in the empirical analyses.

To form a sample to test the similarity facet of cross-country comparability, we first identify all of the industries at the three-digit SIC code level that contain at least two firms from different countries. We rank the firms in each industry by their earnings announcement dates. To qualify as an announcing firm, the firm's earnings announcement window (Day −1 to Day +1) must not overlap with that of any other firm, to avoid the situation of non-announcing firms reacting to more than one earnings announcement on the same day. Each announcing firm is paired with all similar foreign firms with later earnings announcement dates that are of a similar size to the announcing firm (a ratio of the smaller total asset value to the larger value of greater than 50 percent). This approach yields 744 pair-year observations. To form a sample to test the difference facet of cross-country comparability, we match each announcing firm in the manufacturing sector with all of the foreign service firms with later earnings announcements and similar total assets, and *vice versa*. This sample contains 910 pair-year observations. For the two within-country samples, we implement the same procedure to form pairs within the same country, resulting in 195 and 630 pair-year observations for the tests of the similarity facet and difference facet of within-country comparability, respectively.

Similarity of ICE and ICBV

We employ the Ohlson (1995) model, in which a firm's market value is regressed on net income and equity book value, to test if ICE and ICBV are similar for two sets of firms:

$$MV_{it} = \beta_0 + \beta_1 NI_{it} + \beta_2 BV_{it} + \beta_3 D^x + \beta_4 D^x{*}NI_{it} + \beta_5 D^x{*}BV_{it} + \varepsilon_{it}, \tag{3}$$

where MV_{it} is the total market value of equity at the end of the fiscal year, NI_{it} is the net income,[14] and BV_{it} is the book value of equity.[15] These three variables are scaled by the number of outstanding common shares. The variable D^x is an indicator where x equals 1 when it is a country indicator, and 2 when it is an industry sector indicator. A significant β_4 and β_5 indicate that firms from different sets have a different ICE and ICBV, respectively, and, thus, a low degree of information comparability.

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[13] The market return indices for Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, The Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom are ATX, BEL, OMXC, OMXH, CAC, DAX, Athex, Ireland SE, MIB, LUX, AEX, OBX, PSI, IBEX, MX Stockholm, Swiss Market, and FTSE, respectively.

[14] In the Worldscope database, net income is defined as earnings (revenues less operating costs, depreciation, interest, taxes, and other expenses) less preferred dividends for non-U.S. firms.

[15] We use annual data to estimate Equation (3) because data on the semiannual book value of equity are missing for most sample firms in the Worldscope database.

To examine the similarity facet of cross-country information comparability, we estimate Equation (3) within each industry, as determined by three-digit SIC code, for every possible combination of two countries in the pre- and post-IFRS periods separately. We assign an ICE (ICBV) comparability score of 1 if β_4 (β_5) is insignificant (defined as a two-tailed p-value of more than 5 percent), and 0 otherwise. We require at least three firms in an industry in a given country to ensure that there are at least 18 observations in each regression. This process yields 210 regressions and, hence, 210 comparability scores for ICE and ICBV in the pre- and post-IFRS periods, respectively.

For the difference facet of within-country comparability analysis, we estimate Equation (3) using a set of firms from the manufacturing sector (one-digit SIC code of 2 or 3) and the other set of firms from the service sector (one-digit SIC code of 7 or 8), for every possible combination within a country in the pre- and post-IFRS periods separately. This results in 64 comparability scores for ICE and ICBV in each period. To estimate Equation (3), the sample firms must differ on only one of the industry sector or country dimensions captured by indicator D^x. Thus, this approach is not applicable to test either the difference facet of cross-country comparability, because sample firms are from different industry sectors and countries, or to test the similarity facet of within-country comparability, because sample firms are from both the same industry and the same country. Our comparability measures for each test are summarized in Figure 1.

IV. RESULTS

Results Using the Similarity of Accounting Functions Measure

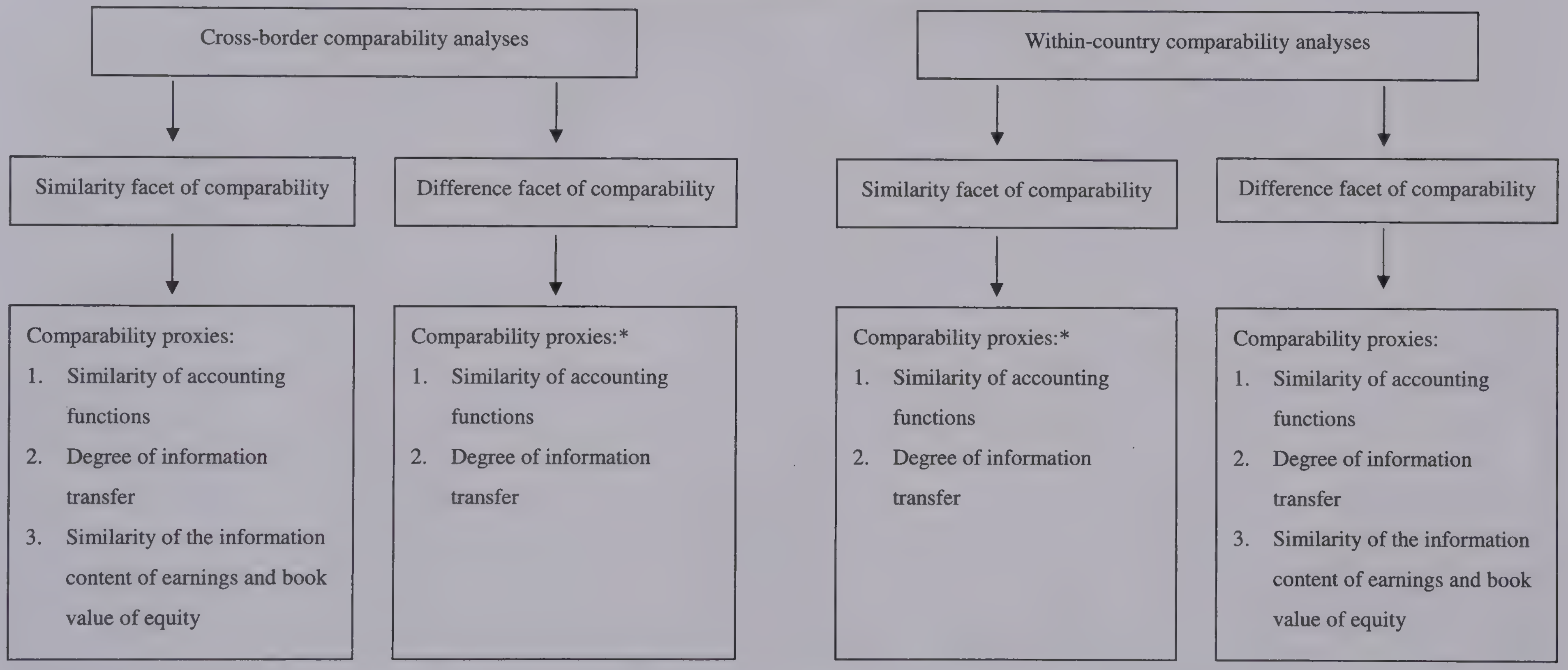
We perform multivariate Regression (4) for four samples: similar firms from different countries, different firms from different countries, similar firms from the same country, and different firms from the same country:

$$Comp_{it} = \beta_0 + \beta_1 IFRS_t + \beta_2 TA\_Ratio_i + \beta_3 Com\_Code_i + \beta_4 Listing_i + ID_i, \tag{4}$$

where $Comp_{it}$ is the comparability of pair i in period t, and $IFRS_t$ is an indicator that equals 1 if t is the post-IFRS period, and 0 otherwise. A significant β_1 indicates that information comparability changes between the pre- and post-IFRS periods. The variable TA_Ratio_i is the ratio of the smaller value of total assets to the larger value of the two firms in a pair in 2006. Com_Code_i is an indicator that equals 1 when the home countries of the two firms in the pair have different legal origins, and 0 otherwise, and $Listing_i$ is an indicator that equals 1 if the two firms in a pair are both listed on at least one of the same stock exchanges, and 0 otherwise. These variables control for differences in firm size, institutional environment, and stock listing between the two firms, all of which may affect firms' reporting incentives and, hence, the comparability of their accounting information. These variables are all winsorized at the top and bottom 1 percent. ID_i is an industry indicator of a pair. For within-country analysis, we exclude *Com_Code* and *Listing* because they take the same value for all of the observations. For difference facet of comparability analysis, ID_i is excluded because each of the pairs consists of one manufacturing firm and one service firm. Following the discussion and practices in prior studies (e.g., Gow et al. 2010; DeFond et al. 2011), we adjust the standard errors by country clusters in the within-country analyses.

Table 2, Panel A, presents descriptive statistics for the four samples. The mean and median of *Comp* between cross-country similar and different firms, within-country similar and different firms, cross- and within-country similar firms, and cross- and within-country different firms do not statistically differ except that the mean of cross-country similar firms is smaller than that of cross-

FIGURE 1
Summary of Empirical Analyses and Comparability Measures


Cross-border comparability analyses
Within-country comparability analyses
Similarity facet of comparability
Difference facet of comparability
Similarity facet of comparability
Difference facet of comparability
Comparability proxies:
1. Similarity of accounting functions
2. Degree of information transfer
3. Similarity of the information content of earnings and book value of equity
Comparability proxies:*
1. Similarity of accounting functions
2. Degree of information transfer
Comparability proxies:*
1. Similarity of accounting functions
2. Degree of information transfer
Comparability proxies:
1. Similarity of accounting functions
2. Degree of information transfer
3. Similarity of the information content of earnings and book value of equity

* The third proxy (similarity of the information content of earnings and book value of equity) is not applicable in this sample.

TABLE 2

Results of the Tests Using the Similarity of Accounting Functions Measure

Panel A: Descriptive Statistics for the Variables in the Various Samples

Variable	n	Mean	Median	Minimum	Maximum	Std. Dev.
Sample of Similar Firms from Different Countries						
$Comp_{it}$	266	−0.282	−0.038	−2.950	−0.002	1.138
$IFRS_t$	266	0.500	0.500	0.000	1.000	0.501
TA_Ratio_i	266	0.810	0.838	0.500	0.995	0.153
Com_Code_i	266	0.459	0.000	0.000	1.000	0.499
$Listing_i$	266	0.045	0.000	0.000	1.000	0.208
Sample of Different Firms from Different Countries						
$Comp_{it}$	296	−0.094	−0.030	−0.729	−0.002	0.071
$IFRS_t$	296	0.500	0.500	0.000	1.000	0.500
TA_Ratio_i	296	0.957	0.982	0.576	1.000	0.075
Com_Code_i	296	0.628	1.000	0.000	1.000	0.484
$Listing_i$	296	0.007	0.000	0.000	1.000	0.082
Sample of Similar Firms from the Same Country						
$Comp_{it}$	122	−0.142	−0.040	−1.157	−0.001	0.278
$IFRS_t$	122	0.500	0.500	0.000	1.000	0.502
TA_Ratio_i	122	0.794	0.812	0.501	0.986	0.155
Sample of Different Firms from the Same Country						
$Comp_{it}$	250	−0.112	−0.041	−0.696	−0.005	0.177
$IFRS_t$	250	0.500	0.500	0.000	1.000	0.500
TA_Ratio_i	250	0.904	0.943	0.551	1.000	0.102

Panel B: Regression Results

	Cross-Country Analysis		Within-Country Analysis		Pooled Sample Analysis	
Variables	Similar Firms	Different Firms	Similar Firms′	Different Firms	Cross-Country Similar and Different Firms	Cross- and Within-Country Similar Firms
Intercept	−0.680	−0.109	−0.446	−4.653	0.206	−0.192
	(−1.332)	(−1.248)	(−1.027)	(−1.313)	(0.656)	(−0.278)
$IFRS_t$	0.474***	0.009	0.655*	0.720	0.009	0.655***
	(2.698)	(0.969)	(1.924)	(1.347)	(0.117)	(3.274)
$Similar\_Firm_i$					−0.542***	
					(−4.186)	
$Cross\_Country_i$						−0.269
						(−0.649)
$IFRS_t * Similar\_Firm_i$					0.465***	
					(4.199)	
$IFRS_t * Cross\_Country_i$						−0.181
						(−0.749)
TA_Ratio_i	−0.470	0.055	0.132	2.849	−0.247	−0.280
	(−1.684)	(0.657)	(1.318)	(1.119)	(−0.785)	(−0.599)
Com_Code_i	−0.143*	0.007			−0.033	−0.208
	(−1.703)	(0.768)			(−0.494)	(−1.116)

(continued on next page)

TABLE 2 (continued)

Variables	Cross-Country Analysis		Within-Country Analysis		Pooled Sample Analysis	
	Similar Firms	**Different Firms**	**Similar Firms**	**Different Firms**	**Cross-Country Similar and Different Firms**	**Cross- and Within-Country Similar Firms**
$Listing_i$	−0.626 (−1.396)	−0.075*** (−18.680)			−0.483** (−2.271)	−0.611 (−1.575)
ID_i	Yes	No	Yes	No	Yes	Yes
Observations	266	296	122	250	562	388
Adj. R^2	0.201	0.006	0.341	0.073	0.317	0.275

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, two-sided.

This table presents the results of the tests using the similarity of accounting functions measure. Panel A presents the descriptive statistics for the variables used in the multivariate test. All of the variables are winsorized at the top and bottom 1 percent. Panel B presents the regression coefficients (t-statistics in parentheses). The base model is as follows:

$$Comp_{it} = \beta_0 + \beta_1 IFRS_t + \beta_2 TA\_Ratio_i + \beta_3 Com\_Code_i + \beta_4 Listing_i + ID_i.$$

For within-country analyses, *Com_Code* and *Listing* are excluded from the model because all of the observations equal 1, and the standard errors are adjusted by country clusters. For difference facet of comparability analysis, ID_i is excluded because each of the pairs consists of one manufacturing firm and one service firm. For analyses using a pooled sample of cross-country similar and different firms, the model includes an indicator variable, *Similar_Firm*, and its interaction with *IFRS*. For analyses using a pooled sample of cross- and within-country similar firms, the model includes an indicator variable, *Cross_Country*, and its interaction with *IFRS*.

Variable Definitions:
$Comp_{it}$ = information comparability between two firms in pair i in period t;
$IFRS_t$ = an indicator that equals 1 if t is a post-IFRS period, and 0 if t is a pre-IFRS period;
TA_Ratio_i = ratio of the smaller value of total assets to the larger value of the firms in a pair in 2006;
Com_Code_i = an indicator that equals 1 when the home countries of the two firms in a pair have different legal system origins, and 0 otherwise;
$Listing_i$ = an indicator that equals 1 if the two firms in a pair are listed on at least one of the same stock exchanges, and 0 otherwise;
ID_i = an industry indicator of a pair;
$Similar\_Firm_i$ = an indicator that equals 1 if *Comp* is the comparability between two similar firms, and 0 if it is between two different firms; and
$Cross\_Country_i$ = an indicator that equals 1 if *Comp* is the comparability between two similar firms from different countries, and 0 if it is between two similar firms from the same country.

country different firms (results are not tabulated).[16] Panel B reports the regression results. For the two cross-country analyses, the coefficient of *IFRS* (β_1) is positive and significant for the sample of similar firms (coefficient of 0.474, $p < 0.01$), but is close to 0 (0.009) and statistically insignificant for the sample of different firms.[17] We also compare the coefficient of *IFRS* between the samples of similar and different firms. We combine these two samples, and include an indicator for the observations from the sample of similar firms (*Similar_Firm*) and its interaction with *IFRS* in

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[16] Seven pairs of firms have extremely small comparability values, mainly in the pre-IFRS period. When these pairs are excluded from the sample of cross-country similar firms, the mean comparability of the sample becomes similar to those of the other samples. When we estimate Equation (4) using the reduced sample, the coefficient of *IFRS* remains positive and significant (coefficient of 0.119, $p < 0.000$), and significantly greater than that of the sample of cross-country different firms.

[17] We follow Milton (1986) to estimate a minimum sample size necessary to reach a t-value of 2.00 and 1.68 for the samples used in the tests of the similarity of accounting functions and information transfer measures where the result for the variable of interest is insignificant. We find that all of our sample sizes are greater than their corresponding minimum sample sizes based on a t-value of 2.00.

Equation (4). We find that the coefficient on the interaction is positive and significant (coefficient of 0.465, $p < 0.01$). These results, thus, suggest that there is a significant improvement in the similarity facet, but not in the difference facet of cross-country comparability in the post-IFRS period.

For the two within-country analyses, the coefficient on *IFRS* is 0.655 and marginally significant for similar firms, and 0.720 but insignificant for different firms. We combine the samples of cross- and within-country similar firms, and include an indicator for the observations from the cross-country sample (*Cross_Country*) and its interaction with *IFRS* in the model. We find that the coefficient on the interaction is insignificant, suggesting that the effect of IFRS adoption on the similarity facet of cross-country comparability is similar to that of within-country comparability.

Results Using the Information Transfer Measure

The regression model using the measure of information transfer is as follows:

$$\begin{aligned} |NAF\_CAR_{it}| = {} & \beta_0 + \beta_1|AF\_UE_{it}| + \beta_2 IFRS_t + \beta_3|AF\_UE_{it}|*IFRS_t + \beta_4 AF\_Size_{it} \\ & + \beta_5 AF\_Analyst_{it} + \beta_6 AF\_Loss_{it} + ID_i, \end{aligned} \tag{5}$$

where $|NAF\_CAR_{it}|$ is the average absolute value of the cumulative abnormal returns of all the non-announcing firms within the three-day earnings release window of an announcing firm, and $|AF\_UE_{it}|$ is the absolute value of the unexpected earnings per share of the announcing firm, scaled by its stock price at the beginning of the fiscal year.[18] We use the absolute value of these two variables because information transfer can be positive or negative. A positive transfer takes place when a positive (negative) earnings surprise for an announcing firm indicates an unexpected improvement (deterioration) in market conditions, which positively (negatively) affects the stock prices of similar firms. A negative information transfer occurs when a positive (negative) earnings surprise for an announcing firm represents an increased (decreased) market share for that firm, which negatively (positively) affects the stock prices of similar firms (Kim et al. 2008). The indicator $IFRS_t$ equals 1 if t is a post-IFRS year, and 0 if it is a pre-IFRS year. We include the logarithm of total assets in U.S. dollars of the announcing firm (AF_Size_{it}) in the model because previous studies find that the earnings surprises of larger firms affect the stock prices of similar firms more than those of smaller firms (e.g., Atiase 1985; Firth 1996).[19] The variable $AF\_Analyst_{it}$ is the number of one-year-ahead earnings forecasts issued and revised for the announcing firm, controlling for the intensity of analyst activity. In addition, we include AF_Loss_{it} in the model to indicate whether the announcing firm is reporting a loss, as prior studies find that losses are less informative than profits (e.g., Hayn 1995). The industry indicator ID_i is also included in the cross- and within-country analysis for similar firms. Further, we adjust standard errors by country clusters based on the announcing firm's country in all of the four analyses.

Our variable of interest is the interaction between the absolute value of the unexpected earnings of the announcing firm and the indicator for the post-IFRS period ($|AF\_UE_{it}| * IFRS_t$). The coefficient on this interaction (β_3) indicates whether information comparability differs across the pre- and post-IFRS periods. The descriptive statistics for the variables for the four samples are summarized in Table 3, Panel A. We winsorize all of the variables at the top and bottom 1 percent. As Panel A shows, the mean absolute values of the cumulative abnormal return of the non-announcing firms range from 0.023 to 0.028, and are all greater than zero.

Table 3, Panel B, reports the regression results. For cross-country analysis, the coefficient β_3 on the variable of interest ($|AF\_UE_{it}| * IFRS_t$) is positive and significant ($p < 0.05$) for the sample of

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[18] *NAF* and *AF* in the variable names denote Non-Announcing Firms and Announcing Firms, respectively.

[19] The Worldscope database provides accounting data in both the local currency and U.S. dollars.

TABLE 3

Results of the Tests Using the Information Transfer Measure

Panel A: Descriptive Statistics for the Variables in the Various Samples

	n	Mean	Median	Minimum	Maximum	Std. Dev.
Sample of Similar Firms from Different Countries						
$\lvert NAF\_CARit\rvert$	744	0.025	0.018	0.000	0.170	0.027
$\lvert AF\_UE_{it}\rvert$	744	0.237	0.016	0.000	2.420	0.996
$IFRS_t$	744	0.472	0.000	0.000	1.000	0.500
AF_Size_{it}	744	2.726	2.692	1.199	5.033	0.740
$AF\_Analyst_{it}$	744	6.742	5.000	1.000	30.000	6.439
AF_Loss_{it}	744	0.181	0.000	0.000	1.000	0.386
Sample of Different Firms from Different Countries						
$\lvert NAF\_CARit\rvert$	910	0.023	0.021	0.005	0.065	0.011
$\lvert AF\_UE_{it}\rvert$	910	0.201	0.016	0.000	2.232	0.505
$IFRS_t$	910	0.488	0.000	0.000	1.000	0.500
AF_Size_{it}	910	2.782	2.759	1.160	4.372	0.717
$AF\_Analyst_{it}$	910	7.158	5.000	1.000	32.000	6.717
AF_Loss_{it}	910	0.188	0.000	0.000	1.000	0.391
Sample of Similar Firms from the Same Country						
$\lvert NAF\_CARit\rvert$	195	0.028	0.019	0.000	0.128	0.029
$\lvert AF\_UE_{it}\rvert$	195	0.363	0.029	0.000	2.225	0.700
$IFRS_t$	195	0.485	0.000	0.000	1.000	0.501
AF_Size_{it}	195	2.696	2.629	0.890	4.512	0.825
$AF\_Analyst_{it}$	195	7.441	5.000	1.000	39.000	7.561
AF_Loss_{it}	195	0.170	0.000	0.000	1.000	0.377
Sample of Different Firms from the Same Country						
$\lvert NAF\_CARit\rvert$	630	0.025	0.020	0.001	0.106	0.019
$\lvert AF\_UE_{it}\rvert$	630	0.239	0.020	0.001	2.227	0.552
$IFRS_t$	630	0.508	1.000	0.000	1.000	0.500
AF_Size_{it}	630	2.681	2.684	1.085	4.194	0.702
$AF\_Analyst_{it}$	630	6.897	5.000	1.000	28.000	6.289
AF_Loss_{it}	630	0.205	0.000	0.000	1.000	0.404

Panel B: Regression Results

	Cross-Country Analysis		Within-Country Analysis		Pooled Sample Analysis	
Variables	Similar Firms	Different Firms	Similar Firms	Different Firms	Cross-Country Similar and Different Firms	Cross- and Within-Country Similar Firms
Intercept	0.010	0.038***	0.039	0.019*	0.036***	0.025***
	(0.272)	(11.200)	(1.027)	(1.668)	(19.560)	(3.697)
$\lvert AF\_UE_{it}\rvert$	0.000	0.000	0.000	0.000	0.000	−0.000
	(0.069)	(0.354)	(0.144)	(0.478)	(1.020)	(−0.289)
$IFRS_t$	−0.002	−0.004	−0.008	−0.000	−0.001	−0.004
	(−1.468)	(−1.385)	(−1.413)	(−0.252)	(−1.036)	(−0.759)
$Similar\_Firm_i$					0.001	
					(1.089)	

(continued on next page)

TABLE 3 (continued)

Variables	Cross-Country Analysis		Within-Country Analysis		Pooled Sample Analysis	
	Similar Firms	**Different Firms**	**Similar Firms**	**Different Firms**	**Cross-Country Similar and Different Firms**	**Cross- and Within-Country Similar Firms**
$Cross\_Country_i$						−0.003
						(−0.588)
$\lvert AF\_UE_{it}\rvert * IFRS_t$	0.001**	0.001	0.007	0.000	0.001***	0.003*
	(2.430)	(0.484)	(1.554)	(0.091)	(3.566)	(2.041)
$\lvert AF\_UE_{it}\rvert * Similar\_Firm_i$					−0.001	
					(−0.431)	
$\lvert AF\_UE_{it}\rvert * Cross\_Country_i$						0.002
						(1.334)
$IFRS_t * Similar\_Firm_i$					0.002	
					(1.439)	
$IFRS_t * Cross\_Country_i$						0.002
						(0.389)
$IFRS_t * \lvert AF\_UE_{it}\rvert * Similar\_Firm_i$					0.003**	
					(2.306)	
$IFRS_t * \lvert AF\_UE_{it}\rvert * Cross\_Country_i$						−0.001
						(−1.261)
AF_Size_{it}	0.001	−0.005***	−0.009	−0.003	−0.004***	−0.002
	(0.210)	(−8.658)	(−1.156)	(−1.497)	(−4.723)	(−0.644)
$AF\_Analyst_{it}$	−0.000	0.000	0.001	−0.000	−0.000	0.000
	(−0.492)	(0.681)	(0.954)	(−1.712)	(−0.018)	(0.189)
AF_Loss_{it}	−0.000	0.002*	0.003	0.002	0.001	0.002
	(−0.070)	(2.197)	(0.369)	(0.865)	(0.941)	(1.651)
ID_i	Yes	No	Yes	No	Yes	Yes
Observations	744	910	195	630	1,654	939
Adj. R^2	0.046	0.154	0.029	0.075	0.124	0.030

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, two-sided.

This table presents the results for the tests using the information transfer measure. Panel A presents the descriptive statistics for the variables in the multivariate test. All of the variables are winsorized at the top and bottom 1 percent. Panel B presents the regression coefficients (t-statistics in parentheses). The standard errors are clustered at the announcing firm's country level. The base model is as follows:

$$|NAF\_CAR_{it}| = \beta_0 + \beta_1|AF\_UE_{it}| + \beta_2 IFRS_t + \beta_3|AF\_UE_{it}|{*}IFRS_t + \beta_4 AF\_Size_{it} + \beta_5 AF\_Analyst_{it} + \beta_6 AF\_Loss_{it} + ID_i.$$

For analysis using a combined sample of similar and different cross-country firms, the model includes an indicator, *Similar_Firm*, and its two- and three-way interactions with *IFRS* and $|AF\_UE|$. For analysis using a pooled sample of cross- and within-country similar firms, the model includes an indicator, *Cross_Country*, and its two- and three-way interactions with *IFRS* and $|AF\_UE|$.

Variable Definitions:

$|NAF\_CAR_{it}|$ = average absolute value of the cumulative abnormal return of all of non-announcing firms within the three-day earnings release window of the announcing firm;

$|AF\_UE_{it}|$ = absolute value of the unexpected earnings per share of the announcing firm, scaled by its stock price at the beginning of the fiscal year;

$IFRS_t$ = an indicator that equals 1 if *t* is a post-IFRS year, and 0 if *t* is a pre-IFRS year;

AF_Size_{it} = size of the announcing firm, measured by the logarithm of total assets in U.S. dollars;

$AF\_Analyst_{it}$ = number of one-year-ahead earnings forecasts issued and revised for the announcing firm;

AF_Loss_{it} = an indicator of whether the announcing firm is reporting a loss;

ID_i = an industry indicator of a pair;

$Similar\_Firm_i$ = an indicator that equals 1 if the announcing and the non-announcing firms are similar firms, and 0 if they are different firms; and

$Cross\_Country_i$ = an indicator that equals 1 if the announcing and the non-announcing firms are from different countries, and 0 if they are from the same country.

similar firms with a coefficient of 0.001, but insignificant for the sample of different firms. Similar to the measure of accounting functions similarity, we pool the two samples and include an indicator for the observations from the sample of similar firms (*Similar_Firm*) and its interaction with $|AF\_UE_{it}| * IFRS_t$ in the model to test the equality of β_3 across the similar and different firms. We find that the coefficient on $IFRS_t * |AF\_UE_{it}| * Similar\_Firm_i$ has a value of 0.003 and is significant ($p < 0.05$), which suggests that IFRS adoption increases cross-country information transfer more for similar firms than for different firms.

For within-country analysis, the coefficient β_3 on $|AF\_UE_{it}| * IFRS_t$ is insignificant for both the sample of similar firms and the sample of different firms. However, the result from the test that examines the equality of β_3 between the cross-country and within-country similar firms shows that the coefficient on the variable of interest ($IFRS_t * |AF\_UE_{it}| * Cross\_Country_i$) is insignificant, suggesting that change in information transfer does not statistically differ between these two samples. Thus, the results using the information transfer measure are generally consistent with those using the similarity of accounting functions measure.

Results Using the Similarity of ICE and ICBV Measure

For the ICE and ICBV measures, we conduct tests for similar firms across countries and for different firms within the country. The descriptive statistics for the variables used in Equation (3) for these two samples are summarized in Table 4, Panel A. We winsorize all of the variables at the top and bottom 1 percent. Table 4, Panel B, presents the aggregate results from estimating Equation (3).

We perform a t-test to compare the mean comparability scores for ICE and ICBV across the pre- and post-IFRS periods. Table 4, Panel C, reports the results. For the sample of cross-country similar firms, the mean comparability scores for ICE and ICBV in the post-IFRS period are 0.890 and 0.862, respectively, indicating that among the 210 regressions of Equation (3), about 89.0 (86.2) percent show that the information content of earnings (book value of equity) does not statistically differ between the two sets of similar firms from two different countries. The 0.890 score for ICE is marginally greater than the corresponding pre-IFRS period score 0.829 ($p = 0.06$), and the 0.862 score for ICBV is significantly greater than the pre-IFRS period score of 0.748 ($p < 0.01$). This again suggests that IFRS adoption increases the similarity facet of cross-country information comparability. However, for the sample of different firms from the same country, the mean comparability scores for ICE and ICBV in the post-IFRS period (0.688 and 0.688) are not significantly different from the corresponding scores in the pre-IFRS period (0.594 and 0.625).

Summary of Results

Our comparability measures consistently indicate a significant improvement in the similarity facet, but an indiscernible change in the difference facet of cross-country information comparability after mandatory IFRS adoption. Further, the comparability improvement in the similarity facet is consistently greater than that in the difference facet. Together, these results suggest that IFRS adoption is, on the whole, beneficial, because it makes similar things look more alike without making different things look discernibly less different.

We also find some evidence of improvement in the similarity facet of within-country comparability. Importantly, our results show that there is no statistical difference between the similarity facet of comparability improvement in the cross-country sample and that in the within-country sample. Together, these results suggest that IFRS adoption improves information comparability among similar firms from different countries, as well as similar firms within the same country. As IFRS adoption generally reduces accounting choices for European firms (Hoogendoorn

TABLE 4

Results of the Tests Using the ICE and ICBV Measures

Panel A: Descriptive Statistics for the Variables in the Estimation of Equation (3)

Variable	n	Mean	Median	Minimum	Maximum	Std. Dev.
Sample of Similar Firms from Different Countries						
MV_{it}	1,798	28.849	6.939	0.070	531.913	73.171
NI_{it}	1,798	1.513	0.283	−12.690	39.300	5.719
BV_{it}	1,798	25.459	3.252	−4.699	802.606	94.491
Sample of Different Firms from the Same Country						
MV_{it}	1,259	30.123	6.853	0.071	625.803	82.706
NI_{it}	1,259	1.459	0.249	−15.144	45.312	6.370
BV_{it}	1,259	26.988	3.216	−5.475	851.320	103.926

Panel B: Aggregate Summary Table of Estimates

	Full Sample		Pre-IFRS Sample		Post-IFRS Sample	
Estimates	Mean	p-value	Mean	p-value	Mean	p-value
Estimates for the Sample of Similar Firms from Different Countries (n = 420)						
Intercept	10.327***	0.000	10.187***	0.000	10.467***	0.000
NI_{it}	3.313***	0.000	1.917***	0.000	4.709***	0.000
BV_{it}	0.690***	0.000	0.623***	0.000	0.757***	0.000
D^1	−6.657***	0.000	−6.445***	0.002	−6.868***	0.000
$D^1 * NI_{it}$	−0.000	0.970	0.000	0.366	−0.000**	0.016
$D^1 * BV_{it}$	−0.000	0.496	−0.000	0.196	0.000*	0.072
Estimates for the Sample of Different Firms from the Same Country (n = 128)						
Intercept	5.642**	0.035	4.289*	0.074	6.996	0.145
NI_{it}	2.384***	0.002	0.761	0.483	2.076***	0.000
BV_{it}	0.924***	0.002	0.897***	0.000	−0.172*	0.096
D^2	7.638***	0.004	7.686**	0.031	−0.338*	0.060
$D^2 * NI_{it}$	1.345*	0.077	2.664**	0.014	−2.100	0.981
$D^2 * BV_{it}$	−0.332	0.270	−0.460**	0.032	−1.332	0.718

Panel C: t-test Results

	Similar Firms from Different Countries	Different Firms from the Same Country
Mean Comparability Score for ICE		
Pre-IFRS	0.829	0.594
Post-IFRS	0.890	0.688
Difference	0.061*	0.094
t-statistic (two-tailed p-value) in a paired t-test	1.868 (0.063)	1.515 (0.135)
Observations	210	64
Mean Comparability Score for ICBV		
Pre-IFRS	0.748	0.625
Post-IFRS	0.862	0.688
Difference	0.114***	0.063
t-statistic (two-tailed p-value) in a paired t-test	3.412 (0.001)	0.942 (0.350)
Observations	210	64

(continued on next page)

TABLE 4 (continued)

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.
This table presents the results of the tests using the ICE and ICBV measure. Panel A presents the descriptive statistics for the variables in the estimation of the following equation:

$$MV_{it} = \beta_0 + \beta_1 NI_{it} + \beta_2 BV_{it} + \beta_3 D^x + \beta_4 D^x{*}NI_{it} + \beta_5 D^x{*}BV_{it}.$$

All of the variables are winsorized at the top and bottom 1 percent. The sample of cross-country similar firms comprises 1,798 firms, and the sample of within-country different firms comprises 1,259 firms. Panel B presents the aggregate summary table for the estimates, which includes the means and two-tailed p-values showing whether the means are significantly different from 0. Panel C presents the results for the t-test of the difference in ICE and ICBV across the pre- and post-IFRS period for the sample of cross-country similar firms (n = 210) and the sample of within-country different firms (n = 64), respectively. An ICE (ICBV) comparability score of 1 is assigned if β_4 (β_5) is insignificant (defined as a p-value of more than 5 percent), and 0 otherwise.

Variable Definitions:
MV_{it} = total market value of equity at the end of the fiscal year, scaled by the number of outstanding common shares;
BV_{it} = book value of equity, scaled by the number of outstanding common shares;
NI_{it} = net income, defined as earnings (revenues less operating costs, depreciation, interest, taxes, and other expenses) less preferred dividends, scaled by the number of outstanding common shares; and
D^x = a country indicator when x equals 1, and an industry sector indicator when x equals 2.

2006), our results are consistent with the view that IFRS adoption improves information comparability via both accounting convergence and higher quality information.

V. SUPPLEMENTAL AND SENSITIVITY TESTS

Supplemental Tests for the Impact of Institutional Environment on Information Comparability

These supplemental tests examine whether the impact of IFRS adoption on the similarity facet of cross-country information comparability is influenced by a firm's institutional environment. On one hand, the importance of institutional factors to a firm's reporting incentives suggests that the comparability improvement resulting from IFRS adoption will be greater among firms from countries with similar institutional environments than among firms from countries with different institutional environments (e.g., Ball et al. 2003; Ball and Shivakumar 2005; Burgstahler et al. 2006). On the other hand, because countries with similar institutional environments usually had similar local accounting standards in the pre-IFRS period, the comparability improvement may be smaller among firms from these countries. We, thus, view this as an empirical question.

We classify a firm's institutional environment according to the origins of the legal system in its home country. For the tests using the accounting function measure, we form two samples. The first (second) sample includes all pairs of two similar firms from countries with the same (different) legal origin(s). We drop the variable Com_Code_i from Equation (4), and estimate the modified model for the two samples. Our results, presented in Table 5, Panel A, show that cross-country information comparability increases after IFRS adoption for both samples. We also test the equality of comparability improvement across the two samples by pooling the two samples and including an indicator for the observations from the first sample and its interaction with *IFRS*. We find that the comparability improvement does not statistically differ between the two samples.[20]

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[20] Our results could be affected by the enforcement changes accompanied with IFRS adoption. For example, the EU passed the Transparency Directive in 2004 to establish disclosure requirements and facilitate IFRS compliance.

TABLE 5

Impact of Institutions on Information Comparability

Panel A: Accounting Function Measure by Legal System Origin

Variable	Same Origin of Legal System $Comp_{it}$	Different Origins of Legal System $Comp_{it}$
Intercept	1.846	−1.642***
	(1.441)	(−8.498)
$IFRS_t$	0.446***	0.507**
	(4.603)	(2.689)
TA_Ratio_i	−1.245	−0.269**
	(−1.143)	(−2.578)
$Listing_i$	−1.123	
	(−1.391)	
ID_i	Yes	Yes
Observations	144	122
Adj. R^2	0.116	0.274

Panel B: Information Transfer Measure by Legal System Origin

Variable	Same Origin of Legal System $\lvert NAF\_CAR_{it}\rvert$	Different Origins of Legal System $\lvert NAF\_CAR_{it}\rvert$
Intercept	0.027***	0.019*
	(5.637)	(1.673)
$\lvert AF\_UE_{it}\rvert$	0.104	0.002
	(1.620)	(0.894)
$IFRS_t$	−0.003	−0.000
	(−1.466)	(−0.092)
$\lvert AF\_UE_{it}\rvert * IFRS_t$	0.024**	−0.001
	(2.157)	(−0.449)
AF_Size_{it}	−0.002	−0.003
	(−1.449)	(−1.510)
$AF\_Analyst_{it}$	0.000	−0.000
	(0.363)	(−1.644)
AF_Loss_{it}	0.001	0.002
	(0.258)	(0.834)
ID_i	Yes	Yes
Observations	514	285
Adj. R^2	0.016	0.075

Panel C: ICE and ICBV Measure by Legal System Origin

Legal Origins	n	Pre-IFRS Mean	Post-IFRS Mean	Difference	t-statistics	p-value
t-test Results for the Difference in ICE Comparability Scores						
Same	155	0.794	0.858	0.064[a]	1.550	0.123
Different	55	0.709	0.782	0.073[a]	0.893	0.376
				0.009[b]	0.097	0.923

(continued on next page)

TABLE 5 (continued)

Legal Origins	n	Pre-IFRS Mean	Post-IFRS Mean	Difference	t-statistics	p-value
t-test Results for the Difference in ICBV Comparability Scores						
Same	155	0.645	0.755	0.110**[a]	2.580	0.011
Different	55	0.745	0.818	0.073[a]	1.071	0.289
				0.037[b]	0.450	0.653

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

[a] Represents a test of mean difference across the pre- and post-IFRS periods.

[b] Represents a test of mean difference between the first and second samples.

This table presents the results for the additional tests of the impact of firms' institutions on their information comparability. Panel A presents the coefficients (t-statistics in parentheses) for the tests using the accounting function measure, in which the first (second) sample includes all of the pairs of two similar firms from countries with the same (different) legal origin(s). The variables are defined in Table 2. Panel B presents the coefficients (t-statistics in parentheses) for the tests using the information transfer measure. In the first (second) sample, the announcing firm's earnings surprise is paired with the average cumulative abnormal return of similar firms from countries with the same (different) legal origin(s) to the home country of the announcing firm. The variables are defined in Table 3. Panel C presents the results of tests using the ICE and ICBV measure, in which the first (second) sample includes all of the comparability scores estimated using two sets of firms from countries with the same (different) legal origin(s). The variables are defined in Table 4.

For the information transfer measure, we pair an announcing firm's earnings surprise with the average cumulative abnormal return of similar firms from countries with the same (different) legal origin(s) to the home country of the announcing firm in the first (second) sample. We find that the cross-country information transfer increases significantly in the post-IFRS period for the first sample, but not for the second sample. These results are reported in Table 5, Panel B.

For the ICE and ICBV measure, the first (second) sample includes all of the comparability scores estimated using the two sets of firms from countries with the same (different) legal origin(s). We compare the mean comparability scores across the pre- and post-IFRS periods in the two samples. We find that for ICE, there is no significant change in the mean comparability score across the two periods. For ICBV, however, the mean comparability score is significantly higher in the post-IFRS period than in the pre-IFRS period for the first sample with the same legal system origin, but not for the second sample. Table 5, Panel C, reports these results.

Overall, we find consistent evidence for enhanced cross-country information comparability among firms from countries with similar institutional environments. However, the results for firms from different institutional environments are mixed. Although the results using the measure of accounting function similarity suggest an improved comparability, the results using the other two measures suggest the opposite. This provides some evidence that the institutional environment in the home countries of firms is an important determinant of their information comparability.

Sensitivity Tests

We perform several sensitivity tests to assess the robustness of our results. First, because unusual accounting choices could be reflected in financial statements due to the anticipation of, and the transition to, IFRS during 2004 and 2005, we exclude these two years by defining the pre-IFRS period as 2002–2003 and the post–IFRS period as 2006–2007. The untabulated results based on the new sample periods are similar to those of the main tests.

Second, our results on the comparability improvement may be driven by increased similarity of firms' responses to economic shocks. To address this possibility, we examine whether there is a significant increase in firms' responses to economic shocks. Specifically, we compute the stock return correlation between two sets of similar firms from different countries for the pre- and post-IFRS periods separately. Because firms in the same industry face similar economic shocks, their stock returns should be correlated if they react to economic shocks in similar ways. A significantly higher mean correlation of stock return in the post-IFRS period than in the pre-IFRS period would suggest that there is an increase in the similarity of firms' responses to economic shock after mandatory IFRS adoption. We find that the mean correlation is significantly lower in the post-IFRS period (0.134) than in the pre-IFRS period (0.314), suggesting that firms' responses to economic shocks become less similar in the post-IFRS period. This, in turn, suggests that the similarity of firms' responses to economic shocks is unlikely to be the driver of improved information comparability in the post-IFRS period.

Third, because level regressions can be subject to econometric problems, we use a returns model to compare ICE across the pre- and post-IFRS periods. In our return model, annual stock returns are regressed on earnings scaled by price, a loss indicator, a country indicator, and the interaction between earnings and the country indicator. We run the regression for every possible combination of two countries in an industry in the pre- and post-IFRS periods separately, and assign a comparability score of 1 if the coefficient on the interaction term is insignificant, and 0 otherwise. We find that the mean comparability score in the post-IFRS period (0.990) is significantly higher than that in the pre-IFRS period (0.945), which is consistent with the finding from the level model that IFRS adoption improves the similarity facet of cross-country information comparability.

Fourth, because all of our comparability measures are based on equity price, we use a non-price-based comparability measure to examine the similarity facet of cross-country information comparability. Following Beuselinck et al. (2007), we assume that earnings comparability is affected by the timely recognition of losses by a firm's accounting system. We use the following modified model from Ball and Shivakumar (2005) to determine the comparability scores for every possible combination of two countries in an industry in the pre- and post-IFRS periods separately:

$$
\begin{aligned}
ACC_{jt} = {} & \beta_0 + \beta_1 CF_{jt} + \beta_2 NegCF_{jt} + \beta_3 CF_{jt}{*}NegCF_{jt} + \beta_4 D + \beta_5 D{*}CF_{jt} \\
& + \beta_6 D{*}NegCF_{jt} + \beta_7 D{*}CF_{jt}{*}NegCF_{jt} + \varepsilon_{jt}, \qquad (6)
\end{aligned}
$$

where ACC_t is accruals, defined as net income minus operating cash flow, and CF_t is operating cash flow. Both are scaled by the average total assets. $NegCF_t$ is an indicator of a negative CF, and D is a country indicator. The coefficient on the last item (β_7) indicates whether the accounting systems in the two countries recognize losses in accounting accruals in a similar fashion. We assign a comparability score of 1 if β_7 is insignificant, and 0 otherwise. We find that the mean comparability score in the post-IFRS period (0.861) is significantly higher than that in the pre-IFRS period (0.764). This result is consistent with the findings from those equity price-based comparability measures that cross-country information comparability improves for similar firms after mandatory IFRS adoption.

VI. CONCLUSION

One of the most commonly mentioned benefits of IFRS adoption is the improvement in cross-country information comparability. This study provides empirical evidence for this posited benefit using data from 17 European countries that adopted IFRS in 2005. We use three proxies for information comparability. The first is the similarity with which two firms translate economic events into their financial statements. The second is the degree of information transfer, and the third is the similarity of the information content of earnings and of the book value of equity.

Overall, our results are consistent with the view that mandatory IFRS adoption improves information comparability across countries. In particular, our results indicate a significant increase in the similarity facet of cross-country comparability in the post-IFRS period. However, we do not find such improvement in the difference facet of cross-country comparability, which implies that the mandatory IFRS adoption is not sufficient to achieve a full enhancement in comparability in the EU. Our results also suggest that both accounting convergence and higher quality accounting information are likely to be the mechanisms underlying the observed comparability improvement in the similarity facet. Further, our results suggest that comparability improvement is more likely to occur among firms from similar institutional environments than among firms from different institutional environments.

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The Effect of *Ex Ante* Management Forecast Accuracy on the Post-Earnings-Announcement Drift

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ABSTRACT: I examine the effect of *ex ante* management forecast accuracy on the post-earnings-announcement drift when management forecasts about next quarter's earnings are bundled with current quarter's earnings announcements. I build a composite measure of *ex ante* management forecast accuracy that takes into account forecast ability, forecast difficulty, and forecast environment. The results show that the bundled forecasts with higher *ex ante* accuracy mitigate investors' under-reaction to current earnings and reduce the magnitude of the post-earnings-announcement drift.

Keywords: *post-earnings-announcement drift; bundled management forecast; ex ante forecast accuracy.*

Data Availability: *The data used in this paper are available from the sources listed in the text.*

I. INTRODUCTION

The post-earnings-announcement drift is one of the most intriguing market anomalies. Following earnings announcements, stock prices move in the same direction as that of earnings surprises for the subsequent 6 to 12 months, and the magnitude of the drift also increases with the magnitude of earnings news. Two main explanations have been advanced for this anomaly: a failure to adjust abnormal returns for risk, and a delayed response to earnings reports (Bernard and Thomas 1989). Existing evidence is more consistent with the market under-reaction explanation: investors underestimate the implications of current earnings for future earnings, and their under-reaction is corrected at future earnings announcement dates.[1]

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[1] See Bernard and Thomas (1989, 1990), Ball and Bartov (1996), Livnat and Mendenhall (2006), and Shivakumar (2006), among others.

In this paper, I examine a prediction of the under-reaction explanation by investigating the effect of bundled management forecasts and their *ex ante* (perceived) accuracy on the post-earnings-announcement drift. Bundled management forecasts refer to forecasts of future earnings that are issued within one trading day of the current earnings announcement date, to yield a post-earnings-announcement drift window consistent with that in previous literature (Livnat and Mendenhall 2006).[2] Bundled management forecasts have become more prevalent recently, increasing from 10.7 percent of all management forecasts in 1996 to approximately 80 percent in 2007 (Rogers and Van Buskirk 2011). I focus on bundled management forecasts and their *ex ante* accuracy because they are pertinent to investigating whether management forecasts of future earnings, when issued together with current earnings announcements, help investors form expectations of future earnings promptly and mitigate the under-reaction to current earnings news. Other management forecasts, such as the forecasts issued two or more days after the current quarter earnings announcements, do not provide as timely information to accelerate investors' reaction to current earnings news.[3]

I hypothesize that investors' response to bundled management forecasts depends on the *ex ante* forecast accuracy, which is distinguished from actual forecast accuracy that is revealed at future earnings announcements. If investors perceive that the bundled management forecasts lack accuracy, then they place less weight on the forecasts, and continue to extrapolate future earnings based on their own information sets. Only when these bundled forecasts have high perceived accuracy do investors incorporate the bundled management forecasts into their expectations of future earnings, thereby possibly mitigating the post-earnings-announcement drift.

This paper is closely related to two recent studies by Li and Tse (2008) and Wang (2008), who examine a similar research question. However, Li and Tse (2008) and Wang (2008) do not demonstrate that investors' response to the bundled management forecasts depends on forecasts' perceived accuracy. In contrast, these two papers examine the effect of the subsequent actual forecast accuracy on the post-earnings-announcement drift, although investors do not know the actual forecast accuracy when the bundled management forecasts are issued.

Further, I investigate the interaction between *ex ante* accuracy and actual accuracy of the bundled forecasts in mitigating the post-earnings-announcement drift, which has not been addressed by prior literature or the contemporaneous papers. Specifically, if bundled forecasts have high *ex ante* accuracy but turn out to have low actual accuracy, then investors' expectations of future earnings are not improved by these forecasts; hence the post-earnings-announcement drift may not be mitigated.

In this paper, I construct a forecast accuracy prediction model that is based on the relation between actual forecast accuracy and forecast properties. The results suggest that a management forecast is likely to have higher actual accuracy if the prior forecast accuracy is higher, if the forecast horizon is shorter, if the forecast difficulty is lower, if the forecast news is less extreme, if the prior stock return is higher, or if the firm's market-to-book ratio is higher. For each management forecast, I measure the *ex ante* forecast accuracy using the estimated relationship and the current forecast properties.[4] Compared with other *ex ante* accuracy measures used in the prior literature (the prior forecast accuracy, or the previous four quarters' average forecast accuracy), this estimated *ex ante* forecast accuracy measure is more significantly associated with the actual forecast accuracy.

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[2] This approach is similar in spirit to that employed by Rogers and Van Buskirk (2011), in which bundled management forecasts refer to the forecasts issued within two days of the earnings announcement date.

[3] If management forecasts of future earnings are issued before, not along with, current earnings announcements, then they may help investors understand the implications of current earnings for future earnings as well. I discuss these forecasts in the robustness tests.

[4] To avoid look-ahead bias, the estimated relationship is based on the data available within the four quarters before the management forecast is announced. The estimation is also conducted separately for annual forecasts and quarterly forecasts.

From the second quarter of 1997 to the second quarter of 2007, 10,641 quarterly earnings announcements are bundled with the management forecasts of next quarter's earnings, in a sample of 72,851 quarterly earnings announcements.[5] Of these bundled management forecasts, 6,267 are perceived to be more accurate (i.e., the estimated accuracy is higher than the median).[6] Firms that have bundled forecasts with higher *ex ante* accuracy tend to be significantly larger and have smaller analyst forecast dispersion, more analyst following, higher institutional shareholding, higher trading volume in the past, higher stock price, lower earnings persistence, less negative earnings surprises, and more responsive analysts than firms that have bundled forecasts with lower *ex ante* accuracy.

The main empirical results suggest that bundled management forecasts, on average, do not mitigate the post-earnings-announcement drift when other drift-related variables are controlled. Consistent with the prediction, the bundled management forecasts reduce the magnitude of the post-earnings-announcement drift when they have higher *ex ante* forecast accuracy. The results hold after a battery of robustness checks. I also use a Heckman two-stage approach to solve the self-selection bias of issuing bundled management forecasts, and the inferences remain unchanged. Further, I investigate whether the effect of *ex ante* forecast accuracy on the drift varies with the actual forecast accuracy. This additional analysis shows that the post-earnings-announcement drift is not mitigated if the bundled forecasts have higher *ex ante* accuracy but lower actual accuracy.

Despite prior research on the existence and causes of the post-earning-announcement drift, there is limited evidence about the effect of voluntary disclosure on the drift. By demonstrating the mitigating effect of bundled forecasts with higher *ex ante* accuracy on the drift, this paper adds to the evidence supporting the under-reaction explanation for the post-earnings-announcement drift. This paper also adds to the evidence about the interaction between mandatory reporting and voluntary disclosure, which has not been adequately investigated in the prior literature (Beyer et al. 2010).

Previous literature (Rogers and Stocken 2005) shows that the market filters out the predicted forecast error in the short event window, especially for good news management forecasts. This paper extends the literature by analyzing how the estimated forecast accuracy affects investors' ability to interpret the implications of current earnings for future earnings in the intertemporal setting. Consistent with the argument in Verrecchia (1980) that the rapidity of price reaction to the information varies with investors' consensus judgment of the information's accuracy, I show that investors respond promptly to bundled management forecasts that have higher *ex ante* accuracy.

This paper distinguishes between *ex ante* and actual management forecast accuracy in affecting the post-earnings-announcement drift. In contrast to Li and Tse (2008) and Wang (2008), I investigate the crucial role performed by *ex ante* forecast accuracy in providing incremental information to resolve the uncertainty related to future earnings. I also provide further evidence that the mitigating effect of the *ex ante* forecast accuracy varies with the actual forecast accuracy, which accentuates the interaction between these two forecast accuracy measures.

Finally, Graham et al. (2005) find that managers make voluntary disclosures primarily to develop a transparent reporting reputation and to increase the predictability of future earnings. In this paper's context, managers may be motivated to build a transparent information environment by issuing bundled management forecasts. However, for the announced forecasts to be promptly reflected in the stock price and to resolve the uncertainty related to future earnings, managers need to promote the perceived forecast accuracy.

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[5] The main analysis focuses on the bundled management forecasts that are about next quarter's earnings. Compared with the bundled management forecasts of two-quarter-ahead (and beyond) earnings, these forecasts are most likely to mitigate investors' under-reaction to current earnings.

[6] The estimated accuracy of bundled management forecasts is benchmarked with the median accuracy of all management forecasts, including bundled management forecasts and unbundled management forecasts. Therefore, more than half (10,521 × 1/2) of the bundled management forecasts are perceived to be more accurate.

The rest of the paper proceeds as follows. Section II reviews the literature and develops the hypothesis. Section III discusses the sample, and constructs the *ex ante* forecast accuracy measure. Section IV presents the empirical results. Section V concludes.

II. LITERATURE REVIEW AND HYPOTHESIS DEVELOPMENT

Voluntary disclosure, especially forecasts of future performance, is not subject to the restrictions of accounting standards, and managers can use such disclosure to communicate their private information to investors. Since the enactment of Regulation Fair Disclosure, it has become increasingly common for firms to announce current earnings and management forecasts about future earnings simultaneously, partly because of the greater use of earnings-related conference calls along with or close to earnings announcements (Rogers and Van Buskirk 2011).

The under-reaction explanation for the post-earnings-announcement drift suggests that the speed with which investors incorporate the implications of current earnings into their expectations of future earnings is associated with the magnitude of the drift (Bernard and Thomas 1990; Ball and Bartov 1996; Soffer and Lys 1999; Livnat and Mendenhall 2006; Shivakumar 2006; Zhang 2008). If managers issue their forecasts of future earnings along with the current earnings announcements, then investors have more information with which to resolve the uncertainty related to the future periods' earnings. Hence, the post-earnings-announcement drift may be affected.

Even though investors can verify management forecasts by comparing them to subsequently announced actual earnings, not all management forecasts are accurate, i.e., close to actual earnings, because of managers' intentional bias or forecast difficulty (Lev and Penman 1990; Rogers and Stocken 2005). For example, Hutton and Stocken (2009) find that only about half of the management forecasts are more accurate than the median analyst forecasts. Previous literature documents that investors are likely to estimate the accuracy of the management forecasts and incorporate it into their reaction to the forecast news. Rogers and Stocken (2005) investigate the market's response to the predicted forecast errors and find that, for good news management forecasts, the market's response varies with the predicted forecast errors. Ng et al. (2010) find that forecast credibility mitigates investors' under-reaction to management forecast news. They use various forecast credibility measures, including prior forecast accuracy, firm-level litigation risk, industry concentration, and research and development intensity.

In this paper, I argue that investors incorporate the bundled forecasts into their expectations of future earnings when the forecasts have high *ex ante* accuracy. *Ex ante* accuracy is the investors' perception of the forecast accuracy *before* the related actual earnings are announced, and it is different from actual accuracy that is revealed *when* the related actual earnings are announced. Because investors do not know the actual accuracy when the bundled forecasts are issued, they should only rely on the *ex ante* accuracy to determine whether the bundled forecasts help resolve the uncertainty related to future earnings. If investors perceive the bundled forecasts to be accurate, then they incorporate these forecasts into their expectations of future earnings and accelerate their reaction to current earnings news.

If some bundled forecasts have high *ex ante* accuracy, but turn out to have low actual accuracy, then investors' expectations of future earnings are not improved. To the extent that bundled forecasts provide incremental information to accelerate investors' reaction to current earnings and reduce the magnitude of the post-earnings-announcement drift, I expect the drift to continue for such forecasts. The main objective of this paper is to examine the effect of *ex ante* forecast accuracy on the post-earnings-announcement drift.[7] Further, I investigate whether the mitigating effect of the *ex ante* accuracy varies with the actual accuracy.

A concurrent study by Li and Tse (2008) finds that bundled management forecasts in general mitigate the post-earnings-announcement drift, which is different from this paper's prediction and from the results shown in Section IV. Another contemporaneous paper by Wang (2008) also finds that earnings announcements with previously issued management forecasts have lower magnitude of the post-earnings-announcement drift.[8] Neither Li and Tse (2008) nor Wang (2008) demonstrates the crucial role *ex ante* management forecast accuracy performs, or explores the interaction between *ex ante* forecast accuracy and actual forecast accuracy in mitigating the post-earnings-announcement drift.

In a similar vein, Zhang (2008) finds that responsive analysts accelerate market reaction to earnings announcements, thereby mitigating the post-earnings-announcement drift.[9] She also finds that when earnings announcements are bundled with conference calls or management forecasts, analysts are more likely to be responsive. Compared with Zhang (2008), this paper focuses directly on the bundled management forecasts of next quarter's earnings, and incorporates their *ex ante* accuracy into the analysis.[10]

III. SAMPLE SELECTION, RESEARCH METHOD, AND DESCRIPTIVE STATISTICS

Sample Selection

I obtain management forecasts announced from 1995 to 2007 from First Call's Company Issued Guidance database. Because explicit forecast numbers are necessary for the calculation of the forecast accuracy, I include only point estimates and range estimates in the analysis of the association between actual forecast accuracy and forecast properties. Managers might pre-announce earnings after the corresponding fiscal period ends, and I exclude these pre-announcements from the management forecast sample.[11] Because management forecast error is deflated by the average stock price one week before the issuance of the management forecast, I delete the observations that do not have corresponding stock prices, or that have stock prices smaller than $1. I also exclude financial firms and firms whose shares are not traded on NYSE/AMEX/NASDAQ, or that have non-ordinary shares.

I obtain firms' actual earnings per share (EPS) data over the period 1997–2007 from the I/B/E/S database.[12] Next, I cross-check the earnings announcement dates in I/B/E/S with those in First Call, and delete the observations that have different earnings announcement dates.[13] Analyst

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[7] This paper is a joint test of whether investors use all of the available forecast properties to estimate the forecast accuracy and whether they incorporate the forecasts with high estimated accuracy into their reaction to current earnings news.

[8] Footnote 24 discusses the potential reasons for the difference of the results.

[9] Zhang (2008) defines a responsive analyst as one who revises the forecast of next quarter's earnings within two trading days after the current quarter earnings announcement.

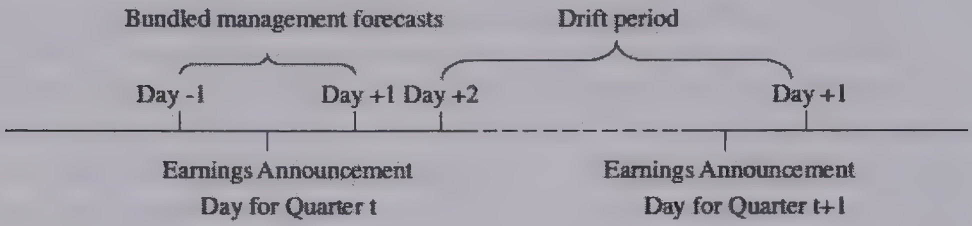
[10] Using a sample of 518 firms that initiated conference calls between 1994 and 2000, Kimbrough (2005) shows that the initiation of conference calls leads investors to respond in a more timely fashion to current earnings surprises. Initiation of conference calls is a one-off event in a firm's life, which says very little about how bundled management forecasts affect the post-earnings-announcement drift on a regular basis.

[11] The untabulated analysis shows that the actual accuracy of earnings preannouncements is significantly higher than that of ordinary management forecasts.

[12] This time range is selected because the *ex ante* forecast accuracy can be estimated during this period. Specifically, it is from the second quarter of 1997 to the second quarter of 2007.

[13] This paper uses the earnings numbers in I/B/E/S rather than Compustat because earnings in Compustat are restated and, hence, are not the actual earnings that investors observe and react to. See Livnat and Mendenhall (2006) for a comparison between I/B/E/S and Compustat.

FIGURE 1
Timeline for Measurement of Variables


Bundled management forecasts
Drift period
Day -1
Day +1
Day +2
Day +1
Earnings Announcement Day for Quarter t
Earnings Announcement Day for Quarter t+1

If the management forecast of Quarter $t+1$'s earnings is issued within one trading day around the earnings announcement date of Quarter t, then it is treated as the bundled management forecast. The post-earnings-announcement drift period starts two trading days after the earnings announcement date of Quarter t and ends on the first trading day after the earnings announcement date of Quarter $t+1$.

forecasts are non-split-adjusted forecasts from I/B/E/S. The most recent consensus analyst forecast used to calculate the unexpected earnings is the median of the analyst forecasts issued within 90 days before quarterly earnings announcements.[14]

Figure 1 shows the timeline for measurement of variables. The earnings announcement window is from trading day −1 to trading day +1 around the earnings announcement date (trading day 0) of Quarter t. Consistent with the prior literature (Livnat and Mendenhall 2006), the drift window starts two trading days after the earnings announcement date of Quarter t and ends on the first trading day after the earnings announcement date of Quarter $t+1$. The main analysis focuses on the management forecasts of the next quarter's earnings issued within the earnings announcement window, and investigates their *ex ante* accuracy on the drift period return.[15]

Measurement of *Ex Ante* Management Forecast Accuracy

I propose that investors base their estimate of *ex ante* management forecast accuracy on a variety of forecast properties. Rogers and Stocken (2005) develop a management forecast error prediction model that identifies the following explanatory variables: forecast horizon, forecast difficulty, forecast news, previous stock return, litigation risk, industry concentration, financial distress, market value to book value of equity ratio, and firm size. This prediction model in Rogers and Stocken (2005), specifically, is about forecast bias, while forecast accuracy is subject to not only the effects of intentional forecast bias, but also forecast difficulty and forecast ability. When firms' operating environments are more volatile, their earnings-generating processes are subject to multiple contingencies. Although managers may truthfully reveal the private information through management forecasts, these forecasts are still subject to random forecast errors, owing to high forecasting difficulty. The actual accuracy of firms' prior forecasts is a proxy for firm-level management forecasting ability, which influences investors' perceptions of current forecast accuracy (Hutton and Stocken 2009). I also incorporate the differential accuracy levels of bundled versus unbundled management forecasts (Rogers and Van Buskirk 2011).

The following model is used to estimate the association between the actual management forecast accuracy and the preceding forecast properties:

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[14] The results are similar if I use the latest individual analyst forecast to calculate the unexpected earnings, following Zhang (2008).

[15] I also include the annual forecasts that have the next quarter ends as the forecast period end dates.

$$\begin{aligned} ActualAccuracy_{i,t} = {} & \alpha_0 + \alpha_1 PriorAccuracy_{i,t} + \alpha_2 ForecastHorizon_{i,t} + \alpha_3 ForecastDifficulty_{i,t} \\ & + \alpha_4 ForecastNews_{i,t} \times GoodNews_{i,t} + \alpha_5 ForecastNews_{i,t} \times BadNews_{i,t} \\ & + \alpha_6 CAR_{i,t} + \alpha_7 Litigation_{i,t} + \alpha_8 Concentration_{i,t-1} + \alpha_9 Distress_{i,t-1} \\ & + \alpha_{10} MB_{i,t-1} + \alpha_{11} Size_{i,t-1} + \alpha_{12} Bundle_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{1}$$

where the variables are defined as follows:

Actual forecast accuracy (*ActualAccuracy*): Following Ng et al. (2010), management forecast accuracy is calculated as:

$$ActualAccuracy = -1 \times ABS\left(\frac{ActualEarnings - ManagementForecast}{Price}\right)$$

where *Price* refers to the average stock price one week before the management forecast announcement date. A management forecast is more accurate if *ActualAccuracy* is higher (closer to 0);

Prior forecast accuracy (*PriorAccuracy*) refers to the actual accuracy of the prior management forecast. In order for investors to know the actual accuracy of the prior forecasts, the actual earnings related to the forecasts should be announced before the current management forecast announcement date;

Forecast horizon (*ForecastHorizon*) is calculated as the management forecast period end date minus the announcement date, deflated by 360;

Forecast difficulty (*ForecastDifficulty*) is developed by performing Principal Axis Factoring over analyst forecast dispersion, the standard deviation of previous analyst forecast errors, firm's prior performance, future performance forecasted by the management, stock return volatility, bid-ask spread, and forecast width (see Rogers and Stocken 2005);

Forecast news (*ForecastNews*) is calculated as the management forecast minus the consensus analyst forecast, deflated by *Price*. *GoodNews* is an indicator variable that equals 1 if *ForecastNews* is non-negative, and 0 otherwise. *BadNews* is an indicator variable that equals 1 if *ForecastNews* is negative, and 0 otherwise; and

Other control variables: *CAR* is the firm's abnormal stock return cumulated from day −120 to day −1 relative to the management forecast announcement day. *Litigation* is an indicator variable that equals 1 if the firm is in industries with high litigation risk (Standard Industrial Classification codes 2833–2836, 3570–3577, 3600–3674, 5200–5961, 7370–7374, 8731–8734), and 0 otherwise. *Concentration* is measured by the Herfindahl index using the revenues of firms sharing the same four-digit SIC code. *Distress* is an indicator variable that equals 1 if the firm is in the most distressed decile predicted by Ohlson's (1980) bankruptcy model, and 0 otherwise. *MB* is the firm's market value of equity deflated by the book value of equity. *Size* is the natural log of the firm's total assets. *Concentration, Distress, MB*, and *Size* are calculated using the most recent accounting data prior to the announcement of the management forecast. *Bundle* is an indicator variable that equals 1 if a management forecast is issued within one trading day around the earnings announcement date, and 0 otherwise.

I estimate Model (1) separately for 21,790 quarterly forecasts and 20,701 annual forecasts issued from year 1995 to year 2007. The results are presented in Panel A of Table 1. Actual forecast accuracy is positively related to prior management forecast accuracy. Actual forecast accuracy is also higher when the forecast horizon is shorter. The significantly negative coefficient on *ForecastDifficulty* suggests that this latent variable effectively captures the difficulty that managers face in accurately forecasting future earnings. The coefficients on *ForecastNews* × *GoodNews* and *ForecastNews* × *BadNews* indicate that actual forecast accuracy is negatively associated with the

TABLE 1

Measurement of *Ex Ante* Management Forecast Accuracy

Panel A: Test of the Association between Actual Management Forecast Accuracy and Forecast Properties

	Quarterly Management Forecasts	Annual Management Forecasts
PriorAccuracy (α_1)	0.181***	0.114***
	(9.35)	(5.22)
ForecastHorizon (α_2)	−0.007***	−0.013***
	(−12.26)	(−23.02)
ForecastDifficulty (α_3)	−0.002***	−0.005***
	(−11.33)	(−10.07)
ForecastNews × *GoodNews* (α_4)	−0.997***	−0.935***
	(−16.75)	(−10.66)
ForecastNews × *BadNews* (α_5)	0.330***	0.441***
	(15.20)	(8.28)
CAR (α_6)	0.0004***	0.003***
	(3.86)	(7.26)
Litigation (α_7)	0.0001	−0.0002
	(0.65)	(−0.26)
Concentration (α_8)	−0.0002	−0.001
	(−0.76)	(−1.26)
Distress (α_9)	−0.002*	−0.001
	(−1.85)	(−0.27)
MB (α_{10})	0.0001***	0.0004***
	(6.09)	(5.49)
Size (α_{11})	−0.0001**	0.0003*
	(−1.97)	(1.68)
Bundle (α_{12})	0.0001	0.001***
	(1.22)	(4.51)
Adjusted R^2	47.57%	45.66%
Year fixed-effect	Yes	Yes
Industry fixed-effect	Yes	Yes
Number of observations	21,790	20,701

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*, **, *** Indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

All continuous variables are winsorized at the 1 percent and 99 percent levels.

t-statistics are in parentheses, and are based on standard errors clustered on the firm level.

This table presents results from the regression of actual management forecast accuracy (*ActualAccuracy*) on forecast properties. The management forecasts and the related actual earnings are announced from year 1995 to year 2007. The actual forecast accuracy is calculated as: *ActualAccuracy* = −1 × *ABS(ActualEarnings* − *ManagementForecast)/Price*. *Price* is the average stock price one week before the management forecast announcement date. *PriorAccuracy* is the actual accuracy of the prior management forecast. The actual earnings related to the prior forecast should be announced before current management forecast announcement day. *ForecastHorizon* is the current management forecast horizon, calculated as the management forecast period end date minus the announcement date, deflated by 360. *ForecastDifficulty* is developed by performing Principal Axis Factoring over analyst forecast dispersion, standard deviation of previous analyst forecast errors, firm performance, predicted loss, stock return volatility, bid-ask spread, and forecast width, as in Rogers and Stocken (2005). *ForecastNews* is calculated as the management forecast minus the consensus analyst forecast, deflated by *Price*. *GoodNews* is an indicator variable that equals 1 if *ForecastNews* is non-negative, and 0 otherwise. *BadNews* is an indicator variable that equals 1 if *ForecastNews* is negative, and 0 otherwise. *CAR* is the firm's abnormal stock return cumulated from day −120 to day −1 relative to the management forecast announcement day. *Litigation* is an indicator variable that equals 1 if the firm is in industries with high litigation risk (Standard Industrial Classification codes 2833–2836, 3570–3577, 3600–3674, 5200–5961, 7370–7374, 8731–8734), and 0 otherwise. *Concentration* is measured by the Herfindahl index using the revenues of firms sharing the same four-digit SIC code. *Distress* is an indicator variable that equals 1 if the firm is in the most distressed decile predicted by the Ohlson bankruptcy model, and 0 otherwise. *MB* is the firm's market value of equity deflated by book value of equity. *Size* is the natural log of the firm's total assets. *Bundle* is an indicator variable that equals 1 if the management forecast is issued along with the earnings announcement, and 0 otherwise.

(continued on next page)

TABLE 1 (continued)

Panel B: Test of the Association between Actual Management Forecast Accuracy and *Ex Ante* Forecast Accuracy Measures

	Coefficients	Coefficients	Coefficients
Intercept (α_0)	−0.002***	−0.005***	−0.004***
	(−14.90)	(−29.76)	(−23.35)
EstimatedAccuracy (α_1)	0.752***		
	(29.08)		
PriorAccuracy (α_1)		0.357***	
		(16.96)	
AverageAccuracy (α_1)			0.373***
			(14.02)
Adjusted R^2	24.14%	9.16%	8.92%
Number of observations	41,077	41,077	30,564[a]

*** Indicates significance at the 1 percent level.

t-statistics are in parentheses, and are based on standard errors clustered on the firm level.

This table presents results from the regression of actual management forecast accuracy (*ActualAccuracy*) on *ex ante* forecast accuracy measures. *ActualAccuracy* and *PriorAccuracy* are defined in the notes to Panel A. *EstimatedAccuracy* is the *ex ante* accuracy measure developed by Model (1). The *EstimatedAccuracy* of 20,929 quarterly management forecasts and 20,148 annual management forecasts can be estimated using Model (1). These management forecasts are issued from the second quarter of year 1997 to the second quarter of year 2007. *AverageAccuracy* is the prior average actual forecast accuracy, and the actual earnings related to the prior management forecasts are announced during the previous four quarters relative to the current announcement quarter.

[a] I delete 10,513 management forecasts for which *AverageAccuracy* cannot be calculated, because managers did not issue forecasts for the earnings that are announced in the previous four quarters.

magnitude of the management forecast news. Previous stock return is positively related to actual forecast accuracy, suggesting that managers tend to forecast more accurately if they are under less share-price pressure. The actual accuracy of an annual forecast is likely to be higher when the forecast is bundled with an earnings announcement.

Using the above prediction model, I build an *ex ante* forecast accuracy measure by applying the estimated coefficients to the current forecast properties. For every consecutive four calendar quarters, I use the actual quarterly (annual) management forecast accuracy and other forecast properties to estimate the coefficients of Model (1). To avoid look-ahead bias, I use these estimated coefficients to obtain the *ex ante* accuracy of the quarterly (annual) management forecasts issued in the following calendar quarter. For example, I first obtain the quarterly earnings announcements made from the first quarter of 1999 to the fourth quarter of 1999 and the management forecasts of these earnings. Next, I estimate Model (1) using the actual accuracy of these management forecasts and other forecast properties. Finally, I use the estimated coefficients to compute the *ex ante* accuracy of the quarterly forecasts announced in the first quarter of 2000. In this way, the *ex ante* accuracy of 20,929 quarterly forecasts and 20,148 annual forecasts issued from the second quarter of 1997 to the second quarter of 2007 is estimated.[16]

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[16] The estimation is conducted separately for quarterly management forecasts and annual management forecasts. The *ex ante* accuracy of forecasts announced before the second quarter of 1997 cannot be estimated because there are not enough observations to generate meaningful coefficients for the prediction model.

Previous literature has used other *ex ante* accuracy measures. Ng et al. (2010) use the actual accuracy of the prior management forecast, and Li and Tse (2008) use the previous four quarters' average management forecast accuracy. Panel B of Table 1 reports the association between the actual forecast accuracy (*ActualAccuracy*) and the multiple *ex ante* accuracy measures, including the estimated accuracy (*EstimatedAccuracy*) developed by Model (1), the prior forecast accuracy (*PriorAccuracy*), and the previous four quarters' average forecast accuracy (*AverageAccuracy*). *EstimatedAccuracy* has the highest association with the actual forecast accuracy.[17] The coefficient of the univariate regression is 0.752, and the adjusted R^2 is 24.14 percent.

In the following analyses, I transform the *EstimatedAccuracy* values into indicator variables based on the previous four quarters' cut-offs. The indicator variable *Dummy_EstimatedAccuracy* equals 1 if *EstimatedAccuracy* is greater than the median, and 0 otherwise. Similarly, *Dummy_PriorAccuracy* equals 1 if *PriorAccuracy* is greater than the median, and 0 otherwise. *Dummy_AverageAccuracy* equals 1 if *AverageAccuracy* is greater than the median, and 0 otherwise.

Test of the Effect of *Ex Ante* Forecast Accuracy on the Post-Earnings-Announcement Drift

I examine the effect of *ex ante* management forecast accuracy on the post-earnings-announcement drift by estimating the following model:

$$\begin{aligned} ADJ\_RET_{i,t} = {} & \beta_0 + \beta_1 DSUE_{i,t} + \beta_2 D\_BUNDLE_{i,t} + \beta_3 DSUE_{i,t} \times D\_BUNDLE_{i,t} \\ & + \beta_4 DSUE_{i,t} \times D\_BUNDLE_{i,t} \times Accuracy_{i,t} + \beta_5 DSUE_{i,t} \times DFD_{i,t} \\ & + \beta_6 DSUE_{i,t} \times DME_{i,t} + \beta_7 DSUE_{i,t} \times DAC_{i,t} + \beta_8 DSUE_{i,t} \times DVOL_{i,t} \\ & + \beta_9 DSUE_{i,t} \times DPRC_{i,t} + \beta_{10} DSUE_{i,t} \times DINS_{i,t} + \beta_{11} DSUE_{i,t} \times DEP_{i,t} \\ & + \beta_{12} DSUE_{i,t} \times EABADNEWS_{i,t} + \beta_{13} DSUE_{i,t} \times 4THQTR_{i,t} \\ & + \beta_{14} DSUE_{i,t} \times RESPONSIVE_{i,t} + \beta_{15} DFD_{i,t} + \beta_{16} DME_{i,t} + \beta_{17} DAC_{i,t} \\ & + \beta_{18} DVOL_{i,t} + \beta_{19} DPRC_{i,t} + \beta_{20} DINS_{i,t} + \beta_{21} DEP_{i,t} + \beta_{22} EABADNEWS_{i,t} \\ & + \beta_{23} 4THQTR_{i,t} + \beta_{24} RESPONSIVE_{i,t} + \varepsilon_{i,t} \end{aligned} \quad (2)$$

where *ADJ_RET* is the size-adjusted return over the drift window and equals the compounded raw return minus the compounded benchmark return of the same CRSP size decile and the same CRSP exchange index (NYSE/AMEX or NASDAQ) that the firm belongs to. Following Shumway and Warther (1999), when a firm is delisted because of poor performance (delisting code is 500, or from 520 to 584), the delisting return is assumed to be −35 percent if it is traded on NYSE/AMEX, and −55 percent if it is traded on NASDAQ.[18]

DSUE refers to the decile rank of earnings surprise, which is defined as the actual EPS minus the most recent consensus analyst forecast, scaled by the stock price at the end of the fiscal quarter. The earnings surprises are ranked into deciles within each calendar quarter using the cut-off values from the previous quarter, coded from 0 to 1 to yield *DSUE*. The coefficient of *DSUE* can be interpreted as the abnormal return earned on a zero-investment portfolio that takes a long position in the highest *DSUE* decile ($DSUE = 1$) and a short position in the lowest *DSUE* decile ($DSUE = 0$).[19]

When an earnings announcement is bundled with the management forecast of next quarter's earnings, the indicator variable *D_BUNDLE* equals 1, and 0 otherwise. I expect bundled management forecasts to mitigate the post-earnings-announcement drift when their *ex ante* forecast accuracy (*Accuracy*) is higher; hence $\beta_3 + \beta_4$ should be significantly negative. β_4 should be significantly negative as well, under the prediction that bundled forecasts with higher *ex ante* accuracy and bundled forecasts with lower *ex ante* accuracy should have different mitigating effects on the post-earnings-announcement drift.

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[17] There is no strong reason to justify why the prior forecast accuracy should necessarily capture the *ex ante* forecast accuracy. Owing to managers' situational incentives (Mercer 2004) and the changing forecasting environment, I expect investors to use a more comprehensive, albeit more complicated, model to assess the accuracy of management forecasts, as reflected in Model (1).

[18] The percentage of firms delisted because of poor performance is 0.04 percent for the whole sample. The results are not affected if I simply delete these observations.

[19] See Doyle et al. (2006) and Zhang (2008).

Model (2) includes several control variables that prior studies have identified as being associated with the post-earnings-announcement drift. These are: analyst forecast dispersion (*DFD*); firm size (*DME*); analyst coverage (*DAC*); trading volume (*DVOL*); price (*DPRC*); institutional shareholding (*DINS*); earnings persistence (*DEP*); negative unexpected earnings (*EABADNEWS*); the fourth fiscal quarter earnings announcement (*4THQTR*); and analyst responsiveness (*RESPONSIVE*).[20]

The first three variables are proxies for information uncertainty. Analyst forecast dispersion is defined as the standard deviation of analyst forecasts divided by the stock price at the fiscal quarter end. Firm size is measured by the market value of equity at the fiscal quarter end. Analyst coverage is the number of analysts following the firm, based on the analyst estimates from I/B/E/S. Institutional shareholding is the percentage of institutional ownership, which is available from CDA/Spectrum. Following Mendenhall (2002), I estimate trading volume by multiplying the closing price and the shares traded from day −272 to day −21 relative to the earnings announcement day, and earnings persistence is the first-order serial correlation of seasonally differenced earnings estimated over the past 20 quarters. Stock price is the average stock price within one week before the earnings announcement. I transform these drift-related variables into decile ranks within each calendar quarter using the cut-off values from the previous quarter and code them from 0 to 1 (Mendenhall 2002). In this test, I use the inverse of the standard deviation of analyst forecasts to form *DFD*. Hence, consistent with *DME* and *DAC*, a higher value of *DFD* corresponds to lower analyst forecast dispersion and lower information uncertainty. *EABADNEWS* is an indicator variable that equals 1 if the unexpected earnings are negative, and 0 otherwise. *4THQTR* is an indicator variable that equals 1 if the earnings announcement is for the fourth fiscal quarter, and 0 otherwise. Following Zhang (2008), *RESPONSIVE* equals 1 if there is at least one analyst revising the forecast of next quarter's earnings within two trading days after the current quarter earnings announcement, and 0 otherwise.

Descriptive Statistics

Panels A and B of Table 2 present the means and medians of the drift-related variables conditional on the existence of the bundled management forecasts and the *ex ante* accuracy of the bundled forecasts. t-tests for means and Wilcoxon tests for medians are conducted over three subsamples: earnings announcements bundled with forecasts of higher *ex ante* accuracy (*D_BUNDLE* = 1 and *Dummy_EstimatedAccuracy* = 1); earnings announcements bundled with forecasts of lower *ex ante* accuracy (*D_BUNDLE* = 1 and *Dummy_EstimatedAccuracy* = 0); standalone earnings announcements (*D_BUNDLE* = 0).

From the second quarter of 1997 to the second quarter of 2007, among 72,851 quarterly earnings announcements, 10,641 quarterly earnings announcements are bundled with the management forecasts of next quarter's earnings.[21] Of the bundled management forecasts, 6,267

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[20] See Rangan and Sloan (1998), Bartov et al. (2000), Mendenhall (2002), Zhang (2006), and Zhang (2008), among others.

[21] To achieve a cleaner test, I delete observations that have management forecasts of Quarter $t+1$ earnings issued before the earnings announcement date of Quarter t, or between the earnings announcement date of Quarter t and the earnings announcement date of Quarter $t+1$.

TABLE 2

Descriptive Statistics for Three Categories of Earnings Announcements

Panel A: Comparison of Firm Characteristics Based on Mean Values

Mean	Earnings Announcements Bundled with Forecasts of Higher *Ex Ante* Accuracy	Earnings Announcements Bundled with Forecasts of Lower *Ex Ante* Accuracy	Standalone Earnings Announcements	Column 1 versus Column 2 t-stat (p-value)	Column 1 versus Column 3 t-stat (p-value)	Column 2 versus Column 3 t-stat (p-value)
Analyst forecast dispersion	0.002	0.007	0.007	−10.04 (<0.01)	−56.88 (<0.01)	−0.67 (0.51)
Firm size (in millions of dollars)	8,112.76	2,579.83	3,226.41	14.18 (<0.01)	13.27 (<0.01)	−4.14 (<0.01)
Analyst coverage	11.01	8.44	6.46	19.37 (<0.01)	48.29 (<0.01)	19.79 (<0.01)
Institutional shareholding	0.60	0.53	0.40	10.46 (<0.01)	43.04 (<0.01)	21.87 (<0.01)
Trading volume (in millions of dollars)	677.67	469.02	299.45	6.11 (<0.01)	13.61 (<0.01)	8.01 (<0.01)
Stock price	32.89	21.17	22.85	34.06 (<0.01)	39.02 (<0.01)	−6.09 (<0.01)
Earnings persistence	0.35	0.36	0.29	−2.73 (<0.01)	14.35 (<0.01)	15.24 (<0.01)
EABADNEWS	0.18	0.25	0.33	−8.32 (<0.01)	−29.44 (<0.01)	−12.59 (<0.01)
4THQTR	0.19	0.22	0.22	−2.57 (<0.01)	−4.54 (<0.01)	−0.52 (0.60)
RESPONSIVE	0.67	0.57	0.34	9.73 (<0.01)	51.82 (<0.01)	29.95 (<0.01)
Number of firm-quarters	6,267	4,374	62,210			

(continued on next page)

TABLE 2 (continued)

Panel B: Comparison of Firm Characteristics Based on Median Values

Median	**Earnings Announcements Bundled with Forecasts of Higher *Ex Ante* Accuracy**	**Earnings Announcements Bundled with Forecasts of Lower *Ex Ante* Accuracy**	**Standalone Earnings Announcements**	**Column 1 versus Column 2 Z-stat (p-value)**	**Column 1 versus Column 3 Z-stat (p-value)**	**Column 2 versus Column 3 Z-stat (p-value)**
Analyst forecast dispersion	0.001	0.003	0.003	−40.33 (<0.01)	−45.22 (<0.01)	7.46 (<0.01)
Firm size (in millions of dollars)	1,671.09	634.93	465.40	33.40 (<0.01)	59.19 (<0.01)	14.16 (<0.01)
Analyst coverage	9	7	5	21.09 (<0.01)	58.86 (<0.01)	25.79 (<0.01)
Institutional shareholding	0.71	0.62	0.41	10.95 (<0.01)	45.22 (<0.01)	22.88 (<0.01)
Trading volume (in millions of dollars)	194.58	144.52	59.10	12.16 (<0.01)	53.43 (<0.01)	31.05 (<0.01)
Stock price	29.18	17.00	16.92	37.79 (<0.01)	52.49 (<0.01)	0.35 (0.73)
Earnings persistence	0.39	0.41	0.33	−3.74 (<0.01)	12.53 (<0.01)	14.62 (<0.01)
EABADNEWS	0	0	0	−8.47 (<0.01)	−24.79 (<0.01)	−11.65 (<0.01)
4THQTR	0	0	0	−2.59 (<0.01)	−4.38 (<0.01)	−0.52 (0.60)
RESPONSIVE	1	1	0	9.76 (<0.01)	50.67 (<0.01)	30.87 (<0.01)
Number of firm-quarters	6,267	4,374	62,210			

(continued on next page)

TABLE 2 (continued)

Panel C: Comparison of Actual Forecast Accuracy and Estimated Forecast Accuracy across Subsamples

	No of Obs.	Actual Forecast Accuracy (*ActualAccuracy*)		Estimated Forecast Accuracy (*EstimatedAccuracy*)	
		Mean	Median	Mean	Median
Bundled quarterly management forecasts with higher *ex ante* accuracy	4,556	–0.0014	–0.0008	–0.0009	–0.001
Bundled quarterly management forecasts with lower *ex ante* accuracy	3,972	–0.0046	–0.0021	–0.0054	–0.004
Bundled annual management forecasts with higher *ex ante* accuracy	1,711	–0.0027	–0.0027	–0.0002	–0.0004
Bundled annual management forecasts with lower *ex ante* accuracy	447	–0.0091	–0.0028	–0.0158	–0.0132

The sample includes 72,851 quarterly earnings announcements from the second quarter of 1997 to the second quarter of 2007, among which 10,641 quarterly earnings announcements are bundled with the management forecasts of next quarter's earnings. Analyst forecast dispersion is the standard deviation of analyst forecasts divided by the stock price at the fiscal quarter end. Firm size is measured by the market value of equity at the fiscal quarter end. Analyst coverage is the number of analysts following the firm. Trading volume is estimated by multiplying the closing price and the shares traded from day –272 to day –21 relative to the earnings announcement day. Stock price is the average stock price within one week before the earnings announcement. Earnings persistence is the first-order serial correlation of seasonally differenced earnings estimated over the past 20 quarters. Institutional shareholding is the percentage of shares held by institutional investors at the quarter end that is closest to the earnings announcement day. *EABADNEWS* is an indicator variable that equals 1 if the unexpected earnings are negative, and 0 otherwise. *4THQTR* is an indicator variable that equals 1 if the earnings announcement is for the fourth fiscal quarter, and 0 otherwise. *RESPONSIVE* equals 1 if there is at least one analyst revising the forecast of next quarter's earnings within two trading days after current quarter earnings announcement, and 0 otherwise. *ActualAccuracy* is the *ex post* management forecast accuracy, which is calculated as: *ActualAccuracy* = –1 × *ABS(ActualEarnings – ManagementForecast)/Price*. *EstimatedAccuracy* is the *ex ante* accuracy measure developed by Model (1).

forecasts are perceived to be more accurate and 4,374 forecasts are perceived to be less accurate. Based on the t-tests for mean values, firms that have earnings announcements bundled with management forecasts of higher *ex ante* accuracy are significantly larger and have smaller analyst forecast dispersion, more analysts following, higher institutional shareholding, higher trading volume during the past, higher stock price, lower earnings persistence, less negative earnings surprises, and more responsive analysts than firms that have earnings announcements bundled with management forecasts of lower *ex ante* accuracy. Firms that have earnings announcements bundled with management forecasts of lower *ex ante* accuracy are not significantly different from firms that have standalone earnings announcements in terms of analyst forecast dispersion, but have a better information environment in terms of analyst coverage, institutional shareholding, and analyst responsiveness. The former firms are significantly smaller in market value of equity than the latter firms, based on the mean values, whereas the median values suggest the opposite. The different characteristics of these three categories of firms justify controlling the drift-related variables.

Panel C of Table 2 presents the actual forecast accuracy (*ActualAccuracy*) and the estimated forecast accuracy (*EstimatedAccuracy*) for the bundled quarterly (annual) forecasts with higher *ex ante* accuracy (i.e., *EstimatedAccuracy* is higher than the median) versus those with lower *ex ante* accuracy (i.e., *EstimatedAccuracy* is lower than the median). The statistics suggest that the estimated forecast accuracy measure correctly partitions management forecasts into the right subsamples, so that the actual forecast accuracy is in conformity with the *ex ante* forecast accuracy. For instance, the mean actual forecast accuracy of the bundled quarterly forecasts that have higher estimated accuracy is –0.0014, which exceeds the mean actual forecast accuracy (–0.0046) of the bundled quarterly forecasts that have lower estimated accuracy.

Table 3 reports the correlations among *DSUE*, *D_BUNDLE*, *Dummy_EstimatedAccuracy*, and other drift-related variables.[22] The Pearson product moment (Spearman rank order) correlations are presented above (below) the diagonal. Unless specified in parentheses with the corresponding two-sided p-values, all other correlations are significant at less than the 1 percent level. The indicator variable *D_BUNDLE* is significantly correlated with all drift-related variables. It is positively correlated with the inverse of analyst forecast dispersion (*DFD*), market value of equity (*DME*), analyst coverage (*DAC*), trading volume (*DVOL*), stock price (*DPRC*), institutional shareholding (*DINS*), earnings persistence (*DEP*), and analyst responsiveness (*RESPONSIVE*), and negatively correlated with the negative earnings surprise (*EABADNEWS*) and the fourth fiscal quarter earnings announcement (*4THQTR*). This univariate analysis indicates that firms in less uncertain information environments are more likely to issue bundled management forecasts than firms in more uncertain information environments.

IV. EMPIRICAL RESULTS

Main Analysis

Table 4 shows the regression results of Model (2).[23] Model (2a) tests the effect of the bundled management forecasts on the post-earnings-announcement drift. The average drift is 4.2 percent if earnings announcements are not bundled with the management forecasts of next quarter's earnings. The coefficient of the interaction term between *DSUE* and *D_BUNDLE* is –0.012, meaning that the

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[22] The correlation of *Dummy_EstimatedAccuracy* and other variables should be interpreted with caution because *Dummy_EstimatedAccuracy* is set to be 0 for standalone earnings announcements for test purposes.

[23] Following Zhang (2008), I delete the observations with absolute value of studentized residuals greater than 2 in all regressions, to remove the effects of outliers. This estimation procedure decreases the sample size by about 4 percent and does not change the inferences.

TABLE 3

Correlation Matrix

Panel A: Variables *DSUE* to *DAC*

	DSUE	*D_BUNDLE*	*Dummy_ Estimated Accuracy*	*DFD*	*DME*	*DAC*
DSUE	1.000	0.029	−0.001 (0.83)	0.036	0.005 (0.20)	0.004 (0.32)
D_BUNDLE	0.031	1.000	0.742	0.160	0.150	0.171
Dummy_Estimated Accuracy	0.002 (0.57)	0.742	1.000	0.217	0.189	0.175
DFD	0.045	0.161	0.216	1.000	0.440	0.256
DME	0.013	0.151	0.189	0.450	1.000	0.694
DAC	0.012	0.173	0.176	0.275	0.691	1.000
DVOL	0.015	0.129	0.124	0.150	0.654	0.645
DPRC	0.033	0.154	0.204	0.561	0.757	0.465
DINS	−0.004 (0.27)	0.117	0.113	0.193	0.363	0.303
DEP	−0.016	0.062	0.039	−0.034	0.061	0.109
EABADNEWS	−0.781	−0.095	−0.089	−0.197	−0.126	−0.124
4THQTR	0.009 (0.01)	−0.013	−0.016	−0.035	0.058	0.196
RESPONSIVE	0.027	0.208	0.178	0.165	0.377	0.500

Panel B: Variables *DVOL* to *RESPONSIVE*

	DVOL	*DPRC*	*DINS*	*DEP*	*EABADNEWS*	*4THQTR*	*RESPONSIVE*
DSUE	0.010	0.026	−0.007 (0.05)	−0.015	−0.772	0.009 (0.02)	0.025
D_BUNDLE	0.126	0.153	0.114	0.059	−0.095	−0.013	0.208
Dummy_Estimated Accuracy	0.122	0.204	0.111	0.036	−0.089	−0.016	0.178

(continued on next page)

TABLE 3 (continued)

	DVOL	*DPRC*	*DINS*	*DEP*	*EABADNEWS*	*4THQTR*	*RESPONSIVE*
DFD	0.143	0.555	0.185	−0.039	−0.198	−0.037	0.160
DME	0.663	0.755	0.355	0.056	−0.125	0.058	0.377
DAC	0.656	0.464	0.295	0.109	−0.123	0.095	0.500
DVOL	1.000	0.324	0.223	0.063	−0.087	0.063	0.375
DPRC	0.320	1.000	0.366	0.069	−0.146	0.058	0.275
DINS	0.229	0.373	1.000	0.163	−0.051	0.016	0.187
DEP	0.066	0.074	0.171	1.000	0.005	0.004	0.085
					(0.14)	(0.32)	
EABADNEWS	−0.088	−0.147	−0.052	0.007	1.000	−0.011	−0.074
				(0.08)			
4THQTR	0.063	0.058	0.016	0.004	−0.011	1.000	−0.071
				(0.33)			
RESPONSIVE	0.372	0.275	0.191	0.086	−0.074	−0.071	1.000

Unless specified in parentheses with the corresponding two-sided p-values, all other correlations are significant at less than the 1 percent level.

This table presents the correlations among *DSUE*, *D_BUNDLE*, *Dummy_EstimatedAccuracy* and other drift-related variables. *DSUE* is the decile rank of the standardized unexpected earnings, and is coded from 0 to 1. *D_BUNDLE* equals 1 if an earnings announcement is bundled with the management forecast of next quarter's earnings, and 0 otherwise. *Dummy_EstimatedAccuracy* equals 1 if *EstimatedAccuracy* is greater than the median, and 0 otherwise. Analyst forecast dispersion (*DFD*), market value of equity (*DME*), analyst coverage (*DAC*), trading volume (*DVOL*), stock price (*DPRC*), institutional shareholding (*DINS*), and earnings persistence (*DEP*) are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. *EABADNEWS*, *4THQTR*, and *RESPONSIVE* are defined in Table 2. The Pearson product moment (Spearman rank order) correlations are presented above (below) the diagonal.

bundled management forecasts decrease the drift by 1.2 percent; however, the effect is significant only at the 10 percent level (t-statistic $= -1.67$). β_3 becomes insignificantly negative (-0.004 with a t-statistic of -0.50) when other drift-related variables are controlled, as is shown in the estimation results of Model (2b). Therefore, the bundled management forecasts, in general, do not significantly decrease the magnitude of the post-earnings-announcement drift when other drift-related variables are controlled.[24]

Models (2c), (2d), (2e), and (2f) test the effect of the *ex ante* accuracy of bundled management forecasts on the post-earnings-announcement drift, using multiple measures of the *ex ante* accuracy. Models (2c) and (2d) use *Dummy_EstimatedAccuracy*, the *ex ante* accuracy measure developed in Model (1). When the drift-related variables are not controlled, the coefficient on the interaction term between *DSUE* and *D_BUNDLE* is -0.007 (t-statistic $= -0.90$), and the coefficient on *DSUE* × *D_BUNDLE* × *Dummy_EstimatedAccuracy* is -0.015 (t-statistic $= -2.53$). This result suggests that while the bundled management forecasts with lower estimated accuracy have no significant effect on the drift, the bundled forecasts with higher estimated accuracy significantly reduce the magnitude of the post-earnings-announcement drift by 2.2 percent ($\beta_3 + \beta_4$ with p-value of 0.004). In addition, the difference of the mitigating effects between these two kinds of forecasts is statistically significant as well. When the drift-related variables are controlled in Model (2d), the bundled forecasts with lower estimated accuracy do not mitigate the post-earnings-announcement drift at all ($\beta_3 = 0.005$ with t-statistic of 0.66). The bundled forecasts with higher estimated accuracy continue to have a significant mitigating effect on the drift, with a coefficient of -0.021 ($\beta_3 + \beta_4$ with p-value of 0.009).[25] Consistent with prior research, the magnitude of the post-earnings-announcement drift is significantly smaller when firms have higher trading volume, when the announced earnings are for the fourth fiscal quarter, and in the presence of responsive analysts after earnings announcements. Firm size and institutional ownership are shown to significantly reduce the drift in the previous literature (Foster et al. 1984; Bartov et al. 2000). However, these two variables do not have a significant mitigating effect on the drift when other drift-related variables are controlled.[26]

When the *ex ante* management forecast accuracy is measured by *Dummy_PriorAccuracy* or *Dummy_AverageAccuracy* in Models (2e) and (2f), β_4 and $\beta_3 + \beta_4$ become insignificant.[27] I interpret the results as the evidence that these *ex ante* forecast accuracy measures fail to correctly capture investors' perceived forecast accuracy.

I use the indicator variable *Dummy_EstimatedAccuracy* in the main analysis to better interpret the coefficients on the interaction terms, and highlight the different mitigating effects of the bundled forecasts with higher versus lower estimated accuracy. In the untabulated results, when I replace the

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[24] This result is different from Li and Tse (2008) and Wang (2008) who examine a similar research question. Li and Tse (2008) find that bundled management forecasts mitigate the post-earnings-announcement drift on average. The sample period in Li and Tse (2008) is from 1996 to 2003, during which time only 4 percent of earnings announcements are bundled with management forecasts versus the 14.6 percent of earnings announcements that are bundled with management forecasts of next quarter's earnings in my sample period. I replicate their result and find it does not hold anymore if I expand their sample period to include 2004, 2005, 2006, and 2007, suggesting the difference in the results is primarily caused by the different sample periods. Wang (2008) finds that the post-earnings-announcement drift is less significant when management forecasts of future earnings are issued. However, Wang (2008) uses a test where the dependent variable is the future earnings announcement period return, which is different from the drift period return I use.

[25] I also use the unranked earnings news to test the robustness of the results. Untabulated analysis shows that β_3 is positive and β_4 is significantly negative when drift-related variables are controlled.

[26] A possible explanation for the size effect is that this variable is highly correlated with the trading volume, as is shown in the correlation matrix.

[27] I delete 1,871 observations for which *AverageAccuracy* cannot be calculated because managers did not issue forecasts for the earnings that are announced in the previous four quarters.

TABLE 4

Regression Analysis of the Effect of *Ex Ante* Management Forecast Accuracy on the Post-Earnings-Announcement Drift

	Model (2a)	Model (2b)	Model (2c)	Model (2d)	Model (2e)	Model (2f)
Intercept (β_0)	−0.030***	−0.083***	−0.030***	−0.083***	−0.083***	−0.082***
	(−10.09)	(−14.67)	(−10.10)	(−14.69)	(−14.66)	(−14.34)
DSUE (β_1)	0.042***	0.073***	0.042***	0.072***	0.073***	0.070***
	(15.01)	(9.81)	(15.01)	(9.64)	(9.78)	(9.26)
D_BUNDLE (β_2)	0.019***	0.008*	0.021***	0.011**	0.009**	0.0001
	(4.80)	(1.86)	(5.23)	(2.53)	(2.07)	(0.03)
DSUE × *D_BUNDLE* (β_3)	−0.012*	−0.004	−0.007	0.005	−0.003	0.007
	(−1.67)	(−0.50)	(−0.90)	(0.66)	(−0.36)	(0.91)
DSUE × *D_BUNDLE* × *Dummy_EstimatedAccuracy* (β_4)			−0.015**	−0.026***		
			(−2.53)	(−4.32)		
DSUE × *D_BUNDLE* × *Dummy_PriorAccuracy* (β_4)					−0.008	
					(−1.29)	
DSUE × *D_BUNDLE* × *Dummy_AverageAccuracy* (β_4)						−0.009
						(−1.38)
DSUE × *DFD* (β_5)		0.046***		0.048***	0.046***	0.049***
		(4.94)		(5.12)	(4.95)	(5.21)
DSUE × *DME* (β_6)		−0.024		−0.023	−0.024	−0.027
		(−1.41)		(−1.37)	(−1.41)	(−1.55)
DSUE × *DAC* (β_7)		−0.012		−0.011	−0.011	−0.009
		(−0.94)		(−0.88)	(−0.92)	(−0.71)
DSUE × *DVOL* (β_8)		−0.044***		−0.044***	−0.044***	−0.039***
		(−3.69)		(−3.71)	(−3.68)	(−3.24)
DSUE × *DPRC* (β_9)		−0.008		−0.008	−0.009	−0.010
		(−0.64)		(−0.61)	(−0.66)	(−0.72)
(*DSUE* × *DINS* (β_{10})		0.001		0.001	0.001	0.002
		(0.06)		(0.14)	(0.06)	(0.22)
DSUE × *DEP* (β_{11})		0.010		0.009	0.010	0.009
		(1.28)		(1.19)	(1.26)	(1.10)
DSUE × *EABADNEWS* (β_{12})		0.009		0.009	0.009	0.009
		(0.59)		(0.57)	(0.60)	(0.57)
DSUE × *4THQTR* (β_{13})		−0.025***		−0.025***	−0.025***	−0.022***
		(−4.06)		(−4.12)	(−4.08)	(−3.69)

(continued on next page)

TABLE 4 (continued)

	Model (2a)	Model (2b)	Model (2c)	Model (2d)	Model (2e)	Model (2f)
DSUE × *RESPONSIVE* (β_{14})		−0.013**		−0.014**	−0.014**	−0.015**
		(−2.21)		(−2.33)	(−2.24)	(−2.40)
DFD (β_{15})		−0.024***		−0.024***	−0.024***	−0.024***
		(−4.40)		(−4.40)	(−4.38)	(−4.45)
DME (β_{16})		0.040***		0.040***	0.040***	0.041***
		(4.05)		(4.07)	(4.05)	(4.12)
DAC (β_{17})		0.023***		0.023***	0.023***	0.023***
		(3.31)		(3.27)	(3.30)	(3.20)
DVOL (β_{18})		−0.008		−0.008	−0.008	−0.010
		(−1.24)		(−1.25)	(−1.25)	(−1.44)
DPRC (β_{19})		0.015*		0.015*	0.015*	0.015*
		(1.89)		(1.91)	(1.90)	(1.87)
DINS (β_{20})		0.023***		0.023***	0.023***	0.023***
		(4.98)		(4.92)	(4.98)	(4.90)
DEP (β_{21})		0.011***		0.012***	0.011***	0.011**
		(2.61)		(2.64)	(2.61)	(2.57)
EABADNEWS (β_{22})		0.007*		0.006	0.006	0.006
		(1.69)		(1.63)	(1.63)	(1.55)
4THQTR (β_{23})		0.031***		0.031***	0.031***	0.029***
		(8.71)		(8.72)	(8.72)	(8.25)
RESPONSIVE (β_{24})		0.003		0.004	0.003	0.004
		(0.98)		(1.06)	(0.99)	(1.22)
$\beta_3 + \beta_4$			−0.022	−0.021	−0.011	−0.002
(p-value)			(0.004)	(0.009)	(0.26)	(0.89)
Adjusted R^2	1.04%	2.15%	1.05%	2.17%	2.15%	2.12%
Year fixed-effect	Yes	Yes	Yes	Yes	Yes	Yes

*, **, *** Indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

t-statistics (in parentheses) are based on standard errors clustered on the firm level. Year dummies are included in the regressions.

This table examines the effect of the *ex ante* management forecast accuracy on the post-earnings-announcement drift, controlling other drift-related variables. The sample includes 72,851 quarterly earnings announcements from the second quarter of 1997 to the second quarter of 2007. The dependent variable (*ADJ_RET*) is the drift-period return, which equals the compounded raw return minus the compounded benchmark return of the same CRSP size decile and the same CRSP exchange index (NYSE/AMEX or NASDAQ) that the firm belongs to. *DSUE* is the decile rank of the standardized unexpected earnings, and is coded from 0 to 1. The indicator variable *D_BUNDLE* equals 1 if an earnings announcement is bundled with the management forecast of next quarter's earnings, and 0 otherwise. *Dummy_EstimatedAccuracy* equals 1 if *EstimatedAccuracy* is greater than the median, and 0 otherwise. *Dummy_PriorAccuracy* equals 1 if *PriorAccuracy* is greater than the median, and 0 otherwise. *Dummy_AverageAccuracy* equals 1 if *AverageAccuracy* is greater than the median, and 0 otherwise. *DFD*, *DME*, *DAC*, *DVOL*, *DPRC*, *DINS*, *DEP*, *EABADNEWS*, *4THQTR*, and *RESPONSIVE* are defined in Tables 2 and 3.

indicator variables with the raw values, the coefficient on *DSUE* × *D_BUNDLE* × *EstimatedAccuracy* is −2.545 with a t-statistic of −3.77, consistent with the *ex ante* forecast accuracy being negatively associated with the magnitude of the drift. In contrast, the coefficient on *DSUE* × *D_BUNDLE* × *PriorAccuracy* or *DSUE* × *D_BUNDLE* × *AverageAccuracy* is not statistically significant.

In sum, the above results are consistent with the prediction that the bundled management forecasts of next quarter's earnings mitigate the post-earnings-announcement drift when the forecasts have high *ex ante* accuracy.

Endogeneity

The main analysis does not control for the self-selection of the decision to issue bundled management forecasts. Firms that choose to issue management forecasts of next quarter's earnings along with current-quarter earnings announcements may have certain characteristics that are associated with the relation between earnings and stock returns. To mitigate the issue of omitted correlated variables, I use the Heckman (1979) two-stage approach.

In the first stage, I adapt the probit model in Rogers and Van Buskirk (2011) to estimate the probability of issuing the bundled management forecast for each quarterly earnings announcement. Larcker and Rusticus (2010) point out that it is crucial to "develop the economic theory of the decision-making process" and identify the choice variables in the selection model. Lennox et al. (2012) provide corroborating evidence and support for this requirement. I propose that managers have the following motivations and constraints with regard to issuing bundled management forecasts.

Firms have incentive to cater to investors' information demand and to provide a transparent information environment by voluntarily issuing management forecasts of future earnings (Graham et al. 2005). Therefore, firms exposed to greater information demand are expected to voluntarily disclose more information (Hutton 2005; Chen et al. 2008). To capture this, I include two explanatory variables, the natural log of market value of equity (*LOG_ME*) and the natural log of analysts following (*LOG_AC*), in the selection model. Firms' current disclosure behavior is likely to be related to their previous disclosure record (Hutton 2005; Rogers and Van Buskirk 2011). Hence, the probability of issuing management forecasts along with the current quarter's earnings announcement should be positively associated with the existence of a prior management forecast for the current quarter (*MF_EXIST*) and the existence of a bundled management forecast for the most recent quarterly earnings announcement (*LAG_DBUNDLE*). The selection model also includes prior stock return (*PRIOR_RETURN*) and the proportion of earnings announcements that have met analyst expectations during the prior four quarters (*MEET*). The variables reflect the incentive for firms to issue management forecasts when they perform well in the stock market, or when they have successfully avoided negative earnings surprises (Feng and Koch 2010; Rogers and Van Buskirk 2011). On the other hand, firms are less likely to issue management forecasts if managers face high information uncertainty regarding predicting future earnings. Even though managers are insiders in the company, they may be constrained by the volatility of earnings, and it is costly for managers to forecast frequently and inaccurately. The selection model includes the following variables as proxies for information uncertainty: the absolute value of earnings surprise (*ABS*(*SUE*)); an indicator variable *EABADNEWS* that equals 1 if the current earnings surprise is negative, and 0 otherwise; an indicator variable *LOSS* that equals 1 if the reported earnings number is negative, and 0 otherwise, and analyst forecast dispersion (*AFD*) (Feng and Koch 2010; Rogers and Van Buskirk 2011).

Table 5 reports the results. Firms are more likely to issue bundled management forecasts if there is a prior management forecast for the current quarter's earnings, if there is a bundled management forecast for the most recent quarterly earnings announcement, if the prior stock return is higher, if the firm is larger, if there are more analysts following the firm, or if the firm is more likely to have met analyst estimates during the prior four quarters. The probability of issuing a bundled management forecast is lower when the absolute value of the earnings surprise is larger, when the current earnings announcement conveys bad news, or when the announced earnings number is negative. The results are largely consistent with the univariate analysis in Table 3. The inverse Mills ratios (*IMR*) for all firms are calculated accordingly, using the estimated coefficients. In the second stage, I include the *IMR* in Model (2) and present the results in Table 6.[28]

Under all specifications, the coefficients on *IMR* are statistically significant, justifying the control for endogeneity. The coefficient on *IMR* can be interpreted as the covariance of the error terms of the self-selection model and the post-earnings-announcement drift test. The coefficient is significantly negative when the drift-related variables are not controlled, and becomes significantly positive when the drift-related variables are included in the regressions. The main results are qualitatively similar to those in Table 4. The bundled forecasts with higher *ex ante* accuracy continue to significantly mitigate the post-earnings-announcement drift with a coefficient of −0.022 ($\beta_3 + \beta_4$ with p-value of 0.005) when the *ex ante* accuracy is measured by *Dummy_EstimatedAccuracy*.

Additional Analysis

I examine whether the effect of *ex ante* forecast accuracy on the drift varies with the actual forecast accuracy. Specifically, I compare the coefficients on *DSUE* × *D_BUNDLE* × *Dummy_EstimatedAccuracy* between the bundled forecasts that have higher actual accuracy versus the bundled forecasts that have lower actual accuracy.

When the bundled forecasts have higher actual accuracy (i.e., the actual accuracy is higher than the median), the untabulated results show the coefficient on *DSUE* × *D_BUNDLE* × *Dummy_EstimatedAccuracy* is −0.053 with a t-statistic of −8.12 when the self-selection of issuing bundled forecasts and other drift-related variables are controlled . In contrast, the coefficient is 0.018 with a t-statistic of 2.18 for the bundled forecasts that have lower actual accuracy. This finding suggests that, even though the bundled management forecasts with higher *ex ante* accuracy on average mitigate the drift, the results are driven by the forecasts that have both higher *ex ante* accuracy and higher actual accuracy. Bundled forecasts that have higher *ex ante* accuracy but lower actual accuracy do not mitigate the drift. Instead, such forecasts increase the magnitude of the drift compared to the forecasts with lower *ex ante* accuracy. Taken together, the results suggest that it is necessary for bundled management forecasts to have both high *ex ante* accuracy and high actual accuracy to mitigate the post-earnings-announcement drift.

Robustness Checks

This section tests the robustness of the preceding inferences. First, in many cases, managers forecast not only next quarter's earnings, but also earnings for two, three, four, or more future quarters along with current quarter earnings announcements. These management forecasts may provide incrementally valuable information about the persistence of current earnings into future earnings. To erase this confounding effect, I define *D_BUNDLE* as equal to 1 when an earnings

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[28] The proportion of earnings announcements meeting analyst expectations in the prior four quarters (*MEET*) is significantly associated with the issuance of bundled management forecasts, but prior literature has not documented the relation between this ratio and the post-earnings-announcement drift. This research design ensures the effectiveness of the two-stage approach, as is suggested by Gong et al. (2011) and Lennox et al. (2012).

TABLE 5

Determinants of Issuing Bundled Management Forecasts of Next Quarter's Earnings

	Coefficients (p-value)
MF_EXIST	0.956
	(<0.01)
LAG_DBUNDLE	1.118
	(<0.01)
PRIOR_RETURN	0.146
	(<0.01)
LOG_ME	0.034
	(<0.01)
LOG_AC	0.166
	(<0.01)
MEET	0.240
	(<0.01)
ABS(SUE)	−10.158
	(<0.01)
EABADNEWS	−0.061
	(<0.01)
LOSS	−0.435
	(<0.01)
AFD	−1.943
	(0.20)
Pseudo R^2	62.47%
Year dummies	Yes
Industry dummies	Yes
Number of observations	68,855

All continuous variables are winsorized at the 1 percent and 99 percent levels.

This table presents the results of the probit regression with the dependent variable *D_BUNDLE* that equals 1 if the earnings announcement is bundled with the management forecast of next quarter's earnings, and 0 otherwise. The time period is from the second quarter of year 1997 to the second quarter of year 2007. *MF_EXIST* is an indicator variable that equals 1 if there is a prior management forecast for the current quarter's earnings. The indicator variable *LAG_DBUNDLE* equals 1 if there is a bundled management forecast for the most recent quarterly earnings announcement. *PRIOR_RETURN* is the prior stock return over day −120 to day −1 relative to the earnings announcement date (day 0). *LOG_ME* is the natural log of market value of equity. *LOG_AC* is the natural log of analysts following. *MEET* is the proportion of earnings announcements that have met analyst expectations during the prior four quarters. *ABS(SUE)* is the absolute value of earnings surprise. The indicator variable *EABADNEWS* equals 1 if current earnings surprise is negative, and 0 otherwise. The indicator variable *LOSS* equals 1 if the reported earnings number is negative, and 0 otherwise. *AFD* is the standard deviation of analyst forecasts divided by the stock price at the fiscal quarter end.

announcement is bundled only with the management forecast of next quarter's earnings. The inferences regarding the effect of the *ex ante* accuracy of the bundled management forecasts on the post-earnings-announcement drift remain unchanged.

Second, the main analysis focuses on management forecasts of next quarter's earnings issued within one trading day around the current quarter earnings announcements. I next widen the event window for the definition of bundled management forecasts to include any management forecast of next quarter's earnings made in the period [−10, +1] or [−30, +1] around the current quarter

TABLE 6

Regression Analysis of the Effect of *Ex Ante* Management Forecast Accuracy on the Post-Earnings-Announcement Drift Including the Control for Self-Selection Bias

	Model (2g)	Model (2h)	Model (2i)	Model (2j)	Model (2k)	Model (2l)
Intercept (β_0)	−0.027***	−0.083***	−0.027***	−0.083***	−0.083***	−0.083***
	(−8.90)	(−13.56)	(−8.89)	(−13.56)	(−13.54)	(−13.41)
DSUE (β_1)	0.039***	0.066***	0.039***	0.064***	0.065***	0.063***
	(13.52)	(8.03)	(13.50)	(7.83)	(7.99)	(7.64)
D_BUNDLE (β_2)	0.024***	0.003	0.026***	0.005	0.004	−0.004
	(5.38)	(0.58)	(5.85)	(1.12)	(0.87)	(−0.70)
DSUE × *D_BUNDLE* (β_3)	−0.014**	−0.006	−0.008	0.003	−0.006	0.005
	(−1.96)	(−0.86)	(−1.02)	(0.44)	(−0.76)	(0.58)
IMR	−0.005**	0.005**	−0.006***	0.005**	0.005**	0.005**
	(−2.49)	(2.50)	(−2.64)	(2.42)	(2.47)	(2.06)
DSUE × *D_BUNDLE* × *Dummy_EstimatedAccuracy* (β_4)			−0.016***	−0.025***		
			(−2.67)	(−4.26)		
DSUE × *D_BUNDLE* × *Dummy_PriorAccuracy* (β_4)					−0.009	
					(−1.39)	
DSUE × *D_BUNDLE* × *Dummy_AverageAccuracy* (β_4)						−0.009
						(−1.51)
DSUE × *DFD* (β_5)		0.052***		0.055***	0.052***	0.055***
		(5.14)		(5.43)	(5.15)	(5.45)
DSUE × *DME* (β_6)		−0.026		−0.028	−0.026	−0.027
		(−1.50)		(−1.58)	(−1.50)	(−1.53)
DSUE × *DAC* (β_7)		−0.005		−0.004	−0.005	−0.004
		(−0.40)		(−0.34)	(−0.40)	(−0.29)
DSUE × *DVOL* (β_8)		−0.041***		−0.040***	−0.041***	−0.039***
		(−3.41)		(−3.32)	(−3.41)	(−3.14)
DSUE × *DPRC* (β_9)		−0.008		−0.007	−0.008	−0.009
		(−0.63)		(−0.50)	(−0.63)	(−0.64)
DSUE × *DINS* (β_{10})		0.003		0.002	0.003	0.004
		(0.32)		(0.21)	(0.32)	(0.48)
DSUE × *DEP* (β_{11})		0.011		0.011	0.011	0.010
		(1.42)		(1.44)	(1.41)	(1.26)

(continued on next page)

TABLE 6 (continued)

	Model (2g)	Model (2h)	Model (2i)	Model (2j)	Model (2k)	Model (2l)
DSUE × *EABADNEWS* (β_{12})		0.006		0.005	0.006	0.005
		(0.36)		(0.34)	(0.36)	(0.30)
DSUE × *4THQTR* (β_{13})		−0.024***		−0.024***	−0.024***	−0.022***
		(−3.82)		(−3.92)	(−3.82)	(−3.48)
DSUE × *RESPONSIVE* (β_{14})		−0.013**		−0.013**	−0.013**	−0.014**
		(−2.11)		(−2.16)	(−2.12)	(−2.25)
DFD (β_{15})		−0.022***		−0.023***	−0.022***	−0.023***
		(−3.78)		(−3.85)	(−3.75)	(−3.85)
DME (β_{16})		0.041***		0.041***	0.041***	0.042***
		(4.09)		(4.13)	(4.09)	(4.14)
DAC (β_{17})		0.022***		0.022***	0.022***	0.022***
		(3.09)		(3.10)	(3.09)	(3.00)
DVOL (β_{18})		−0.009		−0.010	−0.009	−0.010
		(−1.34)		(−1.38)	(−1.34)	(−1.37)
DPRC (β_{19})		0.012		0.012	0.012	0.012
		(1.53)		(1.53)	(1.53)	(1.44)
DINS (β_{20})		0.022***		0.022***	0.022***	0.022***
		(4.70)		(4.72)	(4.70)	(4.61)
DEP (β_{21})		0.012**		0.012**	0.012***	0.012***
		(2.60)		(2.57)	(2.60)	(2.55)
EABADNEWS (β_{22})		0.008*		0.007*	0.007*	0.008*
		(1.87)		(1.81)	(1.80)	(1.93)
4THQTR (β_{23})		0.032***		0.032***	0.032***	0.031***
		(8.85)		(8.85)	(8.85)	(8.34)
RESPONSIVE (β_{24})		0.003		0.003	0.003	0.004
		(0.96)		(0.97)	(0.96)	(1.24)
$\beta_3 + \beta_4$			−0.024	−0.022	−0.014	−0.005
(p-value)			(0.002)	(0.005)	(0.13)	(0.60)
Adjusted R^2	1.03%	2.18%	1.04%	2.20%	2.18%	2.17%
Year fixed-effect	Yes	Yes	Yes	Yes	Yes	Yes

*, ** ,*** Indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

t-statistics (in parentheses) are based on standard errors clustered on the firm level. Year dummies are included in the regressions.

This table examines the effect of the *ex ante* management forecast accuracy on the post-earnings-announcement drift including the control for self-selection of issuing bundled management forecasts. The sample includes 68,855 quarterly earnings announcements from the second quarter of 1997 to the second quarter of 2007. *IMR* is the inverse Mills ratio calculated for all firms from the estimation results in Table 5. All other variables are defined in Tables 2, 3, and 4.

earnings announcement date (day 0). Investors will incorporate these earlier management forecasts into their reaction to the current quarter earnings announcements as well. When I re-estimate Model (2) by treating these earlier management forecasts as bundled forecasts the results are very similar to those in the main analysis.

Third, in the post-earnings-announcement drift test, the sample period is from the second quarter of 1997 to the second quarter of 2007, which includes the time period before the issuance of Regulation Fair Disclosure (Reg FD). Some earnings announcements might be bundled with private management forecasts, which may also reduce the post-earnings-announcement drift by affecting analyst forecasts, but this effect is not captured, owing to the limited coverage of the First Call CIG database before the issuance of Reg FD. Therefore, I next use only the data after the issuance of Reg FD to estimate Model (2a) and get similar results.

Finally, the inferences are unchanged if I define the drift window starting from trading day 2 after the earnings announcement date of Quarter t and ending on trading day −2 relative to the earnings announcement date of Quarter $t+1$, or a 60-day drift window, as in Liang (2003).

V. CONCLUSION

The under-reaction explanation for the post-earnings-announcement drift argues that investors do not fully understand the implications of current earnings for future earnings, and that this under-reaction is corrected at future earnings announcement dates. I examine a prediction of the under-reaction explanation by investigating whether bundled management forecasts, conditional on their *ex ante* accuracy, mitigate the post-earnings-announcement drift. Using an estimated *ex ante* forecast accuracy measure that is based on forecast ability, forecast difficulty, and forecast environment, I find that bundled management forecasts mitigate the post-earnings-announcement drift when the forecasts have higher estimated accuracy. Additional analysis suggests that this finding is driven by the bundled forecasts that have both higher *ex ante* accuracy and higher actual accuracy.

By analyzing the effect of bundled management forecasts and their *ex ante* accuracy on the post-earnings-announcement drift, this paper adds to the evidence supporting the under-reaction explanation for the drift and complements previous papers that focus on the roles of conference calls and responsive analysts surrounding the earnings announcement dates. Future study may investigate the effect of other information sources on the post-earnings-announcement drift and provide more evidence to explain the existence of the drift.

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BOOK REVIEWS

Stephen A. Zeff, Editor

Editor's note: Two copies of books for review should be sent to the Book Review Editor: Stephen A. Zeff, Rice University, Jesse H. Jones Graduate School of Business, 6100 Main St., Houston, TX 77005. The policy of The Accounting Review is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

VIVIEN BEATTIE, STELLA FEARNLEY, and TONY HINES, *Reaching Key Financial Reporting Decisions: How Directors and Auditors Interact* (Chichester, West Sussex, U.K.: John Wiley & Sons, 2011, ISBN 978-0-470-74874-9, pp. xxii, 362).

This book explores how chief financial officers (CFOs), audit committee chairs, and audit engagement partners resolve issues that have given rise to interactions, discussions, or negotiations, between two of these three participants. It is a follow-up of *Behind Closed Doors* (Beattie et al. 2001), which received the Deloitte/American Accounting Association's Wildman Medal in 2007. The book provides an update of the 1999 results in the 2007/2008 U.K. regulatory environment that, as the U.S. one, has undergone significant change since 1999. The book consists of two parts.

The authors present these changes in Chapter 2. After reading the book, I understand why they do it so early in the book. As shown in their updated model of financial reporting interaction, the national regulatory regime now has a significant influence on the interaction outcomes. As in many other jurisdictions, the enforcement regime is now characterized by an increased review of company accounts and inspection of audits of public companies, which has greatly increased the risk of companies and auditors. Also, more specific to the U.K. is the adoption of IFRS and the International Standards on Auditing (ISAs) in 2005. Whereas, for a North American, these standards are often considered as principle-based and requiring the application of judgment, for many of the study's participants these standards are considered as rule-based and reducing the need for judgment.

The research method is identical to that of Beattie et al.(2001): a two phases, mixed-method approach. The first phase consists of a survey of 498 chief financial officers, audit committee chairs, and audit partners of domestic, officially listed U.K. companies. The research instrument used is a combination of closed-form and open questions on a variety of topics, including factors affecting audit quality, the impact of a range of recent changes in the U.K. regulatory framework on their working practices, and their recent experience with discussions and negotiations on 35 financial statement issues. In Chapter 3, the authors provide a summary of the results regarding the top ten issues. While the method is mixed, the significance accorded to the survey in this book is marginal: it is utilized mainly to select the ten companies used for the second phase of the study. Mixing of the data from the two methods (Creswell 2009) is chiefly at the data analysis stage, where survey data are used to support the representativeness of the interactions studied in the company cases.

The second phase of the study consists of a multiple cases study. For the nine company cases, the authors interviewed the CFOs, audit committee chairs, and audit partners regarding the relationship between the three parties and the issues that had given rise to interactions. The remainder of the book is built upon these nine case studies. In conformity with Yin (2003), the authors analyze each case separately, perform a cross-case analysis, and update/modify the model developed in Beattie et al. (2001).

Part II of the book, comprising nine chapters, one for each case, presents the findings. Each chapter follows the same structure. First, there is a descriptive part, presenting background information and interviewees' views about the corporate governance, and, for each interaction issue, the process of interaction and its resolution as perceived by the interviewees. Numerous quotations are provided to exemplify the interviewees' views. The second part of the chapter provides the authors' analysis of the case where, for each interaction, the authors analyze how each was resolved.

Reading each case is very interesting; it provides the reader with information that normally stays behind "closed doors." The advantage of a book, compared to a journal article, is that there is more space to provide detailed information about the cases. With approximately 30 pages per case, the reader has a good understanding of each case. Even then, sometimes I wanted to know more about the interaction issues. Fortunately, more detailed information about one interaction issue is available in Beattie et al. (2011).

The last part of the book presents the cross-case analysis of the 50 interactions using the framework developed in Beattie et al. (2001).[1] They find that the key influence on the interaction outcomes has changed significantly since the previous study. The influence of the general company/audit firm context and specific context of the interaction is greatly reduced, while the national regulatory regime now has the strongest influence on the interaction (events, strategies, outcomes, and consequences). In the "new" regulatory environment, it appears that the higher threats from various regulatory agencies as seen by the CFO, audit partners, and audit committee chairs encourage compliance with accounting and auditing standards.

As indicated before, the book is a successor to *Behind Closed Doors* (Beattie et al. 2001). Indeed, the authors test and update the model developed in the previous book. Chapter 2 presents a summary of this model. Whether readers need to read *Behind Closed Doors* to understand this new book depends on their interests. If readers are interested in how directors and auditors currently interact in reaching financial reporting decisions, the current book would be self-sufficient. However, if readers are interested in how the process has evolved over time, reading *Behind Closed Doors* would be useful.

This book is an excellent response to the call by Bédard and Gendron (2010) for more research on the process dynamics surrounding audit committees and for researchers to get closer to audit committees, directors, and other participants to collect primary data about processes, power, and influence.

I believe that this book should be on the "must read" list of researchers interested in audit committees and financial reporting, regardless of their research approach. For qualitative researchers, the book not only provides valuable insights into the on-process dynamics in reaching financial reporting decisions, but it is also a textbook example of how a researcher may conduct multiple case studies in accounting and gain access to key individuals in organizations to obtain private information about the financial reporting process. For empirical and experimental researchers, the book provides explanations that could be useful in developing their theoretical framework and raises many issues that could be researched in the future. The book should also be of interest to regulators, auditors, accountants, and students who are interested in the financial reporting process and the impact of recent regulation on this process.

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[1] Part III also includes a chapter on the views of the interviewees on the regulatory framework including IFRS, restrictions on non-audit services, and corporate governance.

MARK LANG and MARK MAFFETT, *Economic Effects of Transparency in International Equity Markets: A Review and Suggestions for Future Research*, Foundations and Trends® in Accounting (Hanover, MA: now Publishers Inc. 2011, ISBN 978-1-60198-448-7, vol. 5, no. 3, pp. 69).

Starting back in 2006, the series "Foundations and Trends® in Accounting" (http://www.nowpublishers.com/product.aspx?product=ACC) has established a track record of so far producing 17 survey and tutorial monographs on various fields in accounting. Each issue comprises a 50–100 page monograph written by leading researchers on a specific topic. The aim of each issue is to provide "tutorial coverage of subjects, research retrospectives, as well as survey papers that offer state-of-the-art reviews." As such, I would argue that its concept is similar to review articles published in the "Commentary" section in *Accounting Horizons* or the review papers commissioned around every ten years by the *Journal of Accounting and Economics*. In terms of required previous knowledge and based on their length, style of writing, and depth, the Foundations and Trends monographs can be situated somewhere between those long-time established sources. The main target audiences are advanced master's students in specialized programs and, in particular, Ph.D. students and their instructors. The series is open-sourced, and the monographs may be downloaded in pdf format from the aforementioned website, a feature I find very helpful for researchers based outside the U.S. who might (still) be faced with accessibility constraints.

The 69-page booklet by Mark Lang and Mark Maffett on the *Economic Effects of Transparency in International Equity Markets* (No. 16 in the series) is an excellent example of what to expect when reading one of these surveys. Its importance arises from the fact that high-quality summary articles and monographs of international work are still rather rare. There is only one prior article in Foundations and Trends by Mary Barth (2006), focusing on challenges in global standard setting. There was no survey published on international work in *JAE* in their last round of survey articles in 2011 (despite, arguably, there having been significant advances in that literature over the past decade).

Lang and Maffett focus on the economic effects of transparency in international equity markets and, therefore, on a core area within the broader context of the informational role of financial accounting in capital markets. They summarize the field of research in international accounting, which probably had the most impact on the academic community, and on where the major advances have occurred over the past decade. These advances were primarily due to the new insights from the finance literature, the availability of new data sources, and the development of new proxies for economic constructs of interest (such as the implied cost of capital). By contrast, advances on the contracting role of accounting in the international context have been much more modest. As such, the general focus of the survey is well chosen.

The booklet is organized in five chapters and has three major parts: Chapters 1 and 2 explain their working framework and focus, Chapter 3 summarizes the relevant literature falling into that framework, and Chapter 4 discusses challenges and suggests directions of future research. Chapter 5 is a one-page conclusion.

The introduction in Chapter 1 defines transparency and lays out how the authors understand "transparency" in what follows. It outlines clearly what the book is about (i.e., capital market consequences of transparency) and which related aspects are excluded from the survey (e.g., determinants of transparency). Next, it describes the core advantages of studying the research questions on transparency and capital market consequences in an international, cross-country setting: (i) there is larger variation in transparency internationally than for U.S. firms, (ii) there is cross-country variation in institutions that have been shown to shape financial reporting practice, and, thus, (iii) firms in the international context face more significant trade-offs in determining their optimal level of transparency.

Also in Chapter 1, the authors set up their framework under which they review the literature. The survey is structured around a standard discounted cash flow model (DCF), which illustrates possible drivers through which transparency may affect equity value: either through expected cash flows (resource allocation) in the numerator of the valuation equation, or alternatively through the denominator and a firm's discount rate (required rates of return), or by affecting both drivers simultaneously.

Chapter 2 explains the focus of their survey on studies analyzing equity capital effects. There are more such studies available, and the underlying relation is theoretically well established. In addition, research on other market-traded sources of financing is just emerging. The chapter also explains that multi-country settings potentially provide more power (as opposed to single-country settings, in the U.S. or elsewhere). Again, cross-country studies offer a much wider range of firm-level disclosure quality than in the U.S. as well as variation in disclosure regulation and enforcement. In addition, these settings allow researchers to study interactive effects and to judge the robustness of U.S. results when applied to multiple other countries. Such studies may also shed light on the discussion on whether reporting standards and reporting incentives are complements or substitutes, a discussion that has emerged recently.

The remainder of Chapter 2 is spent on discussing different ways of measuring transparency in an international setting: analyzing (i) the properties of earnings (e.g., smoothing, discretionary accruals, earnings targets, timely loss recognition), (ii) accounting regime choice (e.g., IFRS adoption), (iii) auditor choice, (iv) the properties of analyst forecasts, and (v) combinations of individual measures ("aggregate measures"). Note that all of these approaches can technically be applied to an international setting because they utilize information available in databases with global coverage (e.g., Compustat Global, Thomson Reuters Worldscope, Bureau van Dijk Osiris).

Chapter 3 represents the core literature review chapter. It summarizes available theory and evidence on the effects of transparency on equity values through two non-exclusive valuation channels: cash flow effects (efficient resource allocation, asset expropriation) in Chapter 3.1 and the effects on firms' discount rates and cost of capital in Chapter 3.2. Within each subchapter, the review is systematically organized into "Theory," "Why international?" and "Evidence." Given that there is more research on the link between transparency and cost of capital, Chapter 3.2 also covers related economic outcome variables beyond the cost of capital per se, for instance, effects on average liquidity or liquidity risk, investor attention, and estimation risk.

Chapter 4 outlines challenges and directions of future research. The authors' conclusion is that research on the topic is "still in its infancy," leaving significant opportunities for future research (p. 53). While I generally agree, I would argue that the "low-hanging fruit," using available global databases and research methods, is nevertheless likely gone. The authors offer three important future directions of research: (i) refining measurement, (ii) establishing more direct linkages, and (iii) identification.

First, refining measurement is important because the available transparency measures are "fairly ad hoc and noisy" (p. 54). I couldn't agree more. In fact, this is an area where accounting researchers need to bring in more of their expertise and comparative advantage. Also note that the transparency measures summarized by Lang and Maffett are almost entirely proxies developed in a U.S. context, which are usually simplistically applied to non-U.S. (GAAP) settings. Although this is a common practice, it will very likely lead to additional measurement error. For example, discretionary accrual models often "forget" to include provisions as a core accrual-type in many non-U.S. accounting regimes. Similarly, it is not even clear what "IFRS adoption" really means when "adopted" at the firm or country level. Finally, the financial statements alone (let alone other information channels) represent much richer information than what is covered in mainstream archival databases. The machine readability of pdf and txt documents or XBRL should lead to new approaches for coding transparency by considering note disclosures or even qualitative information in large samples, reflecting more the subtle effects of language and culture on transparency.

Second, Lang and Maffett note that, while linkages of transparency and economic outcomes have been documented in numerous empirical studies, the interpretation of these results has to be done very carefully. This is because the level of transparency is a choice variable (at the firm or even country level), and therefore an assessment of causality is difficult. Related challenges are the possibility of reverse causality or omitted correlated variables. Thus, as they suggest, we need to work harder on identification and on finding settings that allow for a clean identification strategy. That insight is important and goes beyond international work, because journal editors have become much more demanding when attributing a detected statistical correlation to a theorized economic relation.

In summary, and unsurprisingly, given that Mark Lang has been a key contributor to the international literature for decades, the booklet is an excellent and cohesive overview on the topic, covering most of the relevant work from the accounting, finance, and economics literatures. The comprehensiveness and balance of the survey suffers only slightly due to a natural bias toward the authors' own work, a fact they openly acknowledge (see footnote 2, p. 1). Noteworthy and, in fact, very important is that they put forward a very clear explanation for "Why international?" and consider this issue so important that they cover this question convincingly in every subchapter. If every research project using non-U.S. data would be so clear and honest in asking and answering this question, a lot would be gained for accounting research in general and for a more efficient allocation of research resources around the world.

I would like to comment on a few other positions that the authors take. First, to my taste, there is a bit too much focus on cross-country, or more specifically, "around the world" studies. Note that these studies have unique disadvantages such that country-level institutional variables are highly correlated, the chance of omitted correlated variables bias is high (there is a reason why the institutions are set up in the way they are), and they assume equal informational efficiency across capital markets. Single-country (or -setting) studies may offer more controlled experiments and thus a cleaner identification strategy. In addition, proprietary data to establish a more direct causality in an empirical research design are more likely to become available in a single foreign country setting than simultaneously for many countries around the world. My personal prediction is that we will see more focused single country studies in the near future, offering a unique feature of the accounting system, institution, or market, allowing for a cleaner identification strategy. See, e.g., Giroud et al. (2012) for an illustrative example on unexpected snow in Austrian ski hotels and effects on leverage in the recent finance literature.

Second, I would like to put even more emphasis on analyzing potential *channels* (measurement, disclosure type, content, and form) through which *financial accounting* may affect transparency and ultimately economic outcomes. Despite many decades of work, it is still unclear how specific information channels in financial reporting relate to each other. In the end, we are *accounting* researchers, and much of the content of financial reports of international firms beyond the core summary financials is still largely unexplored territory.

Third, to some extent I missed a discussion on how differences in competing *financial reporting regimes* (beyond local GAAP vs. IFRS) affect transparency in capital markets. There is not much space devoted in the booklet to actual financial reporting issues. I like to note that the global move to IFRS comes at a cost for researchers in that it removes differences across GAAP regimes, which will make an analysis of economic consequences of different measurement basis or disclosure requirements even more challenging in the future.

Finally, the title suggests a survey on "Economic Effects of Transparency in International Equity Markets." However, another important and emerging field of research on the economic consequences of transparency is the macro-level effects on economic growth and financial stability (which, ultimately, will be reflected in a country's equity values). This topic has been of long-time interest in banking supervision and regulation (e.g., third pillar of Basel II), and has recently gained momentum in academic research due to the financial crisis.

In sum, this booklet is a "must read" for any Ph.D. student interested in international work and is suggested background reading for Ph.D. courses in accounting with an emphasis on research using non-U.S. data. To get an even more comprehensive picture, this reading could be complemented by other recent surveys summarizing institutional accounting (e.g., Hail et al. 2011) and IFRS literature (e.g., Pope and McLeay 2011), and related fields of international work where most other advances have occurred over the past decade.

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CAPSULE COMMENTARY

By the Book Review Editor

ROBERT H. PARKER, STEPHEN A. ZEFF, and MALCOLM ANDERSON, *Major Contributors to the British Accountancy Profession: A Biographical Sourcebook* (Edinburgh, Scotland, U.K.: The Institute of Chartered Accountants of Scotland, 2012, ISBN 978-1-904574-85-9, pp. 137).

This is a successor to Robert H. Parker's compilation of obituaries that was published in 1980 by Arno Press. But this edition reproduces obituaries, profiles, and interviews with "major contributors" to the British accounting profession, while the earlier volume was confined to British accountants. This book reports on 37 important figures who died between 1941 and 2010, and the coverage includes professional accountants, academics, accountants in industry, editors of professional and academic journals, librarians, executives in professional institutes, and public servants.

The authors supply a lengthy introduction in which they comment on the roles played by the major contributors as well as on the changing scene in the British accounting profession during the past three decades. In a supplement, they supply references to other sources of biographical information about the individuals treated in the book.

As with other research monographs published by the Institute of Chartered Accountants of Scotland, this volume may be downloaded without charge from the Institute's website (http://icas.org.uk).

EDITORIAL POLICY AND STYLE INFORMATION

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According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature. Topical areas of interest to the journal include accounting information systems, auditing and assurance services, financial accounting, management accounting, taxation, and all other areas of accounting, broadly defined. The journal is also open to all rigorous research methods.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and editors are responsible for providing constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow *The Chicago Manual of Style* (15th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary*.

Format

1. All manuscripts should be formatted in 12-point font on 8 1/2 × 11″ paper and should be double-spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins should be at least one inch from top, bottom, and sides.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we".
5. A cover page should show the title of the paper, all authors' names, titles and affiliations, email addresses, and any acknowledgments.
6. The American Accounting Association encourages use of gender-neutral language in its publications.
7. Experimental studies using human subjects should contain a footnote affirming that approval has been granted by the institution where the experiment took place.
8. Headings should be arranged so that major headings are centered, bold, and capitalized. Second-level headings should be flush left, bold, and both uppercase and lowercase. Third-level headings should be flush left, bold, italic, and both uppercase and lowercase. Fourth-level headings should be paragraph indent, bold, and lowercase. Headings and subheadings should not be numbered. For example:

A CENTERED, BOLD, ALL CAPITALIZED, FIRST-LEVEL HEADING

A Flush Left, Bold, Uppercase and Lowercase, Second-Level Heading

A Flush Left, Bold, Italic, Uppercase and Lowercase, Third-Level Heading

A paragraph indent, bold, lowercase, fourth-level heading. Text starts . . .

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.
Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: three days; 3 kilometers; 30 years. All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text; in tables and figures, the symbol % is used.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: a cross-sectional equation; re-form. See *Webster's* for correct usage.

Keywords: The abstract must be followed by at least three keywords to assist in indexing the paper and identifying qualified reviewers.

Abstract/Introduction

An Abstract of about 100 words (150 maximum) should be presented on a separate page immediately preceeding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. The Keywords statement should appear immediately below the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

Tables and Figures

The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
2. A reference to each graphic should be made in the text.
3. The author should indicate where each graphic should be inserted in the text.
4. Graphics should be reasonably interpreted without reference to the text.
5. Source lines and notes should be included as necessary.
6. When information is not available, use "NA" capitalized with no slash between.
7. Figures must be prepared in a form suitable for printing.

Equations: Equations should be numbered in parentheses flush with the right-hand margin.

DOCUMENTATION

Citations: Within-text citations are made using an author-year format. Cited works must correspond to the list of works listed in the "References" section. Authors should make an effort to include the relevant page numbers in the within-text citations.

1. In the text, works are cited as follows: author's last name and year, without comma, in parentheses. For example: one author, (Berry 2003); two authors, (Fehr and Schmidt 2003); three or more authors, (Dechow et al. 1998); more than one work cited, (Cole and Yakushiji 1984; Dechow et al. 1995; Levitt 1998); with two works by the same author(s), (Nelson 2003, 2005).
2. Unless confusion would result, do not use "p." or "pp." before page numbers. For example, (Dechow and Dichev 2002, 41–42).
3. When the reference list contains two or more works by the same author (as the only author or first of two or more authors) published in the same year, the suffix a, b, etc., is appended to the date in the within-text citations and in the "References" section. For example, (Johansson 2004a, 2004b, 2004c; Baiman and Rajan 2002a, 2002b; Dhaliwal et al. 2005a, 2005b).

4. When the author's name is mentioned in the text, it need not be repeated in the citation. For example: "Cohen et al. (2005) provide ..."
5. Citations to institutional works should use acronyms or short titles where practicable. For example: (NCFFR, The Treadway Commission 1987).
6. If the paper refers to statutes, legal treatises, or court cases, citations acceptable in law reviews should be used.

Reference List: Every manuscript must include a "References" section that contains only those works cited within the text. Each entry should contain all information necessary for unambiguous identification of the published work. Use the following formats (which follow *The Chicago Manual of Style*):

1. Arrange citations in alphabetical order according to the surname of the first author or the name of the institution or body responsible for the published work.
2. Use authors' initials instead of proper names.
3. For two or more authors, separate authors with a comma, including a comma before "and" (Dechow, P. M., R. Sloan, and A. Sweeney).
4. Date of publication follows the author's (authors') name(s).
5. Titles of journals or newspapers should not be abbreviated.

Sample entries are as follows:

Baiman, S., and M. Rajan. 2002a. The role of information and opportunism in the choice of buyer-supplier relationships. *Journal of Accounting Research* 40 (2): 247–278.

Baiman, S., and M. Rajan. 2002b. Incentive issues in inter-firm relationships. *Accounting, Organizations and Society* 27 (3): 213–238.

Berry, R. 2003. Testimony before the Senate Committee on Homeland Security and Governmental Affairs Permanent Subcommittee on Investigations. November 18. Available at: http://hsgac.senate.gov/files/111803berry.pdf.

Cohen, D., A. Dey, and T. Lys. 2005. *The Sarbanes Oxley Act of 2002: Implications for Compensation Structure and Risk-Taking Incentives of CEOs.* Working paper, New York University, University of Chicago, and Northwestern University.

Cole, R., and T. Yakushiji, eds. 1984. *The American and Japanese Auto Industries in Transition.* Ann Arbor, MI: University of Michigan.

Dechow, P. M., R. Sloan, and A. Sweeney. 1995. Detecting earnings management. *The Accounting Review* 70 (2): 193–225.

Dechow, P. M., S. P. Kothari, and R. L. Watts. 1998. The relation between earnings and cash flows. *Journal of Accounting and Economics* 25: 133–168.

Dechow, P. M., and I. Dichev. 2002. The quality of accruals and earnings: The role of accrual estimation errors, *The Accounting Review* 77 (Supplement): 35–59.

Dhaliwal, D., Erickson, and O. Li. 2005a. Shareholder income taxes and the relation between earnings and returns. *Contemporary Accounting Research* 22: 587–616.

Dhaliwal, D., L. Krull, O. Li, and W. Moser. 2005b. Dividend taxes and implied cost of equity capital. *Journal of Accounting Research* 43: 675–708.

Easton, P. 2003. Discussion of: The predictive value of expenses excluded from pro forma earnings. *Review of Accounting Studies* 8: 175–183.

Engel, E., R. Hayes, and X. Wang. 2007. The Sarbanes-Oxley Act and firms' going-private decisions. *Journal of Accounting and Economics* (forthcoming).

Fehr, E., and K. Schmidt. 2003. A theory of fairness, competition, and cooperation. In *Advances in Behavioral Economics*, edited by C. Camerer, G. Loewenstein, and M. Rabin, 271–296. New York, NY: Princeton University Press.

Financial Accounting Standards Board (FASB). 2006. *Accounting for Uncertainty in Income Taxes, and Interpretation of FASB Statement No. 109*. FASB Interpretation No. 48. Financial Accounting Series. Norwalk, CT: FASB.

Janis, I. L. 1982. *Groupthink: Psychological Studies of Policy Decisions and Fiascoes*. Boston, MA: Houghton Mifflin.

Levitt, A. 1998. The numbers game. Speech delivered at New York University, Center for Law and Business, September 28.

Maggi, G. 1999. The value of commitment with imperfect observability and private information. *RAND Journal of Economics (*Winter) 30: 555–574.

National Commission on Fraudulent Reporting (the Treadway Commission). 1987. *Report of the National Commission on Fraudulent Financial Reporting*. Washington, D.C.: NCFFR.

Nelson, M. W. 2003. Behavioral evidence on the effects of principles- and rules-based standards. *Accounting Horizons* 17 (1): 91–104.

Nelson, M. W. 2005. A review of experimental and archival conflicts-of-interest research in auditing. In *Conflicts of Interest: Challenges and Solutions in Business, Law, Medicine, and Public Policy*, edited by D. A. Moore, D. M. Cain, G. Loewenstein, and M. H. Bazerman. Cambridge, U.K.: Cambridge University Press.

Rigdon, E. E., R. E. Schumacker, and W. Wothke. 1998. A comparative review of interaction and nonlinear modeling. In *Interaction and Nonlinear Effects in Structural Equation Modeling*, edited by R. E. Schumacker, and G. A. Marcoulides, 1–16. Mahwah, NJ: Erlbaum Associates.

Schultz, E., and T. Francis. 2002. Companies profit on workers' deaths through "dead peasants" insurance. *Wall Street Journal* (April 19): 1.

Securities and Exchange Commission (SEC). 2002. *Certification of Disclosure in Companies' Quarterly and Annual Reports*. Release Nos. 33-8124, 34-46427. Washington, D.C.: SEC.

U.S. House of Representatives. 2002. The Sarbanes-Oxley Act of 2002. Public Law 107-204 [H. R. 3763]. Washington, D.C.: Government Printing Office.

Footnotes: Footnotes are not used for documentation. Textual footnotes should be used only for extensions and useful excursions of information that if included in the body of the text might disrupt its continuity. Footnotes should be inserted using the "footnote" or "endnote" feature of the word processing software which will automatically number the footnotes throughout the manuscript with superscript Arabic numerals.

SUBMISSION OF MANUSCRIPTS

Authors should note the following guidelines for submitting manuscripts:

1. Manuscripts currently under consideration by another journal or publisher should not be submitted. The author must state upon submission that the work is not submitted or published elsewhere.
2. For manuscripts reporting on field surveys or experiments: If the additional documentation (e.g., questionnaire, case, interview schedule) is sent as a separate file, then all information that might identify the author(s) must be deleted from the instrument.
3. New manuscripts must be submitted through the Manuscript Submission and Peer Review System, located at http://accr.allentrack.net. The site contains detailed instructions regarding the preparation of files for submission. Revisions of manuscripts originally submitted before June 1, 2011 should be sent via email to the senior editor, John Harry Evans III, at tar@katz.pitt.edu. Please submit separate files for (1) the manuscript's title page with identifying information (not forwarded to reviewers), (2) the manuscript with title page and all other identifying information removed, and (3) any necessary supplemental files, such as experimental instructions and/or response memoranda on invited revisions.
4. A nonrefundable submission fee is required unless explicitly waived by the editor for invited revisions of previous submissions. The submission fee in U.S. funds is $200.00 for members and $400.00 for nonmembers of the AAA payable by credit card (VISA or MasterCard only). The payment form is available online at: https://aaahq.org/AAAforms/journals/tarsubmit.cfm. If you are unable to pay by credit card or have any questions, please contact the AAA Member Services Team at (941) 921-7747 or info@aaahq.org.
5. Revisions must be submitted within 12 months from the decision letter inviting a revision.

COMMENTS

The Accounting Review welcomes submissions of comments on previous *TAR* articles. Comments on articles previously published in *The Accounting Review* will generally be reviewed by two reviewers, usually including an author of the original article (to assist the editor in evaluating whether the submitted comment represents the prior article accurately) and an additional reviewer who is independent of the original article. If a comment is accepted for publication, the original author will generally be invited to reply. All other editorial requirements, as enumerated above, apply to proposed comments.

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POLICY ON DATA AVAILABILITY

The AAA's Executive Committee policy (originally adopted in 1989, and amended in 2009) is that the objective of the Association-wide journals (*The Accounting Review, Accounting Horizons, Issues in Accounting Education*) is to provide the widest possible dissemination of knowledge based on systematic scholarly inquiries into accounting as a field of professional research, and educational activity. To fulfill this objective, authors are encouraged to make their data available for use by others in extending or replicating results reported in their articles.

SEPTEMBER 2012 PLACEMENT ADS

The deadline for free position ads to be included in this section of *The Accounting Review* is two months prior to the desired publication in the January, March, May, July, September, or November issues. Position ads, which are free with the purchase of a job posting in the AAA Career Center, should provide all relevant information about the available positions and must include contact information or application instructions for interested candidates. For more information on how to purchase a job posting in the Career Center, or for up-to-date, detailed information about the placement listings in this issue, please go to the AAA website at http://aaahq.org and click on "Career Center," or call our office at 941-921-7747.

TEREX CORPORATION (the Company) is a *Fortune* 500 Company and a global business leader that manufactures a broad range of equipment for the construction, infrastructure, quarrying, mining, shipping, transportation, refining, and utility industries. The Company operates in four business segments throughout the world: Aerial Work Platforms, Construction, Cranes, and Materials Processing. Terex offers a complete line of financial products and services to assist in the acquisition of Terex equipment through Terex Financial Services. The Staff Accountant position is a part of the Terex Utilities Division. Terex Utilities is a truck equipment, final-stage manufacturer of digger derricks, aerial devices, and associated equipment for the utility industry, municipals, telecommunications, tree, and electrical contractor markets. The right candidate will be an energetic experienced professional with excellent communication, organizational, and multi-tasking skills. The candidate should thrive in a changing and dynamic environment and possess the ability to address and resolve integration and customer issues. This is an excellent opportunity for someone who is looking to grow within an organization and become a valued member of our team. This position is a full-time position located in Watertown, SD. Compensation is commensurate with experience and includes benefits such as paid vacation/sick time, medical/dental/vision, and 401K. Relocation is also available for this position. To apply please visit http://www.terex.com/careers and search for Staff Accountant.

FUDAN UNIVERSITY, School of Management, Accounting Department invites applications for Associate Professor/Full Professor. Interested candidates please submit a *cv* and recent research papers. In addition, three reference letters should be sent directly to: Ms. Zhou Yan, Faculty Development Office, School of Management, 670 Guo Shun Road, Shanghai 200433; Phone: 86-21-65642410; Fax: 86-21-65654523; Email: hrfdms@fudan.edu.cn; Website: http://www.fdsm.fudan.edu.cn

SAN JOSE STATE UNIVERSITY invites applications for two tenure-track faculty positions contingent on budget approval. If approved, the applicant start date is August 2013. The available accounting positions are at the Full, Associate, or Assistant Professor rank for Financial, Managerial Accounting/Audit, and Taxation or a temporary Visiting Professor in Audit or Taxation. The candidate must have demonstrated versatility in subject matter competencies and pedagogy. The teaching assignment is at both graduate and undergraduate levels including traditional and accelerated formats. The positions are subject to budget approval. The minimum credential requirement is ABD for tenure-track and visiting appointments. Strong preference will be given to candidates who hold a Ph.D. from an AACSB-accredited university. Applicants must be academically qualified based on SJSU CoB policy. The application deadline is November 11, 2012 or until the position is filled. Send application letter, *vita*, teaching evaluations, and three letters of reference to: Dr. Janis Zaima, Chair, Accounting & Finance Department, College of Business, San Jose State University, One Washington Square, BT850, San Jose, CA 95192-0066; Phone (408) 924-3490; Email: Janis.Zaima@sjsu.edu. Interviews at AAA may be scheduled with Dr. Zaima. SJSU is an Equal Opportunity/Affirmative Action Employer committed to the core values of inclusion, civility, and respect for each individual.

SAN JOSE STATE UNIVERSITY invites applications for two tenure-track faculty positions in Accounting and Tax, contingent on budget approval. If approved, the applicant start date is August 2013. The available tax positions are at Full, Associate, or Assistant Professor ranks or a combination of J.D./CPA, or J.D./M.S.T., or J.D./L.L.M.-tax or a temporary Visiting Professor. The candidate must have demonstrated versatility in subject matter competencies and pedagogy. The teaching assignment is at both graduate and undergraduate levels including traditional and accelerated formats. The positions are subject to budget approval. The minimum credential requirement is ABD for tenure-track and visiting appointments. Strong preference will be given to candidates who hold a Ph.D. from an AACSB-accredited university. Applicants must be academically qualified based on SJSU CoB policy. The application deadline is November 11, 2012 or until the position is filled. Send application letter, *vita*, teaching evaluations, and three letters of reference to: Dr. Janis Zaima, Chair, Accounting & Finance Department, College of Business, San Jose State University, One Washington Square, BT850, San Jose, CA 95192-0066; Phone: (408) 924-3490; Email: Janis.Zaima@sjsu.edu. Interviews at AAA may be scheduled with Dr. Zaima. SJSU is an Equal Opportunity/Affirmative Action Employer committed to the core values of inclusion, civility, and respect for each individual.

DEPAUL UNIVERSITY, School of Accountancy seeks to fill a tenure-track faculty appointment in accountancy and MIS. Teach graduate and undergraduate courses in accounting. Present and deliver lectures. Review and grade assignments. Perform academic advising of students. Conduct research in the field of Accounting. Ability and interest in teaching capstone courses in accounting theory and research required. To apply, go to: http://facultyopportunities.depaul.edu/applicants/Central?quickFind=50892

UNIVERSITY OF DAYTON (UD), Department of Accounting seeks candidates for a tenure-track Asst/Assoc position beginning 08/16/2013. All areas considered; prefer systems, auditing, tax, and those who teach in multiple areas. Must meet, and be able to maintain, AACSB requirements for AQ. Must be ABD; preference given to Ph.D./D.B.A. from an AACSB and/or EQUIS institution, with relevant experience. The University of Dayton, founded in 1850 by the Society of Mary, is a top ten Catholic research university. The University seeks outstanding, diverse faculty and staff who value its mission and share its commitment to academic excellence in teaching, research, and artistic creativity, the development of the whole person, and leadership and service in the local and global community. To attain its Catholic and Marianist mission, the University is committed to the principles of diversity, inclusion, and affirmative action and to equal opportunity policies and practices. We act affirmatively to recruit and hire women, traditionally under-represented minority groups, people with disabilities, and veterans. For a complete listing of qualifications and to apply go to: http://jobs.udayton.edu/applicants/Central?quickFind=53300. Review of applications will begin 07/02/2012 and continue until filled. http://www.udayton.edu/business/accounting/index.php.

BOCCONI UNIVERSITY, Accounting Department has tenure-track positions available at the level of Assistant/Associate Professor. Courses are to be taught in English. Knowledge of Italian is not required. Candidates should have a completed (or close to be completed) Ph.D. with focus on Accounting. Applicants are expected to have a demonstrated potential for, or history of, outstanding research and strong teaching skills. Applicants should send their resume, samples of research, and three letters of reference using the online form at: http://www.unibocconi.eu/jobmarket. Applications will be considered until October 15, 2012. For further academic information please contact: Prof. Sasson Bar-Yosef (sasson.baryosef@unibocconi.it) or Prof. Annalisa Prencipe (annalisa.prencipe@unibocconi.it).

LA SALLE UNIVERSITY invites applications for one tenure-track Assistant Professor beginning August 2013 (subject to budget approval). Candidates must have a doctoral degree in accounting from an AACSB-accredited university. ABDs nearing completion will be considered. Candidates should demonstrate research skills and a strong interest in undergraduate and graduate teaching. Review of applications will begin June 1, 2012 and continue until the position is filled. La Salle University is a Roman Catholic University in the tradition of the De La Salle Christian Brothers and welcomes applicants from all backgrounds who can contribute to our unique educational mission. For further information, please visit our website at: http://www.lasalle.edu. Submit electronically a letter of application, *curriculum vitae*, and contact information for three references to: Dr. Mary Jeanne Welsh, Chair, Accounting Department, La Salle University, 1900 W. Olney Avenue, Philadelphia, PA 19141: Email: welsh@lasalle.edu. AA/EEO.

UNIVERSITY OF BALTIMORE, Merrick School of Business invites applications for a faculty member to start during the 2013–2014 academic year. Individuals with research and teaching interests in auditing and a CPA certification are preferred. Employment offer is contingent on position funding. The Merrick School of Business is AACSB International accredited and is nationally recognized by *U.S. News & World Report* as one of the country's best undergraduate business programs and one of the country's best part-time M.B.A.s in 2012. UB also ranks # 1 in terms of accounting leadership in Baltimore. Qualifications: Successful candidates will have a doctoral degree in accounting from an AACSB-accredited university. Applicants with the doctoral degree are preferred although ABDs will be considered. Experienced applicants are expected to display a strong research record; new Ph.D.s and ABDs must demonstrate potential for research productivity. Salary is competitive and commensurate with experience. Document review will begin immediately. Applicants should electronically submit a letter of interest including a statement of teaching and research philosophy, a *curriculum vitae*, recent teaching evaluations, and any additional supporting materials. Three letters of recommendation highlighting teaching ability and experience should be sent to: Prof. Phil Korb at pkorb@ubalt.edu. http://www.ubalt.edu. EOE/ADA/AA

DREXEL UNIVERSITY, LeBow College of Business, Department of Accounting and Taxation invites applications for two Assistant positions starting September 1, 2013. Applicants should have earned their doctorates before the beginning of the contract period. While all areas will be considered, preference will be given to candidates with interests in Financial and Managerial Accounting. The LeBow College of Business offers an undergraduate accounting degree, an M.S. in Accounting, and a Ph.D. with an accounting emphasis. The Department has a rich tradition of interfacing with the Philadelphia business community through our well-established undergraduate co-op program and an active Accounting Advisory Board. Positions will remain open until filled. Send a letter of application, *vita*, and other supporting materials electronically to: http://www.drexeljobs.com. Drexel University is an Equal Opportunity/ Affirmative Action Employer and encourages applications from women, members of minority groups, disabled individuals, and veterans.

WASHINGTON & LEE UNIVERSITY, Department of Accounting invites applications for a tenure-track position at the Assistant or Associate level beginning in Fall 2013. Washington & Lee is a small, private, highly selective university located in the scenic Shenandoah Valley. Its Williams School of Commerce, Economics, and Politics has been AACSB-accredited since 1927. Job Qualifications: Applicants should have a doctorate or be close to completion and have teaching interests in Auditing. In addition to requiring high-quality teaching, applicants are expected to conduct research publishable in peer-reviewed journals. To support research, the School subscribes to several popular databases available through WRDS. Application Procedure: Candidates should electronically submit a cover letter, *curriculum vitae*, evidence of teaching effectiveness and scholarly work to: http://jobs.wlu.edu/postings/1198. Specific questions about the position should be sent via email to: Dr. Afshad Irani (irania@wlu.edu). Washington & Lee University is an Equal Opportunity/Affirmative Action Employer.

LOUISIANA TECH UNIVERSITY, School of Accountancy seeks applications for Assistant or Associate Professor for Fall 2013 appointment. Accounting doctorate is essential, with a strong preference for candidates with accounting certification and a preference for auditing or tax. An established publication record of discipline-based scholarship in high-quality journals is preferred. Position requires upper-division and graduate-level teaching, publishing discipline-based research, and involvement with the accounting doctoral program. Responsibilities could also include teaching an occasional online class. Applicant review continues until position is filled. Salary is competitive. Located 65 miles east of Shreveport and 30 miles west of Monroe, enrollment is around 11,500 students. Please send applications or letters of inquiry to: Thomas J. Phillips, Jr., Director, School of Accountancy, PO Box 10318, Louisiana Tech University, Ruston, LA 71272; Phone: (318) 257-2822; Fax: (318) 257-4253; Email: phillips@latech.edu; Website: http://www.business.latech.edu. EEO/AA/ADA. Women and minorities encouraged to apply. Member of the University of Louisiana System.

SAM HOUSTON STATE UNIVERSITY, College of Business Administration is seeking candidates for a tenure-track Assistant Professor position in the Department of Accounting beginning Fall 2013 (pending budgetary approval). Sam Houston State University has approximately 17,000 students and is located in picturesque Huntsville, Texas, approximately 70 miles north of downtown Houston, with easy interstate highway access to Houston and Bush Intercontinental Airport. The College is AACSB-accredited and has approximately 3,000 majors including over 550 accounting majors. The Department of Accounting has approximately a dozen tenured/tenure-track faculty. Additional information regarding the College of Business Administration and the Department of Accounting can be found at: http://www.shsu.edu/~coba/ or at http://www.shsu.edu/accounting. Candidates must have a Ph.D. or D.B.A. (or ABD near completion) in Accounting from an AACSB-accredited university, be academically qualified under AACSB standards, be highly committed to quality teaching, and exhibit strong potential for successful research and publication. Professional certification is preferred. Interested candidates may contact accounting@shsu.edu. Sam Houston State University is a member of The Texas State University System and is an EEO/AAP employer.

BRADLEY UNIVERSITY, Foster College of Business Administration invites applications and nominations for a tenure-track Assistant Professor position in Accounting. Located in Peoria, IL with 5,000 undergraduate and 1,000 graduate students, Bradley offers the opportunities of a larger university and personal attention of a small private university. The Department of Accounting has separate AACSB accounting accreditation, offers baccalaureate and master's degrees, and is a member of the Institute of Internal Auditor's Internal Audit Education Partnership program. A Ph.D., D.B.A., or near completion with emphasis in accounting is required. Professional certification (CPA, CMA, or CIA) and work experience are desirable. Primary teaching needs are in Auditing, Financial Accounting, and/or Accounting Information Systems. Demonstrated excellence in teaching and scholarly research is required for tenure and promotion. Salary is AACSB competitive. Position is available Fall 2013 with teaching opportunities available at both the undergraduate and graduate levels. Interested applicants should submit a letter of application, current *vita*, transcripts, and contact information for three professional references to: Dr. Mollie Adams, Search Committee Chair, Department of Accounting, Bradley University, Peoria IL 61625: Phone (309) 677-2284; Email: mtadams@bradley.edu. Screening of applications will begin on July 2, 2012, and continue until the position is filled. For additional information, please visit: http://www.bradley.edu. Employment with Bradley University is contingent upon the satisfactory completion of a criminal background check. Bradley University is an Equal Opportunity and Affirmative Action Employer. The administration, faculty, and staff are committed to attracting qualified candidates from underrepresented groups.

RUTGERS, THE STATE UNIVERSITY OF NEW JERSEY, CAMDEN, School of Business is inviting applications for three tenure-track positions at the Assistant Professor level in the areas of audit and managerial accounting with a secondary emphasis in tax or financial accounting. The positions will be available July 2013, subject to final academic and budgetary approval. The Rutgers School of Business, Camden is AACSB-accredited (http://camden-sbc.rutgers.edu), and serves more than 700 undergraduate and 300 M.B.A. students. The Campus is located in southern New Jersey just across the Delaware River from downtown Philadelphia, with easy access to Atlantic City, New York, and Washington, DC. The review process begins immediately and will continue until the positions are filled. The ideal candidate will possess a Ph.D. degree in Accounting and demonstrate the capacity for high-quality research and effective teaching. ABDs who expect to complete their degree before July 2013 will also be considered. The teaching load is two courses (6 credits) per semester. The positions may require some teaching at off-campus locations of the University. To apply, please email your letter of application clearly indicating your qualifications and interests, a copy of your current *vita*, and a copy of your job market paper to: SBCacctgfaculty@camden.rutgers.edu. Rutgers University is an Affirmative Action/Equal Opportunity Employer, and committed to diversity among faculty. Women, under-represented minorities, veterans, and individuals with disabilities are especially encouraged to apply.

UNIVERSITY OF INDIANAPOLIS, Accounting School of Business seeks candidates for an Assistant or Associate Professor for a full-time, nine-month, tenure-track position with a start date of January 2013. The successful candidate will participate in teaching, service, and scholarship as it relates to the field of accounting. The ideal candidate will have a strong record of teaching excellence at the undergraduate level for accounting and nonaccounting majors; the ability to teach introductory financial and managerial accounting; and willingness to teach some evening sections. Experience with accounting information systems is preferred. Earned doctorate in accounting or related business field and proficiency in teaching and knowledge of the accounting field required. ABD candidates may apply. M.B.A./CPA candidates may be considered for a non-tenure-track appointment. Applied scholarship and active participation in the discipline is a plus. Apply electronically at: http: //www.uindy.edu/visitors/hr. The University of Indianapolis is an Affirmative Action/Equal Opportunity Employer and encourages applications from women and minorities.

AUBURN UNIVERSITY, School of Accountancy is seeking to fill one full-time tenure-track Assistant Professor position, contingent upon budget approval, beginning Fall 2013. Job Qualifications: Successful applicants must possess a doctorate in accounting from an AACSB-accredited institution prior to the date of employment. The successful candidate must meet eligibility requirements to work in the U.S. at the time the appointment is scheduled to begin and continue working legally for the proposed term of employment. Excellent communication skills are required. Job Responsibilities: While all areas of teaching and research will be considered, the areas of immediate need are financial and AIS. Candidates must have strong research credentials/potential and will be expected to publish scholarly journals. The faculty member is expected to contribute in instruction, research, and service/outreach/extension. Candidates with professional certification are desired. Application Procedure: Review of applications will begin September 1, 2012 and continue until a qualified candidate is selected and recommended for appointment. Applicants should mail a letter of application, curriculum *vitae*, teaching evaluations, a current working paper or dissertation proposal, and names of three references to: Recruiting Coordinator, 301 Lowder Business Building, Auburn, AL 36849. Materials may also be emailed to: acct@auburn.edu. Auburn University is an Affirmative Action/Equal Opportunity Employer. Minorities and women are encouraged to apply.

KENT STATE UNIVERSITY, Department of Accounting invites applications for a tenure-track position at the Assistant, Associate, or Full Professor level beginning Fall 2013. We are primarily interested in candidates with teaching and research interests in Financial Accounting. Preference will be given to experienced Assistant, Associate, or Full Professors with an established scholarly record. Candidates must have a Ph.D. with a major in accounting or an appropriate related field. Professional certification such as a CPA or CMA is highly desirable. Research will be expected in high-quality, academic refereed journals; service on doctoral students' dissertations will be part of the faculty member's responsibilities. Review of applications will begin immediately and continue until the position is filled. Applicants may contact: Professor Mark Altieri, Chair Faculty Recruiting Committee, Department of Accounting, Kent State University, PO Box 5190, Kent OH 44242-0001; Email: maltieri@kent.edu. Applicants will be required to apply online at: http://jobs.kent.edu and search for posting #990060. AA/EOE.

KENT STATE UNIVERSITY, Department of Accounting invites applications for a tenure-track position at the Assistant, Associate, or Full Professor level beginning fall 2013. We are primarily interested in candidates with teaching and research interests in Cost/Managerial Accounting. Candidates must have a Ph.D. with a major in accounting or an appropriate related field. Professional certification is highly desirable. Research will be expected in high-quality, academic refereed journals; service on doctoral students' dissertations will be part of the faculty member's responsibilities. Candidates for an Associate or Full position must have an established scholarly record. Review of applications will begin immediately and continue until the position is filled. Applicants may contact: Professor Mark Altieri, Chair Faculty Recruiting Committee, Department of Accounting, Kent State University, PO Box 5190, Kent OH 44242-0001; Email: maltieri@kent.edu. Applicants will be required to apply online at: http://jobs.kent.edu and search for posting #998731. AA/EOE.

CLAYTON STATE UNIVERSITY, Morrow, GA has two permanent faculty tenure-track faculty lines in accounting that are expected to be offered and filled beginning August 2013. An AACSB-accredited School with five full-time accounting faculty supporting an undergraduate B.B.A. accounting major and an M.B.A. accounting concentration, we seek Accounting Ph.D.s from AACSB-accredited institutions. Rank of Assistant or Associate is negotiable with salary. CPA and other certification is desirable; 18 graduate hours in finance is also a plus. Our greatest needs in teaching are auditing and tax, but will consider candidate interests in AIS or financial. All accounting faculty are expected to teach accounting principles courses in addition to their interest areas. Opportunities exist for graduate teaching, and research and service requirements are competitive with other AACSB institutions. Our wooded campus of three lakes is located in beautiful suburban Atlanta, GA. To learn more, contact Greg Kordecki, 404-798-0150, gregkordecki@clayton.edu.

MIAMI UNIVERSITY, Department of Accountancy has up to three positions available. Assistant or Associate Professor to teach accounting courses at both the undergraduate and master's level; conduct and publish scholarly research; provide service to the institution, academy, and profession. Required qualifications: A doctorate in accounting or dissertation in process with completion of degree by December 31, 2013. For appointment to rank of Associate Professor requires high-quality teaching and research. Preferred qualifications: Candidates who have primary interest in tax accounting, although applicants interested in any area of accounting will be considered; professional certification (CPA or similar); relevant professional work experience. Employment: Positions are expected to begin August 2013. Both the undergraduate and graduate accounting programs are AACSB-accredited; information about the faculty and curriculum is available at: http://www.sba.muohio.edu. Application Procedure: Submit letter expressing interest in the position, resume, and letters from three references online at: http://www.fsb.muohio.edu/employment/accounting. Online applications only will be accepted. Screening of applications begins September 1, 2012 and will continue until the positions are filled. Women and minorities are encouraged to apply. Miami University is an EOE/AA employer with smoke-free campuses and offers full same-sex domestic partner benefits. Right to Know-Consumer Information at: http://www.miami.muohio.edu/about-miami/publications-and-policies/student-consumer-info/. Hard copy upon request.

TRUMAN STATE UNIVERSITY, School of Business, AACSB-accredited, invites applications for one (1) tenure-track position in accounting (rank depends on qualifications). Talented students and a collegial faculty create an outstanding work environment. Starting date is August 2013, but is negotiable. For more information, please go to: http://provost.truman.edu/positions/ACCT043012Fall_2013.pdf

CORNERSTONE RESEARCH, a leading economics and finance consulting firm specializing in analyzing economic, financial, accounting, and marketing issues, is interested in hiring full-time and summer associates for its offices in Boston, MA, Los Angeles, CA, Menlo Park, CA, New York, NY, San Francisco, CA, and Washington, DC. Cornerstone Research provides an interesting and rewarding work environment. Associates work directly with esteemed faculty experts in a distinctive partnership that combines the strengths of the academic and business worlds. We seek Ph.D. candidates with the ability to apply academic research to real-world issues, present concise explanations of complex analyses, and independently manage projects and junior staff. The ideal candidate will possess a strong empirical background in Financial, Cost, or Auditing, as well as demonstrate excellent interpersonal skills. We also seek experienced candidates for more senior levels. Compensation commensurate with experience. Submit resume, sample research paper, and cover letter that includes a statement describing your interest in economic consulting and your location preferences, resume, and letters of reference at: http://Associatecareer.cornerstone.com. Recruiting Coordinator, AR web page: http://www.cornerstone.com

NORTHERN ILLINOIS UNIVERSITY invites applications for Director of its CPA Review Program. The Director of the NIU CPA Review is the administrator of the credit and noncredit CPA review courses offered through the Department of Accountancy in all formats (live, online, and blended). The Director is expected to teach in the program, recruit, and supervise CPA Review faculty, review and approve materials developed for the program, market the program, recruit and advise program participants, prepare and monitor the annual budget, and represent the Department of Accountancy by interacting with firms, professional organizations, and other universities. Additional information about the CPA Review can be found at http://www.cpareview.niu.edu/. Candidates must have a master's degree in accounting (or an M.B.A. with an accounting specialization) or a bachelor's degree with a minimum of ten years of significant accounting experience in public accounting or industry. All candidates must have at least three to five years with a public accounting firm or a comparable position in industry. In addition, candidates must have a demonstrated record of effective teaching (either at the college level or in professional accounting continuing education) and be a Certified Public Accountant (CPA) licensed to practice in a U.S. jurisdiction. Applicants should send a cover letter, resume, and a summary of any teaching evaluations to: Jim Young, Chair, Department of Accountancy, Northern Illinois University, DeKalb, IL 60115. Review of complete applications will begin on July 31, 2012 and will continue until the position is filled. AA/EEO institution.

MONTANA STATE UNIVERSITY–BILLINGS, College of Business is seeking an Assistant/Associate Professor of Accounting who will be responsible for teaching some combination of courses in two or more of the following areas: Intermediate Accounting, Auditing, Managerial/Cost, Accounting Information Systems, or Applied Business Decisions. MSU Billings is a teaching-oriented institution, and excellence in teaching is its primary focus. The normal course load is seven sections each academic year. For complete job details and qualifications, please visit: http://www.msubillings.edu/humres and click on the link for Employment Opportunities. Review of applications will begin July 15, 2012; however, the position is open until filled. For more information contact: Mike Campbell, Department Chair, (406) 657-1651 or at: mcampbell@msubillings.edu. Human Resources Office/EEO/AA Office. Montana State University–Billings. 1500 University Drive, Billings, MT 59101-0298; Phone: (406) 657-2278; Fax: (406) 657-2120; Email: employment@msubillings.edu; Website: http://www.msubillings.edu.

WASHBURN UNIVERSITY, School of Business invites applications for an Assistant or Associate Professor of accounting to begin Fall 2013. Preference given to candidates with Ph.D. completed; ABDs considered if substantial progress made toward completion of dissertation. Preference also given to those with a CPA. Teaching is expected at the undergraduate and master's level. Ideal candidate will have demonstrated excellence in teaching and commitment to scholarship and service commensurate with AACSB International accreditation. Applicants should submit a letter of application, current *vita*, names of three references, transcripts of all graduate work, summary of teaching performance, and copies of recent scholarly work to: Chair, Accounting Position Search Committee, School of Business, Washburn University, 1700 SW College Avenue, Topeka, KS 66621. Salary competitive and dependent on qualifications and experience. Application review begins immediately and continues until position is filled. The School of Business is AACSB-International accredited and serves more than 700 B.B.A. and M.B.A. students. Employment at Washburn University may be conditioned upon satisfactory completion of a background check. Washburn University is an EOE. Please visit: http://www.washburn.edu/business for more information. (Recruitment Number 90060612)

UNIVERSITY OF MICHIGAN–DEARBORN, College of Business invites applicants for a tenure-track accounting position, starting in September 2013. A primary interest in Financial/Managerial Accounting or Taxation is preferred, others will be considered. Faculty rank is open for candidates possessing a Ph.D./D.B.A. ABDs will be considered and are encouraged to apply. The successful applicant will be dedicated to quality teaching, research, and service. Salary is competitive including support for research and professional development. The University of Michigan–Dearborn (UM–D) is one of the three campuses of the University of Michigan. UM–D, a comprehensive university offering high-quality undergraduate, graduate, professional, and continuing education to residents of southeastern Michigan, and attracts more than 8,500 students. The campus is strategically located on 200 suburban acres of the original Henry Ford Estate in the Greater Detroit Metropolitan region. The College of Business is AACSB-International accredited and offers B.B.A., M.B.A., M.S.-Accounting, M.S.-Finance, and M.S.-Information Systems Degrees. Interested candidates should send a letter of application and current *vita* to: Dr. Kelly Cai (kcai@umd.umich.edu), College of Business, University of Michigan–Dearborn, 19000 Hubbard Drive, Dearborn, MI 48126. The University of Michigan–Dearborn is dedicated to the goal of building a culturally diverse and pluralistic faculty committed to teaching and working in a multicultural environment, and strongly encourages applications from minorities and women. The University of Michigan-Dearborn is an Equal Opportunity/Affirmative Action Employer.

OHIO UNIVERSITY, School of Accountancy invites applications for two tenure-track appointments as Assistant/Associate Professor beginning in August 2013. We are looking for student-friendly, research-active, innovative faculty members, and will consider applicants with teaching and research interests in any area of accounting. All applicants must be academically qualified under AACSB standards, be able to show evidence of the ability to produce quality research and to achieve a record of teaching excellence, and be committed to professional interaction. Ohio University's College of Business and its School of Accountancy are both accredited by the AACSB. The School of Accountancy at Ohio University takes pride in the ongoing success of our problem-based curriculum, our innovative faculty, and our outstanding relationships with the Accounting Profession. Our graduates are heavily recruited by the Big 4 and large regional accounting firms. Ohio University takes pride in its vibrant residential campus that attracts top-quality students. Athens, Ohio, is a quintessential college town and is a family-friendly and culturally rich community located amidst scenic state and national parks just 75 minutes southeast of Columbus, Ohio. Review of applications will begin August 1, 2012 and continue until the position is filled. Please send *vita* and letter of interest directly to Dr. Ann Gabriel, Director of the School of Accountancy at: gabriele@ohio.edu.

ARKANSAS STATE UNIVERSITY (ASU) is recruiting applicants for an Assistant Professor in the department of Accounting. The Assistant Professor will teach accounting classes at the graduate and undergraduate levels, using a variety of delivery methods. The position is housed on the Jonesboro campus. Therefore, visit: https://jobs.astate.edu for detailed information and to apply for position F00616. ASU is committed to creating a productive workplace in which both persons and property are secure. To achieve that goal, background investigations are conducted on all final applicants recommended for employment. ASU is an Equal Opportunity/Affirmative Action Employer with a strong institutional commitment to the achievement of excellence and diversity among its faculty and staff.

UNIVERSITY OF ILLINOIS AT URBANA–CHAMPAIGN, The Department of Accountancy is recruiting faculty for full-time appointments at all ranks. Candidates at advanced ranks must possess a doctoral degree, and candidates for Assistant Professor positions must possess or be near completion of a doctoral degree. Applicants for senior positions should have a strong research record and the ability to participate in doctoral education and curriculum development at the undergraduate and graduate levels. In addition, non-tenure-track Lecturer and Visiting positions are available. Applicants should submit a resume that lists research publications and courses taught. Compensation is competitive and will be commensurate with the appointment level and applicant's record. For full consideration, please create your candidate profile at: http://jobs.illinois.edu and upload your required documents, including cover letter, *curriculum vitae*, teaching statement, research statement, working papers, course evaluations, and the names and email addresses or phone numbers of three professional references. Tenure-track applications should be submitted by January 31, 2013. Visitor and Lecturer applications should be submitted by March 31, 2013. In your cover letter please state which rank you are applying for. For further information please refer to: http://www.business.illinois.edu/accountancy/recruit.html or email: accyrecruiting@business.illinois.edu. Contact: Recruiting Chair, Department of Accountancy, 360 Wohlers Hall, College of Business, University of Illinois, 1206 S. Sixth Street, Champaign, IL 61820. Illinois is an Affirmative Action/Equal Opportunity Employer and welcomes individuals with diverse backgrounds, experience, and ideas who embrace and value diversity and inclusivity. (http://www.inclusiveillinois.edu).

CALIFORNIA STATE UNIVERSITY, SAN BERNARDINO, Department of Accounting and Finance expects to fill a tenure-track faculty position in accounting beginning September 1, 2013, rank open, pending budgetary approval. At the time of appointment, the candidate must have a Ph.D. in accounting or be academically qualified in accounting according to AACSB standards. Industry experience and/or CPA a plus. Candidates at higher ranks are expected to demonstrate excellence in teaching, research, and service. All areas of accounting will be considered, but preference will be given to candidates with expertise in financial accounting, government accounting, and/or taxation. As one of the five departments in the College of Business and Public Administration, which is accredited by AACSB and NASPAA, the Department of Accounting and Finance is seeking AACSB-accreditation for its accounting programs. The successful candidate will be expected to participate in this endeavor, to meet the traditional requirements of excellence in teaching, research, and service, and to support the University's strategic plan, which includes partnership with the community to enhance the social, economic, and cultural conditions of the community and beyond. Salary dependent on qualifications and experience. Possibility to earn additional compensation through summer school teaching. Generous medical, dental, and vision benefits, support for moving expenses, and CalPERS pension. Applications accepted until the position is filled but preferably by July 20, 2012. Submit a *cv* and letter of application to: rchen@csusb.edu as well as to: tsalas@csusb.edu.

UNIVERSITY OF MISSOURI–COLUMBIA, School of Accountancy is seeking applications for a tenure-track faculty position at the Assistant Professor level for Fall 2013. Candidates for the position must have an interest in Auditing, and be committed to high-quality scholarly research. Applications are encouraged from experienced Assistant Professors as well as new Ph.D.s or those nearing completion of their Ph.D. Salary and research support are competitive. Application materials including a letter of interest, *vita*, evidence of teaching competence, and a sample of working papers should be submitted online at: http://hrs.missouri.edu/find-a-job/academic/index.php. Three reference letters should be mailed to: Recruiting Committee, School of Accountancy, University of Missouri, 303 Cornell Hall, Columbia, MO 65211. Early applications are encouraged, and screening will begin October 1, 2012, and continue until the position is filled. The University of Missouri is an Equal Opportunity/Affirmative Action Employer. Applications from minority and women candidates are strongly encouraged for this position. To request ADA accommodations, please contact the University's ADA Coordinator at (573) 884-7278 (V/TTY).

THE UNIVERSITY OF OKLAHOMA, John T. Steed School of Accounting invites applications for the position of Assistant Professor. Candidates must possess an appropriate doctorate or be near completion. Candidates must have academic training that indicates the potential for significant high-quality academic research. The anticipated appointment date is August 15, 2013. All qualified individuals are encouraged to submit application materials to: Jan Nelson, Managerial Associate, Steed School of Accounting, Price College of Business, University of Oklahoma, 307 W. Brooks, AH 200, Norman, OK 73019-400; Email: jnelson@ou.edu; Phone: (405) 325.4223; Fax: (405) 325.7348. For questions, please contact the search committee chair, Wayne Thomas (wthomas@ou.edu). The screening of applications will begin immediately. Applications will be accepted until the position is filled. The University of Oklahoma is an Equal Opportunity/Affirmative Action Employer. Women and Minorities are encouraged to apply.

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY, Department of Accounting and Information Systems, Pamplin College of Business seeks to fill one or more tenure-track positions to begin August 10, 2013. Preference will be given to qualified candidates at the Assistant or Associate Professor levels. The department has particular need for individuals with teaching and research interests in the areas of auditing, managerial, and financial accounting. However, individuals with interests in other areas will be considered. Successful candidates will have a Ph.D. in accounting from an AACSB-accredited school, a program of scholarly research consistent with the rank applied for, and instructional capabilities at both the undergraduate and graduate levels. Information about the department and its programs is available at: http://www.acis.pamplin.vt.edu. Interested candidates *must* apply online at: http://www.jobs.vt.edu (posting number 0121558) and include with the electronic application a cover letter, resume, and three letters of recommendation or names and contact information of three references. Review of applications will begin June 1, 2012. General inquiries should be directed to: Professor Greg Jenkins, Chair of the Recruiting Committee at: greg.jenkins@vt.edu, or Professor Reza Barkhi, Department Head, 3007 Pamplin Hall (0101), Blacksburg, VA 24061. Virginia Tech is an Equal Opportunity/Affirmative Action Employer.

EAST CAROLINA UNIVERSITY invites applications for a tenure-track Assistant/Associate Professor position in Accounting beginning in the 2013–2014 academic year. Applicants should be (or expect to be) terminally qualified (Ph.D. in Accounting) by August 2013, with appropriate professional certification. Our current needs place an emphasis on systems and cost. A 12 s.h. load is standard with release time granted for research and publication productivity. The normal teaching load is 9 s.h. but 6 s.h. loads are available for outstanding research productivity. Send credentials (cover letter that describes specific skills and experience relevant to this position, current resume/*vita*, current teaching evaluations, and three original letters of reference) to: Dr. Dan Schisler, Chair, East Carolina University, 3415 Bate Building, Greenville, NC 27858-4353: or via email to: schislerd@ecu.edu. Applicants must comply with the Immigration Reform and Control Act. ECU is an Equal Opportunity/Affirmative Action University and accommodates individuals with disabilities.

NEW MEXICO STATE UNIVERSITY seeks candidates for an Assistant Professor, who are required to hold or be near completion of an earned doctorate in accounting or in business administration with major in accounting from an AACSB-accredited business school. Candidates for Associate Professor are additionally required to have an established record of high-quality teaching and research, including publications in major accounting journals. The successful applicant will have responsibilities in teaching and advising, research and service to the Department, College, University, and community. The position requires continued scholarly activity, including research publications in refereed accounting journals. Candidates with teaching interests in financial and managerial accounting, accounting systems, accounting ethics, and auditing will be considered. NMSU holds business and accounting accreditation by AACSB International. The University is the state's land-grant institution and has approximately 18,000 main campus students. Preference will be given to candidates who can provide evidence demonstrating effective teaching and a solid publication record and/or potential for their level of academic experience. Submit letter of application, current resume, unofficial graduate transcripts, and list of three persons who may be contacted for references. Review of applications will begin August 20, 2012; applications received after this date may be considered. Contact: Dr. Kevin Melendrez, Box 30001/MSC 3DH, New Mexico State University, Las Cruces, NM 88003-8001; Phone: (575) 646-4901; Email: kdm@nmsu.edu.

SEATTLE UNIVERSITY invites applications for a tenure-track Assistant or Associate Professor in the Department of Accounting beginning September 2013. The Department of Accounting offers the B.A.B.A. with a concentration in Accounting and the M.P.Ac. degree, including a business valuation specialization. The Accounting Department's M.P.Ac. program is currently ranked 19th in the nation and the undergraduate program is currently ranked 21st in the nation by *U.S. News & World Reports*. The Accounting Department website can be found at: http://www.seattleu.edu/albers/inner.aspx?id=24360. Job responsibilities include teaching, research, and service. Requirements for the position include: Ph.D. in Accounting from an AACSB-accredited institution prior to the date of employment; established record or promise of excellence in scholarship and teaching; ability to teach a range of courses in the undergraduate and graduate programs in the areas of financial accounting and/or auditing. Seattle University, founded in 1891, is a Jesuit Catholic university located on 48 acres on Seattle's Capitol Hill. More than 7,700 students are enrolled in undergraduate and graduate programs within eight schools. *U.S. News & World Report*'s "Best Colleges 2012" ranks Seattle University among the top 10 universities in the West that offer a full range of master's and undergraduate programs. Applicants should submit materials online at https://jobs.seattleu.edu, including a cover letter, *curriculum vitae*, teaching and research statement, recent teaching evaluations, and a sample of research output, as well as name and contact information for three references (if available at this time). Application deadline is October 1, 2012. Seattle University is an Equal Opportunity Employer.

SALEM STATE UNIVERSITY, Bertolon School of Business at Salem State University is seeking to fill a full-time, tenure-track position in the Accounting and Finance Department for an accounting faculty member, starting Fall 2013. In addition to teaching, faculty members, as a part of a cooperative and collegial campus community, will advise students, perform college service, and pursue research. This position is advertised subject to available funding. Required qualifications include: Ph.D. or D.B.A. in Accounting (ABD considered). Preferred qualifications include: previous university/college teaching experience in accounting; experience in and commitment to teaching in a multi-racial, multi-ethnic environment with students of diverse backgrounds and learning styles; and experience in distance learning and instructional technologies. SSU is a comprehensive, publicly supported institution of higher learning located approximately 15 miles north of Boston, Massachusetts. The university enrolls more than 10,000 undergraduate and graduate students representing 27 states and 65 nations, and is one of the largest state universities in the Commonwealth of Massachusetts. Its mission is to educate and prepare a diverse community of learners to contribute responsibly to our global society. Candidates who enjoy serving as role models and mentors for a diverse student body are preferred. The salary is competitive, and commensurate with education and experience. To see the full list of benefits, review the complete job description, and apply online, please visit https://jobs.salemstate.edu/applicants/Central?quickFind=51237, and attach your resume and cover letter. Salem State University is an Equal Opportunity/Affirmative Action Employer. Persons of color, women, and persons with disabilities are strongly urged to apply.

CHRISTOPHER NEWPORT UNIVERSITY, Joseph W. Luter III School of Business invites applications for a tenure-track position in Accounting. Required qualifications include a Ph.D. or D.B.A. in Accounting. Professional certification, professional accounting experience, and experience teaching at the college level are plusses. Individuals with appropriate master's degrees who are ABD at the final stage of completion may be considered for appointment at rank of Instructor and begin the probationary appointment with the terminal degree the following year. Starting date is August 2013. The successful candidate will demonstrate the potential for excellent teaching, producing scholarly work, contributing to the intellectual life of the University, and engaging students inside and outside the classroom. Preference will be given to candidates interested in teaching managerial accounting courses, but qualified individuals in all specialty areas of accounting will be considered. Located between historic Colonial Williamsburg and the ocean resort of Virginia Beach, CNU is committed to outstanding teaching and learning, undergraduate education, and the liberal studies core. The Luter School's AACSB-accredited business curriculum attracts some of the University's best students, and requires a minimum GPA of 3.0. The business program offers students opportunities for international experiences, internships, volunteer work, and research collaboration with faculty. Full job posting (Search #8221) at: http://www.cnu.edu/equalopp/recruitment/jobopps.asp. EOE

SALEM STATE UNIVERSITY. Bertolon School of Business is seeking to fill a full-time, tenure-track position in the Accounting and Finance Department for an accounting faculty member for Fall 2013. This position is advertised subject to available funding. Required qualifications include: Ph.D. or D.B.A. in Accounting (ABD considered). Preferred qualifications include: previous university/college teaching experience in accounting; experience in and commitment to teaching in a multi-racial, multi-ethnic environment with students of diverse backgrounds and learning styles; and experience in distance learning and instructional technologies. SSU is a comprehensive, publicly supported institution of higher learning located approximately 15 miles north of Boston, Massachusetts. The university enrolls more than 10,000 undergraduate and graduate students representing 27 states and 65 nations, and is one of the largest state universities in the Commonwealth of Massachusetts. Its mission is to educate and prepare a diverse community of learners to contribute responsibly to our global society. In addition to teaching, faculty members, as a part of a cooperative and collegial campus community, will advise students, perform college service, and pursue research. Candidates who enjoy serving as role models and mentors for a diverse student body are preferred. The salary is competitive, and commensurate with education and experience. To see the full list of benefits, review the complete job description and to apply online, please visit: https://jobs.salemstate.edu/applicants/Central?quickFind=51237, and attach your resume and cover letter. Salem State University is an Equal Opportunity/Affirmative Action Employer. Persons of color, women, and persons with disabilities are strongly urged to apply.

QUEEN'S SCHOOL OF BUSINESS, Kingston, Ontario, Canada invites applications for an entry-level, tenure-stream position (subject to budgetary approval) in Accounting starting July 1, 2013. Preference will be given to candidates interested in managerial accounting. Candidates must have a Ph.D. or be near completion and exhibit high potential for innovative scholarly research and outstanding teaching contributions in support of the School's various public and private programs. Submit a cover letter, *cv*, three letters of recommendation, teaching evaluations and a sample publication electronically to: accountingrecruiting@business.queensu.ca This is an international search, open to candidates of all nationalities. In accordance with Canadian Immigration requirements, priority will be given to Canadian citizens and permanent residents. Queen's University is committed to employment equity and diversity in the workplace. The academic staff at Queen's University are governed by a Collective Agreement (http://www.qufa.ca).

QUEEN'S SCHOOL OF BUSINESS, Kingston, Ontario, Canada invites applications for an open tenure-stream position (subject to budgetary approval) in Accounting starting July 1, 2013. Preference will be given to candidates interested in managerial accounting. Candidates must have a Ph.D. or be near completion and exhibit high potential for outstanding teaching contributions in support of the School's various public and private programs. Candidates at the Senior Assistant, Associate, or Full Professor levels must have demonstrated research and teaching records consistent with the level of the appointment. Submit a cover letter, *cv*, three letters of recommendation, teaching evaluations and a sample publication electronically to: accountingrecruiting@business.queensu.ca This is an international search, open to candidates of all nationalities. In accordance with Canadian Immigration requirements, priority will be given to Canadian citizens and permanent residents. Queen's University is committed to employment equity and diversity in the workplace and welcomes applications from women, visible minorities, aboriginal people, persons with disabilities, and persons of any sexual orientation or gender identity. The academic staff at Queen's University are governed by a Collective Agreement (http://www.qufa.ca).

UNIVERSITY OF SCRANTON, Kania School of Management (AACSB) invites applications for its tenure-track position in Accounting Information Systems & Auditing beginning August 2012, at the rank of Assistant Professor. Duties include teaching at the undergraduate and graduate levels, scholarly research activities meeting AACSB-related criteria, and service. The primary teaching responsibilities will be in the areas of accounting information systems and auditing. An ability to teach writing-intensive and accounting communications courses is desirable. The qualifications for the position include an earned doctorate in the appropriate field from an AACSB-accredited institution. ABD with completion expected within 18 months of appointment will also be considered. The University of Scranton is, by tradition and choice, a Catholic and Jesuit university. The University is committed to developing a diverse faculty, staff, and student body and to modeling an inclusive campus community. Applications (including a cover letter, *vita*, transcripts, teaching evaluations, if available, and contact information for three references) may be submitted to: Dr. Douglas Boyle, University of Scranton, Kania School of Management, 320 Madison Avenue, Scranton, PA 18510-4602: or email to: accounting@scranton.edu. Applications will be considered until the position is filled. For more information about the Kania School of Management visit: http://matrix.scranton.edu/academics/ksom/index.shtml. The University of Scranton is an Equal Opportunity/Affirmative Action Employer/Educator. Veterans, minority persons, women, and persons with disabilities are encouraged to apply.

SUNY COLLEGE AT ONEONTA, Division of Economics and Business invites applications for a tenure-track position as an Assistant Professor beginning September 2013. The expectations include teaching, research, student advisement, college service, and continuing professional development. SUNY College at Oneonta is a comprehensive, public, liberal arts and sciences college with 6,000 students and 500 faculty. The College is ranked ninth among the best regional universities by *U.S. News & World Report*. The Division is AACSB-accredited. Please visit: http://www.oneonta.edu or http://www.oneonta.edu/econbus to learn more. Preference will be given to candidates who have experience with diverse populations, and/or teaching pedagogies and/or multicultural teaching experience. For a complete description of this position go to: http://www.oneonta.edu/employment. To apply online, go to: http://oneonta.interviewexchange.com/candapply.jsp?JOBID=32756. For other employment and regional opportunities, please visit our website at: http://www.oneonta.edu/employment. SUNY College at Oneonta values a diverse college community. Please visit our website on diversity at: http://www.oneonta.edu/home/diversity.asp. Moreover, the College is an EO/AA/ADA Employer. Women, persons of color, and persons with disabilities are encouraged to apply.

KUTZTOWN UNIVERSITY, Department of Business Administration announces a tenure-track faculty position to teach Accounting, beginning in Fall 2013, subject to final budget approval. Kutztown University is located in a beautiful semi-rural area two hours from New York City and one hour from Philadelphia. Kutztown University is one of 14 universities that comprise the Pennsylvania State System of Higher Education. As part of an energetic faculty you will be teaching undergraduate accounting courses with the opportunity to teach in the M.B.A. program. Faculty are evaluated for tenure and promotion based on their contributions in three areas: (1) teaching, (2) scholarly activity, and (3) service. Ph.D. or D.B.A. in Accounting or a related area is preferred; PQ candidates considered. ABD in Accounting with expected completion within one year will also be considered. Interested applicants should respond to the address below and include the following information: (1) an up-to-date *curriculum vitae*; (2) transcripts for all academic work completed (official transcripts are required before hiring); (3) names and contact information of at least three references (letters from the referees will be required before interview); and (4) a cover letter which clearly outlines the applicant's teaching and research interests. This information should be sent to: Dr. Therese A. Maskulka, Chairperson, Accounting Faculty Search, Department of Business Administration, Kutztown University, PO Box 730, Kutztown, PA 19530. Review of applications will begin on August 27, 2012 and will continue until the position is filled.

LEHIGH UNIVERSITY seeks well-trained research scholars/teachers with strong teaching, research, communication, and interpersonal skills for one open position at the Assistant Professor rank to join its Accounting Department faculty in Fall 2013. Candidates must be able to teach financial accounting, managerial accounting, and information system courses both at the undergraduate (introductory and advanced) and graduate levels, publish related research, able to work in teams with collegial attitude, provide service, and engage in other activities commensurate with our mission. A dynamic accounting department, a world-class undergraduate accounting program, a successful M.S. in Accounting and Information Analysis program, the 31st best undergraduate business school according to Bloomberg *BusinessWeek*'s annual rankings are reasons to consider Lehigh. Applicants must submit a letter of interest, *curriculum vitae*, three letters of reference, and dissertation proposal or other scholarly work to: https://academicjobsonline.org/ajo/Lehigh/Department%20of%20Accounting. For additional information contact: Dr. Parveen P. Gupta, Chair, Department of Accounting, Lehigh University, 621 Taylor Street, Bethlehem, PA 18015; Email: ppg0@lehigh.edu; Phone: (610) 758-3443. Lehigh University is an Equal Opportunity/Affirmative Action Employer. Qualified women and minority candidates are especially encouraged to apply. Lehigh offers excellent benefits including domestic partner benefits. Visit: http://www.lehigh.edu/~inprv/faculty/worklifebalance.html

VICTORIA UNIVERSITY OF WELLINGTON, School of Accounting and Commercial Law, Wellington, New Zealand seeks to appoint a Professor of Accounting with an outstanding record of teaching and research and a record of establishing successful relationships with professional and practitioner groups. Applicants for the position must hold a Ph.D. and have strong quantitative skills to engage in and supervise research of an empirical nature. Apply at: http://www.victoria.ac.nz/sacl.

SOUTHERN ILLINOIS UNIVERSITY EDWARDSVILLE invites applications for a tenure-track position beginning in August 2013. A Ph.D. in Accounting is required, but ABDs nearing completion will be considered. Candidates should have a teaching specialization in the area of financial accounting or taxation but other specialties will be considered. Candidates must be committed to excellence in teaching and scholarly research. The Department of Accounting offers AACSB-accredited programs at both the undergraduate and master's levels. Send *vita* to: Michael L. Costigan, Chair, Department of Accounting, Southern Illinois University Edwardsville, Campus Box 1104, Edwardsville, IL 62026-1104; Email: mcostig@siue.edu. SIUE is a state university. Benefits under state-sponsored plans will not be available to holders of F1 or J1 visas. Applicants will be subject to a background check prior to an offer of employment. Southern Illinois University Edwardsville is an Equal Opportunity Employer and will not discriminate against any person on the basis of race, religion, national origin, or sex in violation of Title VII. Southern Illinois University Edwardsville prohibits discrimination against employees, applicants for employment, and students on the basis of age, color, disability, marital status, national origin, race, religion, sex, sexual orientation, or veteran's status.

STANFORD UNIVERSITY, Graduate School of Business invites applications for tenured and non-tenured faculty positions in accounting, effective September 1, 2013, from candidates at all levels and stages of their careers who are interested in financial accounting, managerial accounting, tax, and other areas. Candidates should have or expect to complete a Ph.D. by September 1, 2013, and should possess a strong and active research record. The successful candidate will be expected to conduct research and to teach both M.B.A. and Ph.D. courses in accounting. Applications should include *cv* and copies of research publications. Applicants are strongly encouraged to submit their applications electronically by visiting: http://www.gsb.stanford.edu/recruiting, and uploading their *curriculum vitae*, research papers and publications, and teaching evaluations, if applicable, to that site. Alternatively, all materials may be sent by email to: faculty_recruiter@gsb.stanford.edu, or by postal mail (non-returnable) to: Office of Faculty Recruiting, Box A, Graduate School of Business, Stanford University, 655 Knight Way, Stanford, CA 94305. However, submissions via email and postal mail can take 4–6 weeks for processing. For an application to be considered complete, all applicants must have three letters of recommendation emailed to the preceding email address, or sent via postal mail. The application deadline is December 1, 2012. Stanford University is an Equal Opportunity Employer and is committed to increasing the diversity of its faculty. It welcomes nominations of and applications from women and members of minority groups, as well as others who would bring additional dimensions of diversity to the University's research, teaching, and clinical missions.

UNIVERSITY OF HOUSTON, Department of Accountancy and Taxation, C. T. Bauer College of Business, invites applications for tenure-track appointments at all ranks for Fall 2013. Applicants for senior positions should have a national reputation for scholarship, and a record of excellence in teaching at the undergraduate and graduate levels. Applicants for junior positions must have a doctoral degree prior to appointment date and exhibit strong potential for and commitment to excellence in research and teaching. We are seeking applications in all areas of Accounting. The C. T. Bauer College of Business and the Department are AACSB-accredited. Salaries are very competitive. Application procedure: To be considered, interested applicants should submit the following documents electronically: (1) current *curriculum vitae*; (2) statement of teaching and research experience; and (3) list of 3 references with contact information to: Search Committee, Department of Accountancy & Taxation, University of Houston, C. T. Bauer College of Business, 334 Melcher Hall, Houston, Texas 77204-6021. Review of applications will begin immediately and applications will be accepted until the position is filled. For a full description of the position and details of the application process, please visit: https://www.bauer.uh.edu/recruitacct. To learn more about C. T. Bauer College of Business, Department of Accountancy and Taxation, please visit our website at: http://www.bauer.uh.edu/departments/accy/index.php. The University of Houston is an Equal Opportunity/Affirmative Action Employer. Women, minorities, veterans and individuals with disabilities are encouraged to apply.

UNIVERSITY OF NEW ORLEANS invites applications for an anticipated Assistant Professor position for Fall 2013. Preference will be given to those with teaching interests in some combination of financial, auditing, and systems at both the undergraduate and graduate levels. Candidates must have the Ph.D. with an accounting major. Professional certification is preferred. Individuals must have a commitment to scholarly research and quality teaching. Preference will be given to those with teaching interests in some combination of financial, auditing, and systems at both the undergraduate and graduate levels. Salary is negotiable and competitive. The Department offers the undergraduate accounting degree (about 500 majors) and two Master's degrees (including a Tax Option) with about 100 students. The Department has separate AACSB accounting accreditation at both the graduate and undergraduate levels. Candidates should send a resume to: Professor Philip J. Harmelink, Chairman, Department of Accounting, College of Business Administration, University of New Orleans, New Orleans, LA 70148; Email: pharmeli@uno.edu. The University of New Orleans is a member of the University of Louisiana System and an Affirmative Action/Equal Opportunity Employer.

YESHIVA UNIVERSITY, Sy Syms School of Business invites applications for a tenure-track faculty appointment in Accounting at the experienced Assistant Professor or Associate Professor levels beginning in the Fall 2013. Salary is competitive. The Sy Syms School of Business (Syms) operates on two campuses in Manhattan (New York City) and is a part of Yeshiva University, a private institution of higher learning consistently ranked among the top 50 U.S. universities by *US News & World Report*, which includes, in addition to Yeshiva and Stern colleges, Syms, the Cardozo Law School, and the Einstein Medical center. Syms offers business and accounting programs for undergraduate men and women, the Master of Science in Accounting, and is launching an Executive M.B.A. program in August 2012. We expect to expand our graduate programs within the next few years. Syms is in the process of seeking accreditation by the AACSB. The Business School was established in 1987 through major gifts by Sy Syms and other business leaders. For over 25 years the Business School has been providing its students with relevant skills and knowledge in business, leading them to satisfying careers, and helping them maximize their potential. The school expects to hire several research active faculty in different departments during this and the next hiring cycle. To learn more about the Sy Syms School of Business and its faculty visit: http://www.yu.edu/syms/. Application Procedure: Please direct inquiries and submit electronic materials (*vita*, cover letter with a statement of teaching and research interests, three references, and teaching evaluations) to: Ms. Billie Coleman, Department Administrator at: bcoleman@yu.edu. Review of applications will begin by August 1st and continue until the position is filled. Apply for this Job: Contact: Billie Coleman, Phone: (212) 960-5400, x 5581; Email: bcoleman@yu.edu. Job Requirements Qualifications: Applicants must possess an earned Ph.D. from a currently AACSB-accredited university. Applicants must demonstrate a record of high-quality research including published refereed papers/accepted papers from quality journals, and papers in progress. Applicants must also provide recent evidence of high-quality teaching. Excellent oral and written communication skills are required. Candidates must be eligible for employment in the United States. Professional certification is a plus but is not required.

UNIVERSITY OF MARYLAND AT COLLEGE PARK, Accounting and Information Assurance Department, Smith School of Business seeks applicants for two non-tenure-track teaching positions. Each position is for nine months beginning mid-August 2013, and is renewable subject to performance. Candidates must possess a Ph.D. (or J.D. and L.L.M.) and an excellent teaching record. The department is particularly interested in individuals with expertise in teaching auditing or managerial accounting (other teaching areas will be considered). Certain service responsibilities will be expected. For full consideration, applicants must apply online at: http://jobs.umd.edu, submit a letter expressing their interest, along with a *curriculum vitae*, and evidence of teaching excellence by October 15, 2012. The University of Maryland at College Park actively subscribes to a policy of equal employment opportunity, and will not discriminate against any employee or applicant because of race, age, sex, color, physical or mental handicap, national origin, or political affiliation.

ROWAN UNIVERSITY, Rohrer College of Business seeks applicants for a position as Assistant/Associate Professor, Accounting, FT/tenure-track. Description: Responsibilities include teaching undergraduate and graduate courses in Accounting subject areas including core accounting courses, financial, and managerial accounting. An interest and background in the application of information systems/technology to accounting practice is desirable. Active research agenda in the discipline and in support of quality teaching, participating in learning assurance activities, active engagement in department, college, university, and professional and outreach activities, and advising is also required. Qualifications: Ph.D. in Accounting by August 31, 2013 is required. Previous college teaching experience is desirable. Starting Date: September 1, 2013. Deadline for applications is October 15, 2012. The applicant should submit a letter of interest, *curriculum vita*, student evaluations, sample research publications intellectual contributions, research agenda, and names and telephone numbers of three references to: Carol Welsh, Chairperson Accounting and Finance Department, William G. Rohrer College of Business, Rowan University, 201 Mullica Hill Road, Glassboro, NJ 08028-1701; Email: welsh@rowan.edu.

UNIVERSITY OF MISSOURI–ST. LOUIS, College of Business Administration, Department of Accounting invites applications for the position of Assistant or Associate Professor of Accounting, beginning August 2013. Applicants must possess or be nearing completion of a doctoral degree in Accounting, awarded from an AACSB-accredited institution, and have a strong commitment to excellence in both teaching and research. We welcome candidates with research interests in any area of accounting, with particular preference for candidates with teaching experience and interest outside of financial accounting. We seek a colleague who will actively pursue scholarly research and enthusiastically approach teaching at both the undergraduate and graduate levels. Salary and benefits are competitive. The University of Missouri–St. Louis is one of the four campuses of the University of Missouri system and serves a diverse student population, reflective of the city of St. Louis, visit: http://www.stlrcga.org. The AACSB-accredited College of Business Administration offers undergraduate and graduate degrees through a variety of programs and in a variety of business disciplines. The Accounting program includes a Bachelor of Science in Accounting and a Master of Accounting. Qualified applicants should submit a letter of interest and a current *vita*, including evidence of teaching success, electronically to: Jennifer Reynolds-Moehrle, Associate Professor of Accounting at: moehrlej@umsl.edu, with subject line reference to 2013POSITION. EOE/ADA/AA

SOUTHERN ILLINOIS UNIVERSITY CARBONDALE, School of Accountancy is seeking an individual who holds an earned doctorate, or appropriate terminal degree, in accounting. Candidates are expected to complete all requirements for the Ph.D. before the date of appointment of 8/16/13. Research interests and expertise in Information Systems Accounting is preferred but quality applicants in other areas of accounting will be considered. Professional certification is preferred. The successful candidate must have a strong commitment to a high-quality research program. Candidates must also have a record of good teaching. Applicants for Advanced Assistant Professor are strongly encouraged to apply and must have a strong publication record in high-quality journals, and evidence of teaching excellence. For more details visit: http://www.siu.edu/jobs. Submit letter of application, *vita*, and contact information for three references to: eod@business.siu.edu; or by post mail to: Dr. Ed O'Donnell, Search Committee Chair, School of Accountancy, COB-100, Mail Code 4631, Southern Illinois University Carbondale, 1025 Lincoln Drive, Carbondale, IL 62901; Phone: (618) 453-2289; Fax: (618) 453-1411. Review of applications will begin August 16, 2012 and continue until the position is filled. SIU Carbondale is an Affirmative Action/Equal Opportunity Employer and strives to enhance its ability to develop a diverse faculty and staff and to increase its potential to serve a diverse student population. All applications are welcomed and encouraged and will receive consideration.

SOUTHEAST MISSOURI STATE UNIVERSITY is seeking applicants for Chairperson of the Department of Accounting available Fall 2013. Provide dynamic collegial leadership to a successful, progressive department with a dedicated faculty and staff. Fulfill the role and responsibilities of a chairperson as defined in the Faculty Handbook, including officially representing the department's on- and off-campus interests, evaluating faculty and staff, managing budgets, providing curricular leadership, and ensuring that the needs of students are met. Teaching in the department will be 6 credit hours per regular term. Requirements include a Ph.D. or D.B.A. in accounting, CPA, and a record of accomplishments consistent with appointment as a tenured Associate or Full Professor. Full position description available at: http://www.semo.edu/hr/employment/faculty_employment.htm. To apply, submit an electronic application via email to: facultyjobs@semo.edu. The application should include letter of interest addressing position requirements and how you learned of this position, current *curriculum vitae*, unofficial copies of transcripts, and name and current contact information for at least three references. To ensure full consideration, applications must be received by October 1, 2012.

NORTHEASTERN UNIVERSITY is seeking applications for tenure-track positions effective September 2013 at the Assistant or Associate Professor rank. Candidates must have or expect to have a doctorate in accounting or a closely related business field before the start date and be committed to excellence in research and teaching. The Accounting Group at Northeastern is nationally recognized for its high-quality behavioral accounting research focused on auditing, governance, tax, and managerial and ethical judgment and decision making. We provide a supportive environment where collaboration in research and teaching is encouraged. The position offers competitive compensation and teaching responsibilities that enable a significant level of high impact research. Rank and salary are dependent on qualifications. Summer funding, graduate assistants, and travel support are available. Accounting courses are offered at both the undergraduate and at the graduate levels (M.B.A., M.S./M.B.A., M.S.A., and M.S.Tax). For consideration, submit your cover letter, *cv*, and two samples of scholarly work to: http://www.cba.neu.edu/faculty/ and clicking on "Faculty Positions" link on the right-hand side. Northeastern University (Boston, Massachusetts) is an Affirmative Action/Equal Opportunity/Title IX Employer and welcomes applications from minorities, women, and disabled persons.

EDHEC seeks candidates who have a strong record of publication including papers in FT-ranked journals, will participate in the research work of EDHEC's Financial Analysis and Accounting Research Centre, and whose major interests are company valuation, determination of risk premium (including accounting), impact of IFRS standards on financial statements (perception of risks by account users and business management), performance management, and risk pricing. The successful candidate will also have some responsibility for teaching and course design in the academic and executive education programs of Groupe EDHEC. Contact Paul Klumpes at: paul.klumpes@edhec.edu; Phone: 00333201544530.

ILLINOIS STATE UNIVERSITY, Department of Accounting invites applications for a full-time, tenure-track Assistant or Associate Professor position in accounting. The Department of Accounting is AACSB-accredited and provides a balanced emphasis on quality teaching, research, and service. Candidates are expected to demonstrate the potential for excellence in teaching and scholarly research. Qualifications include a Ph.D. or D.B.A. in Accounting. A degree from an AACSB-accredited school is preferred. ABDs with substantial progress that are close to degree completion will be considered. Professional certification and work experience in accounting are desirable. The successful candidate will have a primary teaching interest in undergraduate and graduate courses in taxation. The normal teaching load is 9 credit hours per semester. Salary is commensurate with qualifications and experience. The selected applicant will begin August 16, 2013. To ensure full consideration, please send a formal letter of application and current *vita* to: Gerry McKean, Interim Chairperson, Illinois State University, Department of Accounting, Campus Box 5520, Normal, IL 61790-5520; Email: gerry.mckean@ilstu.edu. Illinois State is an Equal Opportunity/ Affirmative Action university and encourages diversity.

WESTERN NEW ENGLAND UNIVERSITY invites applications for a tenure-track Assistant/Associate Professor accounting position to begin August 2013. Candidates are expected to hold a doctorate in accounting or anticipate having completed all degree requirements by the starting date. The successful candidate will demonstrate a strong commitment to teaching in the undergraduate and graduate programs and to publishing scholarly research. An interest in teaching financial accounting or auditing is desirable. Western New England University is a private, independent, coeducational institution founded in 1919 and located on an attractive 215-acre suburban campus in Springfield, Massachusetts within easy access of Boston and New York City. More information about the University is available at: http://www.wne.edu. Send *vita*, teaching evaluations, research manuscript, and reference information to: Dr. Julie Siciliano, Dean, College of Business, Western New England University, 1215 Wilbraham Road, Springfield, MA 01119. Electronic submissions are encouraged and should be sent to: lori.fenton@wne.edu. Western New England University is an Equal Opportunity Employer.

IDAHO STATE UNIVERSITY, College of Business seeks a full-time tenure-track Assistant or Associate Professor or a non-tenure-track Clinical Assistant Professor in its Department of Accounting for a position available for the 2013–2014 academic year. Salary will be commensurate with experience and qualifications. Both the College of Business and the Accounting Program are accredited by AACSB. Our accounting faculty support excellent undergraduate, M.B.A., and Master of Accountancy (M.Acc.) programs. Responsibilities: The faculty member will teach at the undergraduate and graduate levels. Any emphasis area(s) except tax considered. Normal course load includes two preps and three sections per semester for AQ and four sections per semester for PQ. Peer-reviewed scholarly work, service, and research are expected of candidates for an academically qualified tenure-track position. Heavy professional experience and continuing professional involvement is expected of candidates for a professionally qualified non-tenure-track appointment. The successful candidate will demonstrate a collegial attitude, excellent communication skills, and an eagerness to support our students through a mentoring/advising process. Minimum Qualifications: Ph.D. (ABD considered if near completion) and research promise indicating an ability to conduct peer-reviewed scholarly work for a tenure-track academically qualified (AQ) position. Significant high-level accounting experience and professional certification required for a non-tenure-track professionally qualified (PQ) position. Apply URL: https://isujobs.net/applicants/jsp/shared/position/JobDetails_css.jsp?postingId=138603 Contact: Dr. Bob Picard at: picarobe@isu.edu.

DRAKE UNIVERSITY, College of Business and Public Administration, School of Accounting, invites applications for an Assistant/Associate Professor of Accounting, starting in Fall 2013, teaching primarily financial or managerial accounting at the undergraduate and graduate level. The normal teaching requirements are 9 credit hours per semester. Demonstrated effectiveness in teaching and continuing professional development is expected. A doctorate in Accounting at time of appointment is preferred, but ABD candidates close to completion will be considered. Candidates must also be able to provide evidence of demonstrated teaching effectiveness at the university-level and continuing professional development. Candidates for the Associate level must show a record of scholarly publications and an active research stream. Salary is competitive with peer AACSB institutions. Drake University is a private, comprehensive university dedicated to quality teaching. It is located in Des Moines, Iowa, with a metropolitan population over 500,000. Enrollment is selective and limited to approximately 5,500 students in the schools and colleges of the University. Both the CBPA and School of Accounting hold AACSB-accreditation. Send application letter with current *vita* listing three references to: Dr. Stephen Gara, Search Committee Chair, School of Accounting, Drake University, 2507 University Avenue, Des Moines, IA 50311: Email: Stephen.Gara@drake.edu; Website: http://www.drake.edu/hr/. Drake University is an Equal Opportunity Employer.

PORTLAND STATE UNIVERSITY seeks applicants for a full-time tenure-track position as an Assistant or Associate Professor September 16, 2013. The successful candidate will be expected to (1) publish in refereed publications, (2) develop and teach courses in taxation or financial accounting at the undergraduate and/or graduate levels, and (3) perform appropriate service activities. Salary is competitive. Qualifications: The successful candidate for this job will provide current evidence of an on-going research program and hold a doctoral degree with an emphasis in accounting from an AACSB-accredited institution or hold both a CPA and J.D. Advanced ABD candidates will be considered but must possess a doctoral degree by September 1, 2013. Preference given to applicants whose research is complementary with that of existing faculty and to those candidates with academic or professional work experience. Submit a letter of interest, *curriculum vitae*, one or two representative papers, a summary of teaching evaluations or other measures of teaching effectiveness, and contact information for three references. Email correspondence (preferred) should be directed to: Professor Darrell Brown at: AccountingPosition@pdx.edu; Postal: Professor Darrell Brown, School of Business Administration, Portland State University, PO Box 751, Portland, OR, 97207. Portland State University is an Affirmative Action/Equal Opportunity Institution and welcomes applications from diverse candidates and candidates who support diversity.

ALABAMA STATE UNIVERSITY, College of Business Administration, Department of Accounting and Finance, invites applications for the position of Assistant/Associate Professor of Accounting. This is a nine-month tenure-track position. The successful candidate will teach undergraduate- and graduate-level courses, engage in curriculum development and research activities, advise students, and participate in university and community service. An earned Ph.D. degree in Accounting from an accredited institution or ABD within one year of completion is required. The candidate must have demonstrated excellence in teaching and scholarship. Preferred Education and Experience: Professional accounting certifications and business experience are a plus but not required. How to Apply: Applicants should apply online and submit materials including a cover letter and *curriculum vita*, using the Alabama State University application portal at: https://www.governmentjobs.com/js_login.cfm?&TopHeader=alasu&.Additional Information regarding the College and the Department can be found at: http://coba.alasu.edu; or by contacting: Dr. Gow-Cheng Huang, Phone: (334) 229-6920; Email: ghuang@alasu.edu. EEO/AA Statement: Alabama State University is an Equal Opportunity Employer and an E-Verify participant.

UNIVERSITY OF WISCONSIN–WHITEWATER, Accounting Department invites applications for multiple full-time tenure-track Assistant Professor positions in the areas of Tax, Financial, and Managerial Accounting beginning Fall 2013. Applicants should have a doctoral degree in Accounting, however, applicants with doctoral degrees in closely related fields and ABDs will be considered. The College of Business and Economics is AACSB-accredited so applicants must be academically qualified under those standards. Preference will be given to applicants with CPA certification and accounting practical experience. The UWW Accounting Department strives to be among the best regional public comprehensive university accounting departments by focusing on quality teaching, a solid research portfolio, and service. The complete position announcement is at: http://www.uww.edu/employment/unclassified.html For more information about our department go to: http://www.uww.edu/cobe/accounting/. To apply, send a letter of application, resume, contact information for three references, and evidence of teaching effectiveness. Original transcripts will be required of finalists. Application materials should be emailed to: Joanna Stradinger at: stradinj@uww.edu; or mailed to: Dr. Carol J. Normand, Search and Screen Committee Chair, Attn: Joanna Stradinger, Accounting Department, University of Wisconsin–Whitewater, 800 W. Main Street, Whitewater, WI 53190-1790. Only complete applications will be considered. Applications received by October 1, 2012 are ensured consideration. Positions will remain open until filled. UW–Whitewater is an Equal Opportunity/Affirmative Action Employer.

BOWLING GREEN STATE UNIVERSITY (BGSU) invites applicants for one tenure-track position in Accounting at the Assistant or Associate Professor rank beginning in August 2013. We are looking for one individual to support our undergraduate specializations in Accounting and Information Systems Auditing and Control, as well as our Master of Accountancy and M.B.A. programs. Preference will be given to candidates with teaching and research interests in financial accounting/reporting, auditing, and accounting information systems. Ph.D. in business with a concentration in accounting preferred. Professional experience is also highly valued. At the Assistant Professor rank, individuals who expect completion of all Ph.D. requirements by August 2013 are preferred, but the committee will consider candidates who expect to complete all degree requirements during 2013–2014. Potential or demonstrated commitment to teaching excellence, research, and service is expected with an established record being required for appointment at the Associate rank. BGSU offers a competitive salary package based upon experience and accomplishments. Applicants should send a current resume, three letters of recommendation, and a transcript from your doctoral program to: Dr. David M. Stott, Chair, Department of Accounting and MIS, Bowling Green State University, Bowling Green, OH 43403-0262. Review of applications will begin on July 15, 2012 and will continue until the positions are filled. The committee will not accept electronic or fax submissions of applications and materials. The availability of these positions is subject to final budget approval. Information on departmental programs and faculty can be reviewed at: http://www.cba.bgsu.edu/amis. BGSU is an Equal Opportunity/Affirmative Action Employer and encourages applications from women, minorities, veterans, and individuals with disabilities.

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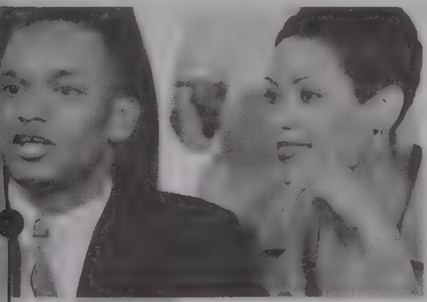

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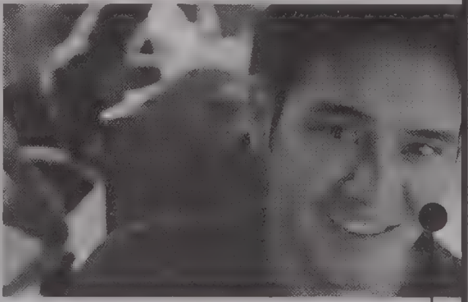

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Home Country Tax System Characteristics and Corporate Tax Avoidance: International Evidence

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ABSTRACT: We examine whether three tax system characteristics—required book-tax conformity, worldwide versus territorial approach, and perceived strength of enforcement—impact corporate tax avoidance across countries after controlling for firm-specific factors previously shown to be associated with tax avoidance (i.e., performance, size, operating costs, leverage, growth, the presence of multinational operations, and industry) and for other cross-country factors (i.e., statutory corporate tax rates, earnings volatility, and institutional factors). We find that, on average, firms avoid taxes less when required book-tax conformity is higher, a worldwide approach is used, and tax enforcement is perceived to be stronger. However, the relations between tax avoidance and all three tax systems characteristics are contextual and depend on the extent to which management compensation comes from variable pay, including bonuses, stock awards, and stock options.

Keywords: *tax avoidance; tax systems; book-tax conformity; managerial incentives.*

Data Availability: *Data are available from sources identified in the text.*

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I. INTRODUCTION

We examine the impact of three tax system characteristics—required book-tax conformity, worldwide versus territorial approach, and perceived strength of tax enforcement—on corporate tax avoidance across countries. Prior research identifies firm-specific determinants of cross-sectional variation in tax avoidance within a given country but few studies examine the impact of tax system characteristics on tax avoidance, primarily because all firms in a given country are subject to the same tax system and because changes to tax systems are infrequent.[1] We fill this void by examining tax avoidance across countries and we focus on the effects of the firm's home-country tax system characteristics.[2]

We follow Hanlon and Heitzman (2010) and define corporate tax avoidance broadly as the reduction in explicit taxes paid, and we measure tax avoidance as the difference between the firm's "unmanaged tax amount" (the home-country statutory corporate tax rate times pre-tax earnings before exceptional items) and its "managed tax amount" (current taxes paid). This difference reflects how aggressively managers pursue strategies that reduce taxes paid. As in Dyreng et al. (2008), our measure of corporate tax avoidance does not necessarily imply that firms are engaging in improper behavior since managing tax costs is a necessary and appropriate component of a firm's long-term strategy.[3] Moreover, since CEOs have incentives to both increase earnings and lower taxable income, earnings management and tax avoidance are like two sides of the same coin. Thus, in our tests, we do not distinguish earnings management from tax avoidance. Rather, we assume that managers' decisions simultaneously consider the effects of planning strategies on both book income (i.e., financial earnings) and taxable income.[4]

Our study is motivated by calls for corporate tax reform in the U.S., specifically, proposals to increase required book-tax conformity (Desai 2005; Joint Committee on Taxation [JCT] 2006; Whitaker 2006), adopt a territorial approach (Avi-Yonah 2010; Hines 2011; Hufbauer 2011), and increase enforcement actions (Shulman 2009; Department of the Treasury [DOT] 2011; Keener 2011), and by calls for more research on factors influencing tax avoidance (Shackelford and Shevlin 2001; Graham 2003; Dyreng et al. 2008; Hanlon and Heitzman 2010). Understanding the impact of tax system characteristics on corporate tax avoidance is important to governments in designing tax systems and to investors in interpreting differences in firms' corporate tax avoidance across countries.

We conduct our empirical analyses using a sample of more than 69,000 firm-year observations from 22 countries from 1995 through 2007. We regress our measure of corporate tax avoidance (*TaxAvoid*) on home-country tax system characteristics (i.e., required book-tax conformity, worldwide versus territorial approach, and perceived strength of tax enforcement) while controlling for statutory corporate tax rates as well as various country- and firm-specific variables that have been shown to influence tax avoidance in prior research. Results from our empirical analyses reveal that tax avoidance is lower, on average, for firms in home-countries with higher required book-tax conformity, a worldwide (rather than territorial) approach, and stronger perceived tax enforcement.

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[1] See Hanlon and Heitzman (2010) for a recent review of the tax avoidance literature.

[2] We use the term "home country" to refer to the country in which the parent is incorporated. The home-country tax system plays a primary role in determining how much tax a firm pays because it dictates the final layer of tax that the firm must pay on any repatriated earnings before they can be paid out as dividends to shareholders.

[3] The terms "tax avoidance," "tax aggressiveness," "tax sheltering," "tax noncompliance," and "tax evasion" are all used in academic literature to describe various aspects of tax avoidance. We use the term "tax avoidance" to refer to all reductions of taxes paid below the "unmanaged tax amount." As such, we do not differentiate between actions that would be considered prudent tax planning versus those that would be considered abusive tax sheltering.

[4] Consistent with the idea that earnings management and tax avoidance decisions are made concurrently, Desai (2005) argues that the degradation of both financial earnings and taxable income stems from an increase in managers' ability to exploit differences between financial and tax accounting rules by structuring transactions to create advantageous book-tax differences (e.g., using derivatives, special purpose entities, etc.). Likewise, Hanlon and Heitzman (2010) assert that differences between financial earnings and taxable income are driven by a wide variety of factors and that these factors likely operate simultaneously to generate the level of book-tax differences that a firm reports.

We further investigate whether the associations between tax avoidance and tax system characteristics differ when the variable pay portion of management compensation (*VarComp*) is high (above the median) versus low (below the median). We suggest that managers have stronger incentives to increase after-tax returns (including lowering taxes) when the variable pay component of management compensation is high. Consistent with this, we find that the association between tax avoidance and required book-tax conformity is less negative when the variable portion of management compensation is high. Moreover, the negative relation between tax avoidance and the perceived strength of tax enforcement is driven by firms in countries where the variable portion of management compensation is high. Finally, the worldwide tax approach is associated with lower tax avoidance only when the variable portion of management compensation is low. These results provide evidence that the influence of tax system characteristics on tax avoidance depends on the strength of management's incentives to avoid taxes.

Overall, our results contribute to the literature that investigates cross-sectional variation in tax avoidance. We extend prior research, which generally focuses on firms within a single country, by providing evidence that cross-country differences in tax systems as well as firm-specific factors impact the extent to which firms avoid taxes. Our study should be of interest to tax policymakers concerned about declining corporate tax revenues and the increasing gap between reported earnings and taxable income since they suggest that increasing tax enforcement is likely to result in lower tax avoidance as proponents argue (Shulman 2009; DOT 2011; Gravelle 2011; Hufbauer 2011; Keener 2011; Zrust 2011). However, our results also suggest that, while proposals to require greater book-tax conformity (Desai and Dharmapala 2009) may reduce tax avoidance, incentives underlying management compensation contracts may mitigate these effects. Indeed, our results suggest that increasing book-tax conformity is unlikely to significantly reduce tax avoidance unless managers' variable compensation, as a percentage of total compensation, is also reduced.

Our results should also be of interest to investors in global markets. Prior research suggests that information about tax expense and tax disclosures in the financial statements may provide information about current and/or future earnings (Hanlon and Heitzman 2010). Understanding how tax system characteristics impact tax avoidance should help investors to better interpret the tax information contained in the financial statements when comparing firms across countries.

The remainder of this paper is organized as follows. Section II discusses prior research and develops our hypotheses, while Section III describes our research design. Section IV presents our sample selection, descriptive statistics, and test results. Section V describes robustness tests and Section VI concludes.

II. PRIOR RESEARCH AND DEVELOPMENT OF HYPOTHESES

Prior Research

Prior research documents considerable cross-sectional variation in tax avoidance and identifies firm-specific factors that explain this variation.[5] Cross-sectional differences in tax avoidance are influenced by firm-level factors such as leverage (Graham and Tucker 2006; Joulfaian 2010; Lisowsky 2010), the scale of international operations (Rego 2003; Dyreng and Lindsey 2009), and management incentives (Phillips 2003; Hanlon et al. 2007; Rego and Wilson 2008; Gaertner 2010; Robinson et al. 2010; Armstrong et al. 2012).

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[5] Shackelford and Shevlin (2001) and Hanlon and Heitzman (2010) provide extensive reviews of prior research on tax avoidance, including studies of marginal tax rates, effective tax rates, book-tax differences, tax shelters, and uncertain tax benefits.

Prior studies primarily explore tax aggressiveness across firms within one country (and mainly within the U.S.), where all firms operate under the same tax system, financial accounting standards, and institutional factors. Although tax system characteristics likely have a strong impact on tax avoidance, these characteristics have little or no variation within countries. Thus, we examine whether three tax system characteristics—required book-tax conformity, worldwide versus territorial approach, and strength of tax enforcement—impact corporate tax avoidance across countries.

Development of Hypotheses

Our conceptual definition of tax avoidance is very broad. Specifically, we follow Hanlon and Heitzman (2010) and define tax avoidance as the reduction of explicit taxes. This definition includes tax avoidance strategies that create permanent or temporary book-tax differences as well as those that do not create any book-tax differences.[6] This definition also includes the full range of tax reduction strategies including those that are clearly legal (such as investing in tax-exempt bonds), those with uncertain tax benefits but for which minimal penalties would be assessed upon discovery by taxing authorities (such as U.S. tax return reporting positions that are undisclosed but are supported by substantial authority, or that are disclosed but have a reasonable basis), those for which more substantial penalties would be assessed but that would not be subject to criminal penalties (such as undisclosed U.S. tax return reporting positions that meet the reasonable basis standard), and those that are clearly illegal and could subject taxpayers to criminal penalties (such as abusive tax shelters and/or fraud).

We view tax system characteristics as determinants of differences in the availability of tax avoidance strategies, as well as determinants of differences in the expected costs and/or benefits of implementing tax avoidance strategies. We predict that these differences in costs and benefits will influence tax avoidance decisions even after controlling for corporate statutory tax rates and firm-specific characteristics identified in prior research.

The first tax system characteristic that we examine is the level of required book-tax conformity in the firm's home country. Firms in countries with higher required book-tax conformity have fewer opportunities to avoid taxes without decreasing reported earnings. Moreover, tax authorities act as an additional monitor of reported earnings when higher conformity between earnings and taxable income is required. Desai (2005) argues that these constraints (decreased opportunity and increased monitoring) on managers would result in less overall tax avoidance if required book-tax conformity were increased in the U.S.

Prior research suggests the amount of flexibility that firms have to report differences between taxable income and pre-tax earnings per the financial statements differs across countries (Ali and Hwang 2000; Hung 2001; Atwood et al. 2010; Lang et al. 2011). When constrained from taking tax positions that create differences between reported earnings and taxable income, managers may implement other tax-reducing strategies, such as locating more operations in tax havens or shifting more income into lower tax jurisdictions (JCT 2006). However, these alternative tax avoidance strategies are more costly to implement than strategies that create differences between reported earnings and taxable income. Thus, we conjecture that tax avoidance is more costly in countries that require more book-tax conformity than in countries that require less book-tax conformity, all else equal. For this reason, we predict a negative relation between tax avoidance and required book-tax conformity.

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[6] For example, the decision to locate a subsidiary in a foreign country with a lower statutory tax rate and shifting income to that subsidiary may reduce explicit taxes paid without creating a difference between pre-tax earnings and the sum of the firm's taxable incomes reported in the home country and in the foreign country. That is, taxes may be avoided without creating either permanent or temporary book-tax differences in the financial statements.

Our first hypothesis is formally stated in the alternative form as follows:

> **H1:** Firms in home countries with higher required book-tax conformity engage in less tax avoidance.

The second tax system characteristic that we examine is the use of a worldwide versus territorial tax system in the home country. For parent firms located in countries with worldwide tax systems, such as the U.S., income earned by foreign subsidiaries is potentially subject to additional tax in the home country when it is repatriated via dividend payments. This additional tax is not imposed on parent firms located in countries with territorial tax systems.

Under the territorial approach, income received from foreign subsidiaries in the form of dividend payments is partially or wholly excluded from the parent firm's taxable income. For example, assume a parent in Country Y has a subsidiary in Country X. This subsidiary earns $100 in pre-tax profits, pays $20 in Country X taxes, and remits an $80 dividend to the Country Y parent. If Country Y has a territorial tax system, then the dividend from the Country X subsidiary is excluded from the parent's taxable income. Thus, the consolidated firm as a whole pays only the Country X tax on income earned in Country X.

Under the worldwide approach, dividends received from foreign subsidiaries are included in the parent firm's taxable income but foreign income taxes paid can be claimed as either a deduction or a credit (subject to limitations) to mitigate the double taxation of foreign-source income. For example, assume a U.S. parent with a 35 percent statutory corporate tax rate has a subsidiary in Country X, and the subsidiary earns $100 in pre-tax profits, pays $20 in Country X taxes, and remits an $80 dividend to the U.S. parent. The U.S. parent includes the $100 of pre-tax Country X earnings in its U.S. taxable income, which increases U.S. taxes due before credits by $35 ($100 × 0.35), but it receives a $20 foreign tax credit. Thus, the U.S. parent must remit the $15 difference between the U.S. tax and the Country X tax to the U.S. tax authorities. As a result, the after-tax income from an investment in Country X is higher for multinationals with parent firms located in countries using the territorial approach than for multinationals with parent firms located in countries using the worldwide approach.

Proponents of adopting a territorial approach in the U.S. claim that U.S. multinationals are at a competitive disadvantage, relative to multinationals in other countries, because of the additional taxes imposed in a worldwide tax system (Hines 2006; Hubbard 2006; Ousterhuis 2006). However, opponents of adopting a territorial approach in the U.S. argue that U.S. multinational firms have a number of options available to minimize or eliminate the perceived disadvantage of the worldwide tax system. For example, firms can defer paying U.S. tax by delaying payments of dividends to the U.S. parent. This deferral can be indefinite if the earnings are reinvested in the foreign country in which they are generated or invested in other foreign countries. When earnings are designated as permanently reinvested abroad, the potential future tax liability is also not recorded in the financial statements (Accounting Standard Codification Section 740). Consistent with this view, Markle (2010) finds no difference in income shifting among foreign affiliates by firms in worldwide tax systems that consistently reinvest earnings abroad versus firms in territorial tax systems. In addition, Lokken (2006) and Mandolfo (2007) argue that U.S. multinationals have refined international tax planning "to a fine art" (using deferral provisions, tax havens, hybrid entities, transfer pricing, cost-sharing arrangements, cross-crediting, etc.) so that they actually pay less in taxes than they would under a territorial approach. Fleming et al. (2008) further suggest that foreign tax credits go beyond what is required to eliminate double taxation and effectively result in a home-country subsidy on the activity that produces the foreign-source income. Thus, multinational firms in home countries with worldwide tax systems may have the ability to use international tax-planning techniques to achieve levels of tax avoidance that are at least as high as those of multinational firms resident in countries that use territorial tax systems.

Hicks et al. (2009) argue that parent firms in countries with territorial tax systems can achieve greater tax benefits from shifting income to low-tax jurisdictions because the dividends received from foreign subsidiaries are permanently exempt from tax in the home country, whereas in countries with worldwide tax systems, shifting income merely defers home-country tax. Consistent with this, Markle (2010) finds that multinational firms with home-country territorial tax regimes shift more income among their foreign affiliates overall than do firms with home-country worldwide tax regimes. This result, combined with the Hicks et al. (2009) point that tax savings from income shifting are permanent under territorial tax systems but are merely deferred under worldwide tax systems, suggests that less tax avoidance is possible for firms in home countries with worldwide tax systems. Therefore, we predict that tax avoidance will be lower among firms in home countries using a worldwide approach compared with firms in home countries using a territorial approach.

Our second hypothesis is formally stated in the alternative form as follows:

> **H2:** Firms in home countries with a worldwide approach engage in less tax avoidance than do firms in home countries with a territorial approach.

The third tax system characteristic that we examine is the perceived strength of tax enforcement. When managers perceive that government enforcement of tax rules is stronger, the higher expected probability of detection and potential for imposition of penalties may discourage tax avoidance. That is, managers may decrease tax avoidance when they believe tax authorities are more likely to detect the avoidance and impose additional taxes plus penalties.

Slemrod (2004) suggests that insights from standard deterrence models of the demand for tax avoidance—that higher penalties and higher probabilities of penalties being imposed deter tax avoidance—may not apply to corporate taxpayers because of the separation of ownership and management. Specifically, while the manager may be risk-averse, shareholders with diversified portfolios are risk-neutral. Thus, shareholders should want the manager to pursue all legal tax avoidance opportunities, including those that are very aggressive. However, there is often no clear, bright line between very aggressive tax avoidance and tax evasion. To align managers' incentives with the shareholders' interests, management compensation is tied to measures such as the effective tax rate or after-tax earnings. In this setting, enforcement efforts directed at the corporation may not deter the manager's tax avoidance decisions because the manager's contract compensates him *ex ante* for the risks of detection and, thus, may offset the deterrence effects of increased enforcement.

Chen and Chu (2005) and Crocker and Slemrod (2005) both develop formal principal-agent models of illegal corporate tax evasion when the principal (owner) is risk-neutral and the agent (manager) is risk-averse. Chen and Chu (2005) find that there is an efficiency loss due to the separation of ownership and control, while Crocker and Slemrod (2005) show that penalties imposed directly on the manager would be more effective in reducing tax evasion than penalties imposed on the corporation. Both models predict that firms evade taxes less when expected costs are higher (e.g., when the expected probability of detection and/or the penalties imposed are higher). However, a key assumption in both models is that tax evasion is illegal and management compensation contracts cannot be based on illegal actions because courts will not enforce such contracts. Thus, compensation contracts cannot be written based on the amount of tax evaded. However, these models do not address less extreme forms of tax avoidance, which may result in additional taxes paid with interest and penalties, but are not illegal. Therefore, it is not clear whether increased enforcement will deter tax avoidance overall.

Slemrod (2004) points out that the basic predictions of the deterrence model have not been subject to much empirical testing. However, two recent working papers use Internal Revenue Service (IRS) data to examine the impact of differences in the probability that a firm will be subject to a tax audit. When the probability of a tax audit is higher, Hanlon et al. (2011) find that accrual quality is higher and Hoopes et al. (2011) find that public firms take less aggressive tax positions. In addition, Desai et al. (2007) examine a sample of Russian firms following an increase in tax enforcement after the 2000 election of Vladimir Putin. They find that tax payments increased, related party trades were curtailed, and tax haven entities were abandoned. Consistent with these prior studies, we predict a negative relation between the perceived strength of tax enforcement and tax avoidance.

Our third hypothesis is formally stated in the alternative form as follows:

H3: Firms resident in countries where tax enforcement is perceived to be stronger engage in less tax avoidance.

III. RESEARCH DESIGN

Measure of Tax Avoidance

Recall that we define tax avoidance broadly as the reduction in explicit taxes paid.[7] We measure tax avoidance as the tax on pre-tax earnings computed at the home-country statutory corporate tax rate less the taxes actually paid, expressed as a percentage of pre-tax earnings.[8] We sum each element in the computation over three years.[9] Our measure of tax avoidance (*TaxAvoid*) for firm *i* in year *t* is calculated as follows:

$$TaxAvoid_{it} = \frac{\left[\sum_{t-2}^{t}(PTEBX \times \tau)_{it} - \sum_{t-2}^{t} CTP_{it}\right]}{\sum_{t-2}^{t} PTEBX_{it}} \tag{1}$$

where:

$PTEBX$ = pre-tax earnings before exceptional items (Item 21 – Item 57);[10]
τ = home-country statutory corporate income tax rate;[11] and
CTP = current taxes paid (Item 24 – the change in Item 100).[12]

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[7] We focus on a measure of the total amount of taxes avoided, rather than on specific actions taken to avoid taxes (e.g., the strategic location of subsidiaries in foreign countries, the decision not to repatriate dividends, the amount of income shifted, investments in tax shelters, use of derivatives) because specific actions taken are differentially costly to implement and provide different benefits across countries. Moreover, specific actions taken are often unobservable in our setting.

[8] This measure is in the spirit of measuring tax avoidance using cash effective tax rates (ETRs, i.e., taxes actually paid divided by pre-tax income) for samples of U.S.-only firms (see Dyreng et al. 2008). We take the difference between taxes computed at the statutory tax rate and taxes actually paid because we draw our sample from different countries. As a result, firms from different countries face different statutory tax rates and, hence, taxes paid must be compared with "unmanaged taxes" at the home-country statutory tax rate to cross-sectionally measure the amount of taxes avoided.

[9] Dyreng et al. (2008) use one-year, five-year, and ten-year tax avoidance measures. They find that one-year measures are highly variable and are not as predictive of long-run tax avoidance as five-year or ten-year measures. Thus, we compute our tax avoidance measure over multiple years. Here, we compute a three-year measure because this time period is adequate to reduce the effects of items that reverse in just one year, but is not as limiting to our sample size as is a five-year window.

[10] Item numbers reference the Compustat Global database.

[11] We hand-collected these statutory rates from a KPMG LLP online summary, PricewaterhouseCoopers LLP's online information, and Coopers & Lybrand LLP's worldwide tax summary guides.

[12] When current tax expense (Item 24) is missing, we replace it with total tax expense less deferred taxes (Item 23 – Item 25) when available. We delete observations where current tax expense (Item 24) is missing and either total tax expense (Item 23) or deferred taxes (Item 25) is missing.

This measure of tax avoidance indicates the total amount of taxes the firm is able to avoid, relative to the tax computed at the home-country statutory corporate tax rate, regardless of whether such tax avoidance creates a book-tax difference.[13]

Tax Avoidance Model

We test for the main effects of tax system characteristics on tax avoidance (H1 through H3) with the following model, estimated using ordinary least squares (firm and year subscripts are suppressed):

$$TaxAvoid = \alpha_{year} + \alpha_{ind} + \alpha_1 BTaxC + \alpha_2 WW + \alpha_3 TaxEnf + \sum \alpha_{\kappa} Control_{\kappa} + e, \qquad (2)$$

where:

$TaxAvoid$ = tax avoidance measure from Model (1);
α_{year} = year fixed-effects;
α_{ind} = industry fixed-effects, based on the Fama-French 30 industry classification scheme;
$BTaxC$ = a measure of the level of required book-tax conformity from Atwood et al. (2010);
WW = 1 for firms in home countries with a worldwide approach, and 0 for firms in home countries with a territorial approach;
$TaxEnf$ = a measure of managers' perceptions of the strength of tax enforcement in the country, from the *1996 World Competitiveness Report*; and
$Control$ = country-level controls (consisting of the statutory corporate tax rate in the home country [*TaxRate*], the average of variable pay as a percentage of total compensation for firms in the country [*VarComp*], the cross-sectional earnings volatility [*Earnvol*], and institutional factors [*Factor*]) and firm-level controls (consisting of pre-tax return on assets [*Pre-Tax ROA*], firm size [*LogSize*], research and development expenditures [*R&D*], leverage [*Leverage*], sales growth [*SalesGrth*], and an indicator variable for multinational operations [*Multi*]).

To ensure that our results are not due to currency changes within a given country, we convert all data to U.S. dollars (using the currency translation rates available from Compustat) before computing the variables in the model.

We use the *BTaxC* measure of required book-tax conformity developed in Atwood et al. (2010).[14] This measure is based on the conditional variance of current tax expense (*CTE*) for a given level of pre-tax book income (*PTBI*) in a given country-year (i.e., $Var(CTE|PTBI)$). This measure captures the required level of book-tax conformity in the firm's home country.[15] Countries with a lower conditional variance are presumed to allow less flexibility in the reporting of taxable income for a given level of reported pre-tax earnings and, thus, have higher required book-tax conformity. To compute *BTaxC*, we use the conditional variance of current tax expense from the following model, estimated by country-year:

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[13] Under U.S. Generally Accepted Accounting Principles (GAAP), the cash tax savings from stock options are not included in current tax expense (Hanlon and Shevlin 2002). These cash tax savings arise from the corporate tax deduction allowed when a manager exercises a nonqualified stock option. This deduction is compensation expense for tax purposes and equals the difference between the stock's fair market value on the date the stock is exercised and the price the manager paid for the stock. This tax benefit is recorded directly to equity and does not reduce income tax expense reported in the financial statement. Thus, tax avoidance by firms reporting under U.S. GAAP is likely understated by the amount of the tax savings from stock options deductions.

[14] Atwood et al. (2010) provide evidence that increasing the required level of book-tax conformity in the home country is associated with lower earnings quality, where earnings quality is measured as earnings persistence and the association between current earnings and future cash flows. They do not address whether increased required book-tax conformity is associated with more or less tax avoidance or whether firms use other mechanisms to achieve tax avoidance when required book-tax conformity is higher.

[15] Note that the *BTaxC* measure in Atwood et al. (2010) includes multinational firms. However, we find that when we eliminate all multinational firms and the foreign pre-tax income variable (*ForPTBI*) and reestimate the *BTaxC* model, the ranking of the *BTaxC* is the same. Thus, the *BTaxC* variable reflects the level of home-country book-tax conformity.

$$CTE_t = \theta_0 + \theta_1 PTBI_t + \theta_2 ForPTBI_t + \theta_3 DIV_t + e_t.$$

Here, *t* is a year indicator; *CTE* is current tax expense (Item #23 – Item #25);[16] *PTBI* is pre-tax book income (Item #21); *ForPTBI* is estimated foreign pre-tax book income (foreign tax expense (Item #51)/total tax expense (Item #23) × *PTBI*); *DIV* is total dividends (Item #34);[17] and *e* is the error term. We divide *CTE*, *PTBI*, *ForPTBI*, and *DIV* by average total assets (Item #89) to control for cross-sectional scale differences. *BTaxC* is computed as the scaled ranking of the root mean squared errors (RMSEs) from these country-year regressions. RMSEs are ranked so that countries with higher rankings in a given year have higher required book-tax conformity, i.e., we use descending ranks so that the highest RMSE in the year is ranked 0 and the lowest RMSE in the year is ranked $n-1$, where *n* is the number of included countries in that year. Firms in countries with higher *BTaxC* have less of an ability to avoid taxes using strategies that create book-tax differences in the home country. As a result, these firms may engage in less tax avoidance because tax-planning strategies that are available outside of the home country (e.g., locating operations in tax havens or shifting income to lower-tax jurisdictions) may be fewer and/or more costly to implement. Thus, H1 predicts that the association between tax avoidance and *BTaxC* will be negative ($\alpha_1 < 0$).

We use an indicator variable (*WW*) to distinguish between firms in home countries with a worldwide versus territorial approach to taxing foreign income. We hand-collected these data from *PricewaterhouseCoopers Corporate Taxes: A Worldwide Summary* guides and from the *Ernst & Young Worldwide Corporate Tax Guide* for years 1990 through 2008. These guides indicate the percentage of dividends from foreign subsidiaries that are subject to tax. In most countries, dividends are either fully taxable or fully exempt, but during our sample period, Belgium, France, Germany, and Italy exempted 95 percent of dividends from foreign subsidiaries (E&Y 2008) and in 2004, Finland adopted a system in which dividends from subsidiaries in European Union (EU) countries are tax-exempt, but dividends from subsidiaries in non-EU countries are 75 percent tax-exempt (E&Y 2004). We code countries as territorial if they exempt from tax at least 75 percent of foreign subsidiary dividends. H2 predicts that the association between tax avoidance and *WW* will be negative ($\alpha_2 < 0$).

To measure perceived tax enforcement (*TaxEnf*), we use the tax evasion index from the *1996 World Competitiveness Report*. This index is derived from a survey of more than 2,000 business executives per country. Respondents report their agreement with the statement "Tax evasion is minimal in your country" on a scale from 1 through 6 (where 1 indicates strongly disagree and 6 indicates strongly agree). Thus, higher numbers indicate that tax evasion is perceived to be lower, so the strength of tax enforcement is perceived to be higher.[18] Dyck and Zingales (2004) find a negative relation between this measure and private benefits of control (i.e., the private benefits that controlling shareholders can extract from non-controlling shareholders) across countries, suggesting that additional monitoring by tax authorities reduces these benefits. H3 predicts that the association between tax avoidance and *TaxEnf* will be negative ($\alpha_3 < 0$).

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[16] As in Atwood et al. (2010), when total tax expense or deferred tax expense is missing, we use current tax expense, if available, otherwise we delete the observation.

[17] Dividends are included in the model to control for those countries with tax rates that depend on whether the earnings are distributed (e.g., during the transition from an imputation system to the current system in Germany). As part of our robustness tests, we estimate *BTaxC* without including dividends in the model and find that the country-year rankings are identical.

[18] We are unable to locate similar surveys in other years in order to determine whether perceptions of the strength of tax enforcement have changed over time.

We include the statutory corporate tax rate as an important control variable in determining the impact of tax system characteristics on tax avoidance. In general, the benefits of engaging in tax-planning strategies are higher when the statutory tax rate is higher, suggesting a positive relation between tax avoidance and statutory tax rates. However, prior empirical research does not provide strong evidence on the relation between statutory corporate tax rates and corporate tax avoidance. Kamdar (1997) uses time-series data from IRS audits to examine corporate tax avoidance. He finds a weak negative relation between statutory corporate tax rates and corporate tax compliance measured as the percentage of "true" income voluntarily reported, and finds no relation using four of six alternative measures. Thus, we make no specific prediction about the relation between our tax avoidance measure and statutory corporate tax rates.

We hand-collected the statutory corporate tax rates (*TaxRate*) from a KPMG LLP online summary, PricewaterhouseCoopers LLP's online information, and Coopers & Lybrand LLP's worldwide tax summary guides. These statutory corporate tax rates include both the federal income tax rate and the average effects of state, provincial, and other local government income tax rates. Since *TaxRate* is included in the calculation of *TaxAvoid*, the association between these variables may be partially mechanical. Because *TaxAvoid* includes the effects of both book-tax differences and tax-planning strategies that do not create book-tax differences (e.g., location decisions, income shifting, repatriation strategies), *TaxAvoid* can be broken down into two components. That is, $TaxAvoid = (BTD \times \tau) + (S \times (\tau - \tau^*))$, where *BTD* equals total book income less total taxable income across all jurisdictions, τ equals the statutory tax rate in the home country, *S* equals income shifted to lower tax jurisdictions, and τ^* equals the tax rate in the jurisdiction to which income is shifted. If one assumes that *BTD* and *S* are determined independently of the tax rate (i.e., $\partial\, BTD/\partial\tau = 0$ and $\partial\, S/\partial\tau = 0$), then the relation between *TaxAvoid* and τ is purely mechanical (i.e., $\partial TaxAvoid/\partial\tau = BTD + Shift$). However, if one assumes that *BTD* and *S* are functions of the home-country and foreign-country statutory tax rates (that is, $BTD = f(\tau)$, and $S = g(\tau,\tau^*)$), then $TaxAvoid = g(\tau,\tau^*) \times (\tau - \tau^*) + f(\tau) \times \tau$ and $\partial TaxAvoid/\partial\tau = (\tau - \tau^*) \times g'(\tau,\tau^*) + g(\tau,\tau^*) + \tau \times f'(\tau) + f(\tau)$. Under these assumptions, the relation between *TaxAvoid* and τ is not purely mechanical. However, because of the potential for a mechanical relation, we include the home-country statutory tax rate as a control variable.[19]

With respect to the other country-level control variables, prior research provides evidence that management incentives impact tax avoidance (Phillips 2003; Rego and Wilson 2008; Hanlon and Heitzman 2009; Gaertner 2010). Because data on individual manager salary, bonus, equity, stock options, and/or other compensation components are not available for non-U.S. firms, we use the country average of managers' variable pay as a percentage of management compensation (*VarComp*) from Towers Perrin (2005) to measure management incentives from compensation contracts.[20] This percentage is calculated as all performance-based compensation (i.e., bonuses and stock-related compensation) divided by total compensation.[21] Towers Perrin (2005) reports significant differences in the pay components of CEOs across 26 countries (22 of which are included in our sample). Specifically, average variable pay as a percentage of total compensation ranges from a high of 62 percent in the U.S. to a low of 14 percent in India. Desai and Dharmapala (2006) and Hanlon et al. (2007) suggest that incentive compensation is positively associated with tax avoidance in the U.S., so we predict that firms in countries where variable pay is a larger portion of CEO compensation will engage in more tax avoidance.

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[19] We suggest, however, that future research should further examine the relation between statutory tax rates and tax avoidance.

[20] Towers Perrin maintains databases for compensation-planning purposes of executive compensation for both local and multinational companies in 97 countries. We believe that the broad cross-section of firms included in the Towers Perrin survey is comparable to the cross-section of firms included in our sample, and are unable to obtain similar surveys for years prior to 2005 in order to determine whether the percentage of compensation from variable pay varied for a given country over time.

[21] The understatement of tax avoidance due to the reporting of tax benefits from stock options under U.S. GAAP (see footnote 13) may induce a negative correlation between tax avoidance and the variable portion of management compensation (*VarComp*), but we note that the correlation between tax avoidance and *VarComp* is positive so this potential effect does not seem to be problematic.

Earnvol is the scaled descending decile rank of cross-sectional pre-tax earnings volatility by country-year, which controls for differences in the cross-sectional variance in pre-tax earnings. As reported in Atwood et al. (2010), *BTaxC* is positively correlated with the cross-sectional variance in pre-tax earnings. Thus, we include earnings volatility as a control in our model to ensure that the results for required book-tax conformity are not overstated due to the effects of cross-country differences in earnings volatility. We follow Atwood et al. (2010) and control for cross-country institutional factors (*Factor*) using the results of a factor analysis (untabulated) of the country's legal tradition (common law versus code law), strength of investor rights, and ownership concentration as developed by La Porta et al. (1998). We find that these variables converge to one significant factor (eigenvalue = 1.96), which explains 65 percent of the variation in the component variables. We extract this principal component (*Factor*) and include it as a control variable in our models.

Our firm-level control variables are shown in prior research to be associated with tax avoidance.[22] We control for firm profitability using pre-tax return on assets (*Pre-Tax ROA*), measured as pre-tax income before exceptional items (Item 21 – Item 57) divided by lagged total assets (Item 89). We control for firm size (*LogSize*) by taking the natural log of total assets. Our measure of intangible intensity or investment in research and development (*R&D*) is calculated as research and development expense (Item 52) divided by lagged total assets (Item 89). Leverage (*Leverage*) is calculated as total long-term liabilities (Item 108 + Item 94) divided by total assets (Item 89). Sales growth (*SalesGrth*) is the three-year average change in sales (Item 5). Finally, our multinational operations indicator (*Multi*) is set equal to 0 when foreign income taxes (Item 51) is missing or 0, and set to 1 otherwise.[23] We use an indicator variable rather than the ratio of foreign pre-tax income to total pre-tax income because Compustat Global does not provide a breakdown of domestic and foreign pre-tax income for non-U.S. multinationals. This indicator variable approach is similar to that used by Hanlon et al. (2007).[24]

Tax Avoidance Model with Incentive Compensation

We further investigate the relations between tax avoidance and tax system characteristics by allowing the coefficients to vary when the variable pay component of compensation is a higher versus lower portion of managers' total compensation. Prior research provides evidence that incentives under management compensation contracts affect tax avoidance decisions. Specifically, prior research finds that: tax avoidance is positively associated with the level of management compensation (Rego and Wilson 2008); effective tax rates are lower when managers' incentive compensation is based on after-tax versus pre-tax performance measures (Phillips 2003; Gaertner 2010); proposed IRS deficiencies are positively associated with the percentage of annual compensation that is in the form of bonuses and with the level of equity incentives from exercisable stock options (Hanlon et al. 2007); and tax avoidance is positively associated with equity risk incentives (Rego and Wilson 2011).

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[22] See, for example, Hanlon et al. (2007), Dyreng et al. (2008), Rego and Wilson (2008), Cazier et al. (2009), Khurana and Moser (2009), Chen et al. (2010), Gaertner (2010), Hogan and Noga (2010), Lisowsky (2010), Badertscher et al. (2011), Cook and Omer (2011), and McGuire et al. (2012).

[23] We note that the Japanese companies in our sample all report missing values for foreign tax expense. We perform a search of *Mergent Online* and *Lexis Nexis Academic Universe* to identify Japanese firms with foreign subsidiaries and, in all tabulated tests, we reclassify these as multinationals. This results in the reclassification of 5,622 Japanese observations (33.8 percent of the total Japanese sample) as multinationals. We note, however, that the results for *Multi* are consistent with and without this correction.

[24] Hanlon et al. (2007) use U.S. tax return data to create an indicator variable equal to 1 if the taxpayer claimed a foreign tax credit or filed a Form 5471, whereas we base our indicator variable on the existence of a foreign income tax expense amount.

Recent models of corporate tax evasion that allow for agency costs also suggest that managerial incentives influence tax avoidance. Desai and Dharmapala (2006) show that increases in higher-powered incentives (e.g., stock option grants as a percentage of total compensation) may induce managers to engage in more or less tax-sheltering activity, and Crocker and Slemrod (2005) show that the optimal management compensation contract adjusts to offset, at least partially, the effect of sanctions against tax evasion.

These studies suggest that managers' tax avoidance decisions may be influenced by the proportion of their compensation that is comprised of bonuses, stocks, and stock options rather than fixed salaries. Moreover, the implementation of tax avoidance strategies and the receipt of any related incentive compensation generally occur several years before any related audit by taxing authorities, and even further from the payment of any additional taxes and penalties. As such, potential future audit adjustments and penalties may be less salient to managers than are the prospects for increased current incentive pay. For these reasons, we include the proportion of the manager's total compensation that is in the form of variable pay (i.e., bonus, stock, and stock options) in our tax avoidance model and allow the coefficients on the tax characteristic variables to differ when variable compensation as a percent of total compensation is high versus low. We test for the influence of variable compensation using the following model:

$$\begin{aligned} TaxAvoid = {} & \beta_{year} + \beta_{ind} + \beta_1 BTaxC \times High + \beta_2 BTaxC \times Low + \beta_3 WW \times High \\ & + \beta_4 WW \times Low + \beta_5 TaxEnf \times High + \beta_6 TaxEnf \times Low + \beta_7 TaxRate \times High \\ & + \beta_8 TaxRate \times Low + \sum \alpha_\kappa Control_\kappa + e \end{aligned} \tag{3}$$

where:

High = 1 if *VarComp* is above the median (0 otherwise);
Low = 1 if *VarComp* is below the median (0 otherwise); and
All other variables are as previously defined.

We expect that the coefficients will differ across the *High* and *Low* categories, but make no prediction about the signs of these differences.

Tax Avoidance Model with Accruals Components

We further investigate the relations between tax avoidance and tax system characteristics after controlling for accruals components. We do this to determine whether our tax system characteristics impact tax avoidance accomplished through accruals management alone or whether they also impact tax avoidance accomplished through other tax-planning strategies. Our approach is similar in spirit to that in Desai and Dharmapala (2006) who attempt to isolate the portion of tax aggressiveness that is attributable to tax sheltering as opposed to accruals management.

In our models, we allow the coefficients on the accruals components to vary based on the reliability classifications developed in Richardson et al. (2005). This allows us to investigate whether tax avoidance is more closely associated with certain types of accruals. Richardson et al. (2005) measure total accruals (*ACC*) as the sum of the change in net non-cash working capital (ΔWC), the change in net noncurrent operating assets (ΔNCO), and the change in net financial assets (ΔFIN), so $ACC = \Delta WC + \Delta NCO + \Delta FIN$. Here, Δ is the change operator, *WC* is current operating assets (Item 75 – Item 60) minus current operating liabilities (Item 104 – Item 94), *NCO* is noncurrent operating assets (Item 89 – Item 75 – Item 80 – Item 81) minus noncurrent operating liabilities (Item 118 – Item 104 – Item 106), and *FIN* is financial assets (Item 62 + Item 80 + Item 81) minus financial liabilities (Item 106 + Item 94 + Item 199). We add the separate accruals measures to the tax avoidance models above. The augmented models (with firm and year subscripts suppressed) are as follow:

$$TaxAvoid = \alpha_{year} + \alpha_{ind} + \alpha_1 BTaxC + \alpha_2 WW + \alpha_3 TaxEnf + \alpha_4 \Delta WC + \alpha_5 \Delta NCO + \alpha_6 \Delta FIN + \sum \alpha_\kappa Control_\kappa + e \quad (4)$$

$$TaxAvoid = \beta_{year} + \beta_{ind} + \beta_1 BTaxC \times High + \beta_2 BTaxC \times Low + \beta_3 WW \times High + \beta_4 WW \times Low + \beta_5 TaxEnf \times High + \beta_6 TaxEnf \times Low + \beta_7 TaxRate \times High + \beta_8 TaxRate \times Low + \beta_9 \Delta WC + \beta_{10} \Delta NCO + \beta_{11} \Delta FIN + \sum \alpha_\kappa \mathrm{Control}_\kappa + \mathrm{e} \quad (5)$$

All variables are as previously defined.

We expect a positive relation between *TaxAvoid* and accruals because prior research indicates that accruals are positively related to tax sheltering (Wilson 2009; Lisowsky 2010), but we allow this relation to vary with the reliability of the accruals. With the accruals variables included in the model, we interpret our results for *BTaxC*, *WW*, and *TaxEnf* as their impact on non-accrual-related tax avoidance.

IV. SAMPLE SELECTION, DESCRIPTIVE STATISTICS, AND TEST RESULTS

We select our sample from all firm-year observations in the Compustat Global Industrial/Commercial database from 1993 through 2007 with sufficient data to calculate the variables in Models (1) through (5).[25] We apply several data filters. First, we limit our sample to the 33 countries included in Atwood et al. (2010) because these are the countries for which we can estimate required book-tax conformity. Second, we limit our sample to the 22 countries included in Towers Perrin (2005) because these are the countries for which we can estimate CEO variable pay. Third, we require three years of positive pre-tax earnings before exceptional items (*PTEBX*) for our calculations of tax aggressiveness. Thus, for all of our observations, $\sum_{t-2}^{t}(PTEBX \times \tau)_{it}$ will be greater than zero. Fourth, we delete all firm-year observations from countries that do not have at least 20 firms with available data in the year.[26] This sample selection procedure yields 69,301 firm-year observations from 22 countries.[27] Table 1 presents a list of the countries in our sample, their corresponding sample periods, and the number of total observations per country[28] as well as the country medians for each of the variables in Models (2) and (3).

Table 1 reveals considerable variation in the number of observations per country, ranging from 323 observations from India to 18,454 observations from the U.S. Thus, in a sensitivity test we also examine whether sample concentration influences our results by calculating the median for each variable by country-industry-year and entering the resulting median value into Models (2) and (3).

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[25] Our sample period is as in Atwood et al. (2010) since we use their methodology to estimate book-tax conformity.

[26] Atwood et al. (2010) require 40 usable observations per country-year. Given that we have greater time-series data requirements, we relax this restriction to 20 usable observations.

[27] To remove the influence of potential outliers, we winsorize all continuous variables at the top and bottom 1 percent for each year.

[28] The earliest year reported in Table 1 is 1995 because we require three years of data to construct our tax aggressiveness variables.

TABLE 1

Sample Composition and Median Characteristics by Country

Panel A: Selected Variables, Part I

Country	Time Period	n	*TaxAvoid*	*BTaxC*	*WW*	*TaxEnf*	*TaxRate*	*VarComp*	*Earnvol*	*Pre-TaxROA*	*LogSize*
Australia	1995–2007	2,510	0.233	0.125	1	4.580	0.300	0.300	0.032	0.108	5.109
Belgium	1996–2007	517	0.130	0.219	0	2.270	0.402	0.280	0.242	0.078	5.893
Brazil	1995–2007	537	0.122	0.406	1	2.140	0.340	0.360	0.625	0.107	6.953
Canada	1997–2007	2,547	0.156	0.031	0	3.770	0.421	0.510	0.441	0.098	6.334
France	1995–2007	3,123	0.086	0.783	0	3.860	0.343	0.410	0.719	0.081	5.984
Germany	1995–2007	2,533	0.180	0.118	0	3.410	0.384	0.520	0.531	0.078	6.181
Hong Kong	1995–2007	712	0.053	0.906	0	4.560	0.165	0.280	0.531	0.080	6.298
India	2004–2006	323	0.185	0.063	1	2.160	0.359	0.140	0.063	0.138	6.073
Italy	1995–2007	1,008	0.112	0.563	1	1.770	0.383	0.350	0.875	0.073	6.662
Japan	1995–2007	16,615	−0.008	0.706	1	4.410	0.420	0.220	0.906	0.046	6.072
Korea	2002–2007	894	0.216	0.594	1	3.290	0.275	0.390	0.500	0.091	6.821
Malaysia	1995–2007	3,029	0.060	0.719	0	4.340	0.280	0.460	0.839	0.074	4.730
Mexico	1997–2007	440	0.141	0.406	1	2.460	0.340	0.400	0.625	0.105	7.350
The Netherlands	1995–2007	1,067	0.114	0.652	0	3.400	0.350	0.410	0.355	0.100	6.444
Singapore	1995–2007	1,745	0.047	0.750	0	5.050	0.220	0.580	0.656	0.075	4.991
South Africa	1995–2007	993	0.244	0.094	1	2.400	0.378	0.570	0.156	0.147	5.949
Spain	1995–2007	946	0.163	0.906	0	1.910	0.350	0.420	0.688	0.076	6.783
Sweden	1995–2007	1,079	0.065	0.424	0	3.390	0.280	0.180	0.438	0.102	6.036
Switzerland	1995–2007	1,255	0.088	0.875	0	4.490	0.247	0.460	0.903	0.076	6.319
Taiwan	2000–2007	2,254	0.123	0.645	1	3.250	0.250	0.240	0.344	0.093	5.187
U.K.	1995–2007	6,720	0.064	0.469	1	4.670	0.300	0.360	0.375	0.108	5.525
U.S.	1995–2007	18,454	0.095	0.130	1	4.470	0.400	0.620	0.273	0.115	6.582
Total		69,301									

(continued on next page)

TABLE 1 (continued)

Panel B: Selected Variables, Part II

Country	Time Period	n	*R&D*	*Leverage*	*SalesGrth*	*Multi*	*Common-Law*	*InvRights*	*OwnCon*	*Factor*
Australia	1995–2007	2,510	0.000	0.214	0.155	0	1	4.000	0.280	0.254
Belgium	1996–2007	517	0.000	0.202	0.088	0	0	0.000	0.620	−2.947
Brazil	1995–2007	537	0.000	0.223	0.179	0	0	3.000	0.630	−1.694
Canada	1997–2007	2,547	0.000	0.259	0.151	0	1	5.000	0.240	0.762
France	1995–2007	3,123	0.000	0.199	0.125	0	0	3.000	0.240	−0.882
Germany	1995–2007	2,533	0.000	0.174	0.109	0	0	1.000	0.500	−2.273
Hong Kong	1995–2007	712	0.000	0.088	0.083	1	1	5.000	0.540	0.137
India	2004–2006	323	0.000	0.207	0.223	0	1	5.000	0.430	0.366
Italy	1995–2007	1,008	0.000	0.238	0.131	0	0	1.000	0.600	−2.481
Japan	1995–2007	16,615	0.002	0.071	0.034	0	0	4.000	0.130	−0.229
Korea	2002–2007	894	0.006	0.172	0.155	0	0	2.000	0.200	−1.224
Malaysia	1995–2007	3,029	0.000	0.081	0.113	0	1	4.000	0.520	−0.246
Mexico	1997–2007	440	0.000	0.196	0.122	0	0	1.000	0.670	−2.627
The Netherlands	1995–2007	1,067	0.000	0.227	0.105	0	0	2.000	0.310	−1.453
Singapore	1995–2007	1,745	0.000	0.138	0.111	0	1	4.000	0.530	−0.266
South Africa	1995–2007	993	0.000	0.060	0.122	0	1	5.000	0.520	0.179
Spain	1995–2007	946	0.000	0.259	0.133	0	0	4.000	0.500	−0.999
Sweden	1995–2007	1,079	0.000	0.143	0.121	0	0	3.000	0.280	−0.965
Switzerland	1995–2007	1,255	0.000	0.198	0.103	0	0	2.000	0.480	−1.806
Taiwan	2000–2007	2,254	0.011	0.043	0.119	0	0	3.000	0.140	−0.674
U.K.	1995–2007	6,720	0.000	0.229	0.116	1	1	5.000	0.150	0.949
U.S.	1995–2007	18,454	0.000	0.223	0.105	0	1	5.000	0.120	1.012
Total		69,301								

This procedure results in a sample of 6,119 country-industry-year observations. While this procedure reduces the influence that any particular country has on the results, it also removes much of the variation in the dependent variable (*TaxAvoid*) and in the firm-specific control variables (*Pre-TaxROA*, *LogSize*, *R&D*, *Leverage*, *SalesGrth*, and *Multi*). Thus, we draw our main inferences from the results that use the full sample, and view the results that use the country-industry-year medians sample as a very conservative robustness test.

Table 2, Panel A provides descriptive statistics for the full sample. The median level of *TaxAvoid* is 7.9 percent of pre-tax earnings. Dyreng et al. (2008) report median annual cash effective tax rates of 25.6 percent. This would imply a median level of tax avoidance of about 9.4 percent of pre-tax earnings (i.e., the 35 percent statutory corporate tax rate less the 25.6 percent cash effective tax rate). Thus, the median *TaxAvoid* for our sample of multinational firms is lower than that for the sample of U.S. firms in Dyreng et al. (2008). However, Table 1 indicates that the median *TaxAvoid* for U.S. firms in our sample is 9.5 percent, so our median tax avoidance measure for U.S. firms is consistent with the median reported by Dyreng et al. (2008). We also note that 73.7 percent of our sample observations are from countries with a worldwide tax approach, and 30.2 percent of our sample observations have multinational operations.

Panels B and C of Table 2 provide Pearson (above the diagonal) and Spearman (below the diagonal) correlations among the variables. All reported correlations are statistically significant at the 5 percent level or better with the exception of the correlations in bold. For both the Pearson and Spearman correlations, *BTaxC*, *WW*, and *TaxEnf* are negatively correlated with our tax avoidance variable, while *VarComp* is positively correlated with tax avoidance. Our tax enforcement variable (*TaxEnf*) is very highly correlated (Pearson Rho = 0.63) with our control variable for institutional factors (*Factor*). Following Gleason and Lee (2003), we conduct a test of collinearity (untabulated) by regressing the dependent variable (*TaxAvoid*) on all of the independent variables and calculating the variance inflation factors (VIFs) for each variable. We find that the average VIF is 2.5 and the highest VIF across the three regressions is 7.5, which is well below the generally accepted threshold of 10. This suggests that multicollinearity is not a problem in our model.

We now turn to the main tests of our hypotheses. We estimate all regressions models using ordinary least squares regression and we assess statistical significance using standard errors adjusted for firm clustering of residuals when using the full sample and for industry clustering of residuals when using the medians sample.[29]

Table 3 presents the results from estimating Model (2). Columns A and B present the results from estimating Model (2) using the full sample and the country-industry-year medians, respectively. Consistent with H1, tax avoidance is negatively associated with *BTaxC* in both samples, indicating that firms engage in less tax avoidance on average when their home countries have higher required book-tax conformity. An increase from the 25th percentile to the 75th percentile of required book-tax conformity results in a 23.74 percent decrease in *TaxAvoid* for the full sample (and a 35.76 percent decrease for the country-year medians sample).[30] These results suggest that firms in home countries with greater required book-tax conformity engage in significantly less tax avoidance.

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[29] For regressions estimated using the medians sample, we do not adjust for firm clustering of residuals because individual firm-year observations do not appear in this country-industry-year sample.

[30] We calculate the percent increase in tax avoidance from moving from the 25th percentile to the 75th percentile as follows: [0.706 (*BTaxC* at Q3 per Table 2, Panel A) – 0.152 (*BTaxC* at Q1 per Table 2, Panel A)] × –0.036 (the coefficient on *BTaxC* per Table 3, Column A) ÷ 0.084 (mean *TaxAvoid* per Table 2, Panel A) = 23.67 percent. We perform similar calculations for our variables of interest below, but we do not provide the details of each calculation for parsimony.

TABLE 2

Descriptive Statistics and Correlations

Panel A: Descriptive Statistics

Variable	Mean	Std. Dev.	Q1	Median	Q3
TaxAvoid	0.084	0.172	−0.007	0.079	0.188
BTaxC	0.451	0.299	0.152	0.478	0.706
WW	0.737	0.440	0.000	1.000	1.000
TaxEnf	4.147	0.688	3.860	4.410	4.470
TaxRate	0.372	0.074	0.310	0.400	0.407
VarComp	0.415	0.160	0.220	0.410	0.620
Factor	0.000	1.000	−0.266	−0.229	1.012
Earnvol	0.504	0.305	0.261	0.438	0.788
Pre-TaxROA	0.108	0.089	0.046	0.084	0.143
LogSize	6.228	1.752	4.997	6.092	7.350
R&D	0.014	0.033	0.000	0.000	0.011
Leverage	0.225	0.281	0.030	0.152	0.318
SalesGrth	0.133	0.250	−0.011	0.098	0.222
Multi	0.302	0.459	0.000	0.000	1.000
ΔWC	0.012	1.541	−0.077	0.043	0.208
ΔNCO	0.264	2.495	−0.056	0.118	0.442
ΔFIN	−0.043	1.337	−0.224	−0.006	0.152
CommonLaw	0.534	0.499	0.000	1.000	1.000
InvRights	4.033	1.162	4.000	4.000	5.000
OwnCon	0.231	0.160	0.120	0.130	0.280
n	69,301				

Our measure of tax aggressiveness (*TaxAvoid*) for firm *i* in year *t* is calculated as follows:

$$TaxAvoid_{it} = \frac{\left[\sum_{t-2}^{t}(PTEBX \times \tau)_{it} - \sum_{t-2}^{t} CTP_{it}\right]}{\sum_{t-2}^{t} PTEBX_{it}} \tag{1}$$

PTEBX = pre-tax earnings before exceptional items (Item 21 − Item 57);
τ = statutory tax rate;
CTP = current taxes paid (Item 24 − the change in Item 100);
BTaxC = proxy for the level of required book-tax conformity, following Atwood et al. (2010);
WW = 1 for firms in countries with worldwide tax systems, and 0 for firms in countries with territorial tax systems;
TaxEnf = proxy for the level of tax enforcement in the country, from the 1996 *World Competitiveness Report*;
TaxRate = statutory tax rate;
VarComp = average of variable pay as a percentage of total compensation for firms in the country, from the Towers Perrin *Worldwide Total Remuneration Report, 2005–2006*;
Factor = control for cross-country institutional factors using the results of a factor analysis of the country's legal tradition (common law versus code law), strength of investor rights, and ownership concentration as developed by La Porta et al. (1998);
Earnvol = country-level control variable for earnings volatility;
Pre-TaxROA = pre-tax return on assets;
LogSize = log of assets;
R&D = research and development expenditures;
Leverage = leverage;
SalesGrth = sales growth;
Multi = indicator variable for multinational operations;

(continued on next page)

TABLE 2 (continued)

Δ = change operator;
WC = current operating assets (Item 75 − Item 60) minus current operating liabilities (Item 104 − Item 94);
NCO = noncurrent operating assets (Item 89 − Item 75 − Item 80 − Item 81) minus noncurrent operating liabilities (Item 118 − Item 104 − Item 106);
FIN = financial assets (Item 62 + Item 80 + Item 81) minus financial liabilities (Item 106 + Item 94 + Item 199);
CommonLaw = 1 if the country's legal tradition is common-law, 0 if code-law;
InvRights = the investors' rights score developed by La Porta et al. (1998); and
OwnCon = the ownership concentration variable developed by La Porta et al. (1998).

Panel B: Pearson (Above) and Spearman (Below) Correlations, 1–10

	Variable	1	2	3	4	5	6	7	8	9	10
1	*TaxAvoid*		−0.27	−0.09	−0.17	−0.09	0.23	−0.27	0.16	0.02	0.03
2	*BTaxC*	−0.26		−0.17	**0.00**	−0.11	−0.65	0.75	−0.29	−0.07	−0.13
3	*WW*	−0.10	−0.16		0.28	0.41	−0.03	−0.09	−0.01	0.08	0.11
4	*TaxEnf*	−0.05	−0.19	0.32		0.03	0.11	**0.00**	−0.02	−0.06	0.06
5	*TaxRate*	−0.17	−0.04	0.42	−0.15		−0.02	0.24	−0.14	0.16	0.06
6	*VarComp*	0.24	−0.60	−0.03	0.29	−0.18		−0.56	0.25	0.09	0.12
7	*Earnvol*	−0.26	0.71	−0.08	−0.30	0.32	−0.57		−0.34	**0.00**	−0.10
8	*Pre-TaxROA*	0.16	−0.31	−0.03	0.13	−0.23	0.31	−0.37		−0.11	0.23
9	*LogSize*	0.04	−0.08	0.09	−0.03	0.15	0.09	**0.00**	−0.09		**0.00**
10	*R&D*	−0.03	−0.03	0.21	**0.00**	0.14	−0.05	**0.00**	0.09	0.08	
11	*Leverage*	0.15	−0.14	−0.07	0.09	−0.07	0.18	−0.16	−0.14	0.37	−0.14
12	*SalesGrth*	0.18	−0.15	−0.09	0.02	−0.17	0.13	−0.17	0.38	**0.00**	0.02
13	*Multi*	−0.07	−0.07	0.20	0.25	0.09	0.11	−0.04	0.06	0.21	0.25
14	*ΔWC*	0.04	0.01	0.02	−0.09	0.04	−0.04	0.01	−0.01	−0.07	0.06
15	*ΔNCO*	0.14	−0.13	−0.08	0.06	−0.04	0.13	−0.11	−0.01	0.09	−0.07
16	*ΔFIN*	−0.04	0.07	0.08	−0.01	0.08	−0.07	0.09	0.04	−0.06	0.08
17	*CommonLaw*	0.16	−0.60	0.08	0.63	−0.31	0.68	−0.63	0.31	−0.03	−0.10
18	*InvRights*	0.04	−0.52	0.39	0.62	0.08	0.51	−0.49	0.22	0.06	0.02
19	*OwnCon*	0.08	0.29	−0.64	−0.37	−0.51	−0.31	0.20	−0.05	−0.15	−0.25
20	*Factor*	0.04	−0.58	0.50	0.65	0.19	0.52	−0.52	0.22	0.09	0.09

Panel C: Pearson (Above) and Spearman (Below) Correlations, 11–20

	Variable	11	12	13	14	15	16	17	18	19	20
1	*TaxAvoid*	0.09	0.18	−0.07	0.01	0.06	−0.03	0.17	−0.03	0.12	0.01
2	*BTaxC*	−0.07	−0.14	−0.07	−0.01	−0.02	0.02	−0.62	−0.30	0.13	−0.44
3	*WW*	−0.01	−0.09	0.20	**0.00**	−0.02	0.03	0.08	0.38	−0.60	0.42
4	*TaxEnf*	−0.01	−0.05	0.25	**0.00**	−0.01	0.02	0.41	0.58	−0.54	0.63
5	*TaxRate*	0.03	−0.14	0.08	−0.01	0.01	0.01	−0.25	0.06	−0.39	0.06
6	*VarComp*	0.08	0.11	0.11	0.01	0.02	−0.03	0.68	0.31	0.05	0.40
7	*Earnvol*	−0.07	−0.16	−0.05	−0.02	−0.01	0.03	−0.65	−0.34	0.13	−0.47
8	*Pre-Tax ROA*	−0.10	0.35	0.05	0.01	−0.02	0.01	0.27	0.13	−0.01	0.17
9	*LogSize*	0.34	−0.01	0.20	−0.05	0.04	−0.02	−0.04	**0.00**	−0.07	0.01
10	*R&D*	−0.09	0.09	0.20	**0.00**	**−0.01**	0.03	0.05	0.07	−0.16	0.10
11	*Leverage*		0.17	−0.02	−0.16	0.24	−0.20	0.06	0.03	−0.02	0.05
12	*SalesGrth*	0.13		−0.02	0.03	0.07	−0.09	0.13	0.03	0.05	0.05
13	*Multi*	**0.01**	−0.01		**0.00**	−0.01	0.01	0.20	0.29	−0.19	0.29
14	*ΔWC*	−0.01	0.11	−0.01		−0.76	0.29	0.01	0.01	**0.00**	0.01
15	*ΔNCO*	0.30	0.17	−0.02	−0.37		−0.73	0.02	**0.00**	**0.00**	**0.01**

(continued on next page)

TABLE 2 (continued)

	Variable	11	12	13	14	15	16	17	18	19	20
16	*ΔFIN*	−0.35	−0.14	0.03	−0.10	−0.55		−0.03	**0.00**	−0.02	**0.00**
17	*CommonLaw*	0.14	0.14	0.20	−0.04	0.11	−0.07		0.71	−0.09	0.78
18	*InvRights*	0.10	0.06	0.31	−0.04	0.09	−0.03	0.83		−0.57	0.97
19	*OwnCon*	**0.00**	0.04	−0.24	−0.02	−0.01	−0.05	−0.23	−0.58		−0.65
20	*Factor*	0.09	0.05	0.30	−0.03	0.08	−0.02	0.76	0.94	−0.77	

All reported correlations in Panels B and C are statistically significant at the 5 percent level or better with the exception of the correlations in bold. Pearson correlations are reported above the diagonal and Spearman correlations are reported below the diagonal.
All variables are as defined in Table 2, Panel A.

Consistent with H2, we find that firms in home countries with a worldwide approach engage in less tax avoidance than do firms in home countries with a territorial approach for both the full and medians sample. Using the full sample, the results suggest that firms in home countries with a worldwide approach avoid taxes by 13.10 percent less than do firms in home countries with a territorial approach (and 12.66 percent less using the median sample). Contrary to suggestions in Lokken (2006), Mandolfo (2007), and Fleming et al. (2008), we find no evidence that firms resident in countries with worldwide tax systems use sophisticated international tax-planning techniques to produce results that are better (i.e., that avoid more taxes) on average than those produced by firms resident in countries with territorial tax systems. Rather, our results suggest that on average, the worldwide approach results in less tax avoidance than does the territorial approach. Our results are consistent with those of Markle (2010), who finds that multinational firms subject to the worldwide approach engage in less tax-motivated income shifting than do multinational firms subject to the territorial approach. The results suggest that this reduced income shifting contributes to lower tax avoidance by multinational firms subject to the worldwide approach.

Consistent with H3, we find that tax avoidance is negatively associated with managers' perceptions of the strength of tax enforcement (*TaxEnf*). When perceived tax enforcement increases from the 25th percentile to the 75th percentile, tax avoidance decreases by 18.88 percent of the average for the full sample (and 16.22 percent for the medians sample). This overall result is consistent with the standard deterrence model of tax evasion—that higher penalties and higher probabilities of penalties being imposed deter evasion—and with the empirical results in Desai et al. (2007), Hanlon et al. (2011), and Hoopes et al. (2011).

We also find that tax avoidance is positively associated with the statutory corporate tax rate (*TaxRate*). When the statutory corporate tax rate increases from the 25th percentile (31 percent) to the 75th percentile (40.7 percent), tax avoidance increases by 6.58 percent of the average for the full sample (and 6.26 percent for the medians sample). However, we interpret this result with caution due to the potential for a mechanical relation between our tax avoidance measure and statutory tax rates (as previously discussed).

Consistent with prior research, we also find that manager incentives (*VarComp*) are positively associated with tax avoidance. Specifically, when the variable component of management compensation increases from the 25th percentile (22.0 percent) to the 75th percentile (62.0 percent), tax avoidance increases by 68.57 percent of the average for the full sample (and 66.33 percent for the medians sample). We further explore this effect below by allowing the associations between the tax system characteristics and tax avoidance to vary across firms where *VarComp* is above versus below the median.

With respect to the remaining country-level control variables in Model (2), we first find that tax avoidance is lower when the level of cross-sectional pre-tax earnings volatility within a country

TABLE 3

Tax Avoidance Regressions

			Dependent Variable: *TaxAvoid*			
			Column A **Full Sample**		**Column B** **Country-Industry-Year Medians**	
Variable	**Hypo.**	**Pred.**	**Coef.**	**t-stat**	**Coef.**	**t-stat**
BTaxC	H1	Neg.	−0.036	−5.34***	−0.051	−5.49***
WW	H2	Neg.	−0.011	−3.56***	−0.010	−2.16***
TaxEnf	H3	Neg.	−0.026	−11.86***	−0.021	−5.69**
Controls						
TaxRate			0.057	2.86***	0.051	1.57*
VarComp			0.144	15.50***	0.131	8.41***
Earnvol			−0.095	−15.51***	−0.065	−6.22***
Factor			−0.011	−5.55***	−0.002	−0.32
Pre-Tax ROA			0.044	3.55***	0.023	0.35
LogSize			−0.002	−2.17**	0.003	0.92
R&D			−0.040	−1.23	−0.128	−0.87
Leverage			0.032	8.06***	0.031	2.01*
SalesGrth			0.065	20.17***	0.056	4.12***
Multi			−0.014	−5.91***	−0.035	−4.96***
Industry Fixed-Effects			Yes		Yes	
Year Fixed-Effects			Yes		Yes	
n			69,301		6,119	
Adjusted R^2			34.7%		21.2%	

*, **, *** Indicate significance at 10, 5, and 1 percent, respectively.

In all regressions, we assess statistical significance using standard errors adjusted for firm clustering for the full sample (Column A) and industry clustering for the medians sample (Column B). We use one-tailed tests when a sign is predicted.

All variables are as defined in Table 2, Panel A.

(*Earnvol*) is higher. As discussed in Atwood et al. (2010), *BTaxC* is highly positively correlated with cross-sectional pre-tax earnings volatility.[31] By including *Earnvol* in our regression, we ensure that our *BTaxC* results are not driven by differences in the cross-sectional volatility of pre-tax earnings rather than required book-tax conformity. Next, we find that cross-country institutional factors (*Factor*) are also negatively associated with tax avoidance, suggesting that tax avoidance is lower in common law countries with stronger investor rights and lower ownership concentration. Including this variable ensures that our tax system characteristics results are not due solely to their correlation with this set of institutional features across countries.

With respect to firm-level control variables, we find that tax avoidance is higher for firms with higher pre-tax return on assets (*Pre-Tax ROA*). Consistent with prior research (Rego 2003; Cazier et al. 2009; Wilson 2009), this result indicates that more profitable firms, which have the greatest incentive to reduce taxes, engage in more tax avoidance. We also find that tax avoidance is lower for large firms (*LogSize*), consistent with Rego (2003), who finds that, for a broad sample of U.S. firms, worldwide effective tax rates (ETRs) increase with firm size. These results are consistent with larger firms engaging in less tax avoidance, as well as with prior literature suggesting that larger firms may act to reduce potential political costs (Zimmerman 1983; Omer et al. 1993).[32] Contrary to prior research that finds a positive relation between R&D expenditures and tax avoidance (Dyreng et al. 2008) and between R&D expenditures and uncertain tax benefits (Cazier et al. 2009), we find no association between tax avoidance and research and development (*R&D*) in our models. However, consistent with prior research that finds a positive association between tax avoidance and leverage (Dyreng et al. 2008) and between uncertain tax benefits and leverage (Cazier et al. 2009), we find that tax avoidance is increasing in leverage (*Leverage*).[33] We also find that firms with higher *SalesGrth* engage in more tax avoidance, consistent with Badertscher et al. (2011) who find that sales growth is negatively associated with annual cash ETRs, suggesting that firms with higher sales growth engage in more tax avoidance.[34] Finally, we find less tax avoidance in firms with multinational operations (*Multi*). Prior research provides mixed evidence on the relation between multinational operations and tax avoidance. Rego (2003) finds that the more extensive a multinational's foreign operations, the higher its ETR, suggesting lower tax avoidance, but Wilson (2009) finds that firms with foreign income are more likely to be tax-shelter participants and Cazier et al. (2009) find that U.S. firms with more extensive foreign operations have greater uncertain tax benefits. Our results are more consistent with those in Rego (2003), suggesting that multinationals engage in less tax avoidance overall than do solely domestic firms.[35]

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[31] In our sample, the Pearson correlation is 0.75 (see Table 2, Panel B).

The Effect of Variable Compensation on the Relations between Tax Avoidance and Tax System Characteristics

Table 4 explores the relations between tax avoidance and tax system characteristics by allowing these relations to vary for firms in countries with high versus low variable compensation (i.e., *VarComp* above and below the median). Columns A and B present the results from estimating Model (3) for the full and medians samples, respectively. Our focus is on the tests of coefficient equality presented at the bottom of the table.

With respect to the relation between tax avoidance and book-tax conformity, the tests of coefficient equality reveal that, for both the full and medians samples, the association between tax avoidance and required book-tax conformity is significantly less negative for firms in countries where the proportion of compensation that is variable (*VarComp*) is high. In fact, when using the full sample, the relation is positive for firms with high *VarComp*. When using the medians sample,

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[32] Note, however, that prior research provides mixed evidence on the relation between firm size and tax avoidance. Contrary to findings in Rego (2003), Cazier et al. (2009) find that larger firms report greater uncertain tax benefits under FIN 48 and Wilson (2009) finds that they are more likely to be tax-shelter participants, consistent with the argument that larger firms engage in more complex tax avoidance transactions.

[33] Note, however, that findings in prior research do not suggest a clear relation between leverage and tax avoidance. In contrast with findings in Dyreng et al. (2008) and Cazier et al. (2009), leverage is negatively associated with tax sheltering (Graham and Tucker 2006; Wilson 2009) and with corporate tax evasion (Joulfaian 2010).

[34] Note, however, that Cazier et al. (2009) find that sales growth is negatively associated with uncertain tax benefits, suggesting that firms with higher sales growth engage in less tax avoidance.

[35] We note that Wilson (2009) uses a sample of 59 firms accused of tax-shelter participation and Cazier et al. (2009) use a sample 578 firm-years that first reported uncertain tax benefits under FIN 48 in 2007. Both are small sample studies where observations are drawn from a population of firms that are considered to be tax aggressive by definition. On the other hand, the sample used in Rego (2003) includes a broad cross-section of U.S. firms (5,379 firm-years). Similarly, our sample includes a broad cross-section of firms from 22 countries. We suggest that this is likely why our results are more consistent with Rego (2003).

TABLE 4

Tax Avoidance Regressions with Equity Compensation Interactions

		Dependent Variable: *TaxAvoid*					
		Column A: Full Sample			Column B: Country-Industry-Year Medians		
Variable	Hypo.	Coef.	t-stat	F-stat	Coef.	t-stat	F-stat
BTaxC × HighVarComp	H1	0.029	3.38***		−0.038	−3.12***	
BTaxC × LowVarComp	H1	−0.051	−6.11***		−0.074	−5.87***	
WW × HighVarComp	H2	−0.005	−1.06		−0.015	−1.45	
WW × LowVarComp	H2	−0.018	−4.25***		0.005	0.68	
TaxEnf × HighVarComp	H3	−0.054	−19.89***		−0.038	−8.10***	
TaxEnf × LowVarComp	H3	−0.001	−0.26		0.001	0.18	
Controls							
TaxRate × HighVarComp		0.316	12.84***		0.213	5.26***	
TaxRate × LowVarComp		−0.034	−1.38		−0.139	−3.71***	
VarComp		0.348	14.73***		0.220	4.40***	
Earnvol		−0.066	−10.85***		−0.044	−3.94***	
Factor		−0.015	−7.22***		−0.010	−2.21**	
Pre-Tax ROA		0.034	2.82***		0.016	0.26	
LogSize		−0.001	−1.94*		0.002	0.66	
R&D		−0.021	−0.64		−0.099	−0.68	
Leverage		0.029	7.36***		0.029	1.90*	
SalesGrth		0.066	20.79***		0.057	4.04***	
Multi		−0.017	−7.14***		−0.041	−5.51***	
Industry Fixed-Effects		Yes			Yes		
Year Fixed-Effects		Yes			Yes		
n		69,301			6,119		
Adjusted R^2		35.8%			24.1%		
Tests of Coefficient Equality							
BTaxC × HighVarComp = BTaxC × LowVarComp				66.25***			4.21*
WW × HighVarComp = WW × LowVarComp				4.48**			3.74*
TaxEnf × HighVarComp = TaxEnf × LowVarComp				405.22***			48.81***
TaxRate × HighVarComp = TaxRate × LowVarComp				151.70***			53.86***

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*, **, *** Indicate significance at 10, 5, and 1 percent, respectively.

In all regressions, we assess statistical significance using standard errors adjusted for firm clustering for the full sample (Column A) and industry clustering for the medians sample (Column B). We use one-tailed tests when a sign is predicted.

All variables are as defined in Table 2, Panel A.

the relation is still negative but it is significantly less negative for firms in home countries with high variable compensation. Thus, book-tax conformity has less of an influence on managers' tax avoidance decisions when managers' compensation contracts provide them with greater incentives to avoid taxes. This result is consistent with results in Hanlon et al. (2007) and Rego and Wilson (2011), which examine tax deficiencies and effective tax rates among U.S. firms. Our results suggest that bonus and equity compensation arrangements motivate managers to undertake more risky tax-planning strategies when countries allow less flexibility in reporting differences between earnings and taxable income.

The tests of coefficient equality reveal that, for the full sample, the negative association between tax avoidance and the use of a worldwide approach is driven by firms in countries with low variable compensation. In addition, for firms in countries where *VarComp* is above the median, the tax approach in the home country (worldwide versus territorial) has no effect on tax avoidance. Note, however, that when we use the medians sample, we find no evidence of a relation between tax avoidance and the tax approach regardless of the level of variable compensation. Overall, these results provide some evidence of a relation between a worldwide approach and tax avoidance, but we refrain from drawing strong inferences given the medians sample results.

With respect to the relation between tax avoidance and the perceived strength of tax enforcement, tests of coefficient equality reveal that tax avoidance is decreasing in the strength of tax enforcement only when the variable portion of compensation is high. This result holds for both the full and the medians samples. A possible explanation for this result is that managers in countries with low variable pay are not motivated to engage in tax avoidance activities even when the strength of enforcement is lower and, as a result, stronger enforcement has no effect.

Finally, tests of coefficient equality indicate that tax avoidance is increasing in the statutory corporate tax rate only when the variable portion of compensation is high. In fact, when *VarComp* is below the median, the relation between tax avoidance and the statutory corporate tax rate is not significant in the full sample and is negative in the medians sample. This result suggests that tax avoidance is increasing in the statutory tax rate only when management compensation is strongly tied to reported earnings, as is bonus and equity compensation. Overall, the results from our medians tests confirm that the variable pay portion of management compensation impacts the relation between tax avoidance and tax system characteristics because managers are incentivized to engage in more tax avoidance when tax avoidance leads to higher compensation.

The Effects of Accruals Components

We now analyze the relations between accruals components and tax avoidance. Table 5, Panel A, Columns A and B present the results of Model (4) for the full and medians samples, respectively. Here, we find that all three accruals components are positively associated with tax avoidance, indicating that some tax avoidance is achieved through accruals management. However, the results for the tax systems characteristics are essentially the same as in Model (2). The only exception is that the relation between tax avoidance and *TaxRate* is no longer significant for the medians sample.

Table 5, Panel B, Columns C and D present the results of Model (5) for the full and medians samples, respectively. Again, all three accruals components are positively associated with tax avoidance and the results for the tax systems characteristics are essentially the same as in Model (3). Thus, the relations between tax avoidance and the tax systems characteristics are not driven solely by tax avoidance through accruals management. This result suggests that altering home-country tax system characteristics not only impacts managers' tax avoidance through accruals management, but also tax avoidance through other mechanisms such as the use of tax havens, transfer pricing, cost sharing arrangements, tax shelters, etc.

TABLE 5

Tax Avoidance Regressions with Controls for Accruals Components

Panel A: Results for Model (4)

			Dependent Variable: *TaxAvoid*			
			Column A: Full Sample		Column B: Country-Industry-Year Medians	
Variable	**Hypo.**	**Pred.**	**Coef.**	**t-stat**	**Coef.**	**t-stat**
BTaxC	H1	Neg.	−0.035	−5.25***	−0.052	−5.60***
WW	H2	Neg.	−0.010	−3.30***	−0.008	−1.84**
TaxEnf	H3	Neg.	−0.026	−11.80***	−0.021	−5.75***
Controls						
TaxRate			0.051	2.58**	0.039	1.20
VarComp			0.143	15.42***	0.126	8.36***
Earnvol			−0.096	−15.84***	−0.065	−6.46***
Factor			−0.012	−5.89***	−0.002	−0.51
Pre-Tax ROA			0.055	4.38***	0.032	0.50
LogSize			−0.001	−2.12**	0.002	0.86
R&D			−0.051	−1.58	−0.156	−1.04
Leverage			0.031	6.96***	0.031	1.63
SalesGrth			0.057	17.35***	0.048	3.35***
Multi			−0.013	−5.70***	−0.034	−4.89***
ΔWC			0.017	9.00***	0.023	2.90***
ΔNCO			0.016	10.15***	0.021	2.41**
ΔFIN			0.017	9.48***	0.024	2.26**
Industry Fixed-effects			Yes		Yes	
Year Fixed-effects			Yes		Yes	
n			69,301		6,119	
Adjusted R^2			35.4%		22.5%	

Panel B: Results for Model (5)

		Dependent Variable: *TaxAvoid*					
		Column C: Full Sample			Column D: Country-Industry-Year Medians		
Variable	**Hypo.**	**Coef.**	**t-stat**	**F-stat**	**Coef.**	**t-stat**	**F-stat**
BTaxC × *HighVarComp*	H1	0.030	3.54***		−0.039	−3.22***	
BTaxC × *LowVarComp*	H1	−0.052	−6.21***		−0.075	−5.96***	
WW × *HighVarComp*	H2	−0.004	−0.76		−0.013	−1.33	
WW × *LowVarComp*	H2	−0.018	−4.08***		0.007	0.89	
TaxEnf × *HighVarComp*	H3	−0.054	−19.80***		−0.037	−8.22***	
TaxEnf × *LowVarComp*	H3	−0.001	−0.35		0.001	0.15	
Controls							
TaxRate × *HighVarComp*		0.307	12.52***		0.198	4.95***	
TaxRate × *LowVarComp*		−0.036	−1.47		−0.146	−3.76***	

(continued on next page)

TABLE 5 (continued)

		Dependent Variable: *TaxAvoid*					
		Column C: Full Sample			**Column D: Country-Industry-Year Medians**		
Variable	**Hypo.**	**Coef.**	**t-stat**	**F-stat**	**Coef.**	**t-stat**	**F-stat**
VarComp		0.343	14.57***		0.211	4.44***	
Earnvol		−0.068	−11.18***		−0.045	−4.11***	
Factor		−0.015	−7.51***		−0.011	−2.41**	
Pre-Tax ROA		0.045	3.67***		0.025	0.41	
LogSize		−0.001	−1.88*		0.002	0.61	
R&D		−0.032	−0.98		−0.127	−0.82	
Leverage		0.028	6.33***		0.029	1.56	
SalesGrth		0.059	17.91***		0.050	3.34***	
Multi		−0.016	−6.91***		−0.039	−5.47***	
ΔWC		0.017	8.93***		0.021	2.79***	
ΔNCO		0.016	9.99***		0.020	2.32**	
ΔFIN		0.017	9.30***		0.023	2.22**	
Industry Fixed-effects		Yes			Yes		
Year Fixed-effects		Yes			Yes		
n		69,301			6,119		
Adjusted R^2		36.4%			25.2%		
Tests of Coefficient Equality							
BTaxC × HighVarComp = BTaxC × LowVarComp				69.83***			4.42**
WW × HighVarComp = WW × LowVarComp				5.06***			3.94*
TaxEnf × HighVarComp = TaxEnf × LowVarComp				400.22***			49.78***
TaxRate × HighVarComp = TaxRate × LowVarComp				146.40***			52.94***

*, **, *** Indicate significance at 10, 5, and 1 percent, respectively.
In all regressions, we assess statistical significance using standard errors adjusted for firm clustering for the full sample (Columns A and C) and industry clustering for the medians sample (Columns B and D). We use one-tailed tests when a sign is predicted.
All variables are as defined in Table 2, Panel A.

In sum, we provide evidence that three tax system characteristics—required book-tax conformity, worldwide versus territorial approach, and the perceived strength of tax enforcement—impact tax avoidance after controlling for the statutory corporate tax rate and for firm-specific factors identified in prior studies. Moreover, the degree to which tax system characteristics impact tax avoidance depends on the structure of management compensation contracts—specifically the extent to which management compensation comes from variable pay. We suggest that this occurs because bonus and equity compensation provides greater incentives for managers to avoid taxes regardless of the tax system characteristics in the home country.

V. ROBUSTNESS TESTS

In this section, we report the results of a number of untabulated robustness tests of our main model estimated with the full sample. First, we re-estimate our tax avoidance variable using pre-tax earnings *unadjusted* for exceptional items in order to investigate whether the results are influenced by our decision to use pre-tax earnings *before* these exceptional items. We find that all results for our tax systems characteristics are robust.[36]

Second, to test whether associations between tax avoidance and the tax system characteristics are driven solely by multinational firms, we remove all firms with multinational operations (where $Multi = 1$) and estimate our models without the *Multi* variable. We find that the results for *BTaxC* and *TaxEnf* are robust, but the coefficient for *WW* is not significant at conventional levels. However, when we estimate the model with only the multinational firms in the sample, we find that the results for *WW* and *TaxEnf* are robust, but the coefficient on *BTaxC* becomes positive and significant, indicating that the negative relation between tax avoidance and *BTaxC* is driven by domestic-only firms. This suggests that firms without foreign operations have available fewer tax avoidance strategies that do not create book-tax differences and, thus, higher required book-tax conformity is a more effective deterrent to tax avoidance for firms with only domestic operations.

Prior studies examining cross-country differences in the properties of accounting earnings use an indicator variable approach to measure book-tax conformity (e.g., Ali and Hwang 2000; Hung 2001; Leuz et al. 2003) where the indicator variable is based on subjective assessments of each country's book-tax conformity level.[37] Therefore, as a robustness test, we replace our *BTaxC* variable with the indicator variable used in those studies and find that all of our inferences about the effects of book-tax conformity and the remaining tax system characteristics hold.

Finally, we consider the impact of firms reporting under International Financial Reporting Standards (IFRS) versus domestic accounting standards. We find that the results for all three of our tax system characteristics hold when we add an indicator variable for firm-years reported under IFRS to both Models (2) and (4).

VI. CONCLUSIONS

This study examines whether tax system characteristics (i.e., required book-tax conformity, worldwide versus territorial approach, and perceived strength of tax enforcement) impact tax avoidance. We find that firms avoid more taxes, on average, when the home country has lower required book-tax conformity, a worldwide approach, and a lower perceived strength of tax enforcement. Moreover, the extent to which tax avoidance is associated with these tax system characteristics depends on the portion of management compensation that is from variable pay (i.e., bonuses, stocks, and stock options).

Our paper responds to proposals for tax reform in the U.S. and to calls for more research on the determinants of tax avoidance. Our results suggest that, in addition to firm-specific factors identified in prior research (i.e., performance, size, intangibles, debt financing, growth, and the presence of multinational operations), tax avoidance is driven by home-country tax system characteristics and the interaction between those characteristics and the structure of management compensation contracts.

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[36] For all of our additional tests, we define results as "robust" when inferences drawn from coefficient sign and statistical significance (using $p < 0.10$ or better) are the same as those drawn from coefficients reported in the main tables. We discuss all results that are not robust in the text.

[37] Hung (2001) constructs the book-tax conformity variable using five factors derived from international tax and accounting summaries: the existence of deferred taxes; whether additional accelerated depreciation is allowed; whether amortization periods depend on tax laws; whether lease capitalization depends on tax laws; and a subjective determination of the relation between book and tax income. These factors may be suggestive, but many other factors result in differences between accounting earnings and taxable income in countries with low required book-tax conformity, and their measure is very highly correlated with an indicator variable for common versus code law countries.

Our paper has implications for tax policymakers in evaluating proposed tax reforms. First, our results provide evidence that supports proposals to decrease tax avoidance in the U.S. by increasing the strength of enforcement efforts. Second, proposals to increase required book-tax conformity may deter tax avoidance for domestic-only firms, but are likely to be less effective for multinational firms that have more opportunities to avoid taxes using strategies that do not create book-tax differences in the home country. Third, if the U.S. adopted a territorial approach, then tax avoidance is likely to increase, rather than decrease. Finally, the structure of management compensation contracts (specifically, the proportion of compensation that is variable) significantly impacts managers' tax avoidance responses to tax system characteristics.

We suggest two caveats regarding our results. First, using an indicator variable for the worldwide versus territorial tax system is a very simple test of differences across these two systems and we view our results as a first step in understanding the impact of this tax system characteristic on tax avoidance. We suggest that further research is needed in this area. Second, there is the potential for a mechanical relation between our tax avoidance measure and the statutory tax rate. Thus, we include the statutory tax rate as an important control variable, but we suggest this is an important area that should be addressed by future research.

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Audit Quality and the Trade-Off between Accretive Stock Repurchases and Accrual-Based Earnings Management

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ABSTRACT: We examine whether audit quality affects the trade-off between accrual-based and real earnings management. We hypothesize that firms motivated to manage earnings per share (EPS) to meet or beat consensus analysts' forecasts are more likely to engage in accretive stock repurchases (a form of real earnings management) when their ability to manage earnings through accruals is constrained by high audit quality. We find that firms with high audit quality are more likely to use accretive stock repurchases and less likely to use accrual-based earnings management to meet or beat consensus analysts' forecasts. Our results are robust to various controls for endogeneity concerns.

Keywords: *real earnings management; stock repurchases; audit quality; audit industry specialization.*

I. INTRODUCTION

We examine whether audit quality affects managers' choices between accrual-based earnings management and accretive stock repurchases,[1] one form of real earnings management. We focus on accretive stock repurchases where managers' motivations

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[1] Stock repurchases affect both the numerator and denominator of reported EPS. Opportunity costs (in terms of forgone potential profit) associated with the cash used to fund stock repurchases affect the numerator and the timing and amount of stock repurchases affect the number of shares in the denominator. Stock repurchases are defined as accretive when the numerator and denominator effects of a repurchase jointly increase reported EPS by at least $0.01.

are to meet or beat earnings per share (EPS) targets rather than to execute normal financing activities. While prior research provides evidence that high audit quality is negatively associated with managers' ability to opportunistically report accruals (Krishnan 2003; Balsam et al. 2003), our paper examines whether managers constrained in their use of accrual-based earnings management are more likely to use open-market stock repurchases as a substitute. Similar to accrual-based earnings management, open-market stock repurchases affect EPS, involve managerial discretion, and lack adequate disclosure prior to their execution. However, unlike accrual-based earnings management, stock repurchases are not subject to auditor scrutiny since they involve decisions about a real financing activity. Given the difference in auditor scrutiny, we argue that in the presence of high audit quality, managers trade off the use of accrual-based earnings management and are more likely to use accretive stock repurchases to meet or beat consensus analysts' EPS forecasts.

Our study brings together and extends two research streams. First, Hribar et al. (2006) examines open-market stock repurchases and provides evidence that firms use accretive stock repurchases to meet or beat analysts' forecasts. We extend their paper by examining whether accretive stock repurchases are more frequently used when accrual-based earnings management is likely constrained by the presence of high audit quality. Second, prior research (Cohen and Zarowin 2010; Chi et al. 2011; Zang 2012) examines the trade-off between operational real earnings management activities (i.e., cash flow from operations, production costs, and discretionary expenses) and accrual-based earnings management for firms employing high-quality auditors. Evidence from these studies is mixed. These studies document a trade-off for seasoned equity offering (SEO) firms (Cohen and Zarowin 2010) and for firms sharing strong incentives to manage earnings upward (Chi et al. 2011), while Zang (2012) fails to document a trade-off around earnings benchmarks. We extend these studies by presenting evidence of a trade-off using a financing activity, open-market stock repurchases, as the form of real earnings management focused on meeting or beating analysts' forecasts. Examining accretive stock repurchases is advantageous because they are not subject to potential misspecifications present in measures of operational real earnings management that rely on fitted models (Cohen et al. 2011). In addition, our use of both industry auditor specialization and propensity score matching addresses concerns raised by Lawrence et al. (2011) that other firm characteristics, such as client size, subsume Big N as an appropriate measure of audit quality.

We examine firms that meet or beat consensus analysts' forecasts *ex post* to determine whether firms employing industry-specialist auditors (Hogan and Jeter 1999; Krishnan 2003; Gul et al. 2009) are more likely to use repurchases to meet or beat analysts' forecasts as compared to other firms who are able to do so absent an accretive stock repurchase. We similarly test whether firms with industry-specialist auditors are less likely to use discretionary current accruals to meet or beat analysts' forecasts as compared to firms that are able to do so absent the use of discretionary current accruals. We also directly compare firms that likely share a common *ex ante* expectation of missing consensus analysts' forecasts, but *ex post* meet or beat consensus analysts' forecasts either through accretive stock repurchases or discretionary current accruals. Our findings provide consistent evidence that firms that both engage industry-specialist auditors and meet or beat analysts' forecasts are more likely to use accretive stock repurchases and less likely to use discretionary current accruals to meet or beat analysts' forecasts *ex post*.

Our contribution of documenting a trade-off between real earnings management and accrual-based earnings management to meet or beat consensus analysts' forecasts not only extends prior accounting research, but also has implications in corporate governance. As boards of directors and investors rely on high audit quality to curtail accrual-based earnings management, they should be aware that managers may substitute accretive stock repurchases to meet or beat EPS forecasts. If a higher frequency of stock repurchases motivated by earnings management incentives potentially obfuscates earnings quality or harms long-term firm value, then alternative governance mechanisms may be necessary to curtail this behavior. We discuss these implications in the conclusion of the paper.

In addition to our main tests, we perform several additional tests to address potential concerns with our results. We provide evidence that our results are robust when we explicitly control for expected repurchases in our models, alleviating concerns that analysts and investors fully anticipate share repurchases. Additionally, we examine whether our results are driven by observable differences in firm characteristics and/or misspecification in regression analysis. Our results remain robust when we use propensity score matching on auditor industry specialization to more directly control for firm characteristics that could spuriously drive our main results. We also provide evidence that earnings management incentives around analysts' forecasts are an important component of our findings. Finally, our results are robust for the first three quarters of the fiscal year, which indicates that results in the fourth quarter, when auditor oversight is more stringent, do not drive our main results.

The remainder of the paper is as follows. Section II discusses the prior literature and develops our hypotheses. Sections III and IV present the research methodology and the results of the paper, respectively. Section V reports the sensitivity analyses. Section VI discusses the implications of our findings and concludes the paper.

II. PRIOR LITERATURE AND HYPOTHESES

Earnings Benchmarks and Stock Repurchases

Analyst forecasted EPS is commonly used as a performance benchmark in capital markets. Firms that consistently meet or beat analysts' EPS forecasts enjoy higher credibility (Graham et al. 2005) and valuation premiums (Kasznik and McNichols 2002; Brown and Caylor 2005) that persist even if EPS is knowingly managed (Bartov et al. 2002). When EPS benchmarks are missed, firms suffer severe stock price losses (Skinner and Sloan 2002), managerial compensation declines (Matsunaga and Park 2001), and managers perceive that their reputations are at risk (Graham et al. 2005).

Managers engaging in earnings management select from two broad categories of mechanisms. Accrual-based earnings management occurs when managers choose purposefully to influence earnings through accounting choices and estimates (Healy and Wahlen 1999). Real earnings management involves managerial decisions affecting both cash flows and reported earnings, such as R&D expenditures, capital investment decisions, and stock repurchases. Real earnings management is a departure from normal business activities that focuses more on short-term reporting goals than on promoting firm value (Roychowdhury 2006). Both methods of earnings management are questionable because they are actions managers take to mislead stakeholders about firm performance or influence accounting-based contractual arrangements (Healy and Wahlen 1999).[2]

Bartov (1993) and DeFond and Jiambalvo (1994) find that managers use accruals when faced with the prospect of just missing analysts' forecasted EPS. Payne and Robb (2000) show that managers use accruals to manipulate EPS to meet or beat analysts' forecasts when forecast dispersion is low and Burgstahler and Eames (2006) document that managers meet or beat EPS forecasts through both accrual-based earnings management and expectations management. In addition, Ayers et al. (2006) find that efforts to meet EPS targets through accruals extend to pseudo targets that may, in part, relate to analysts' forecasts. Barton and Simko (2002) suggest that aggressive accrual-based earnings management is constrained by the cumulative nature of accruals, suggesting that managers may turn to other earnings management tools when those limits are reached. In contrast, Beaver et al. (2007) argues that the asymmetric nature of income taxes and special items may drive the distribution of earnings near earnings benchmarks rather than earnings management incentives. Consistent with this finding, Dechow et al. (2003) find that small-profit-and-loss firms have similar proportions of both total and positive discretionary accruals and fail to document an association between accrual-based earnings management and meeting or beating analysts' forecasts. Overall, prior research provides general, but not unqualified support for managers' use of accrual-based earnings management to meet or beat earnings benchmarks.

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[2] Managerial choices between accrual-based and real earnings management is not assumed to be an "either/or" decision. Instead, we expect that in the face of higher audit quality, managers may still misreport accruals, but at a reduced rate. In lieu of this, managers may increase their use of real earnings management techniques.

A few studies examine real earnings management to meet or beat EPS targets. Roychowdhury (2006) finds a positive association between real earnings management and meeting or beating analysts' forecasted EPS. With respect to accretive stock repurchases as a real earnings management tool, Hribar et al. (2006) find that accretive stock repurchases occur more frequently when firms are faced with an EPS shortfall and show that negative stock price responses are tempered in the presence of accretive stock repurchases. In addition, Bens et al. (2003) find that firms pressured to demonstrate EPS growth are more likely to use stock repurchases when faced with the dilutive effects of employee stock options. Our paper extends the literature related to real earnings management focused on meeting or beating an EPS benchmark and investigates whether audit quality affects the use of accretive stock repurchases as a real earnings management tool.

Earnings Management and Audit Quality

Audit quality is the joint probability that an auditor will discover and report material misstatements found in financial statements (DeAngelo 1981). Auditor industry specialization as a proxy for audit quality has been widely studied in the literature. Bedard and Biggs (1991) find that as auditors' industry-specific expertise increases, their ability to locate data errors also increases. Industry-specific auditors more likely identify extraordinary transactions, have more exposure to industry best practices, and have improved ability to identify inherent and control risks (Krishnan 2003). The use of industry-specialist auditors is found to be negatively correlated with violations of accounting standards (O'Keefe et al. 1994) and restatements (Romanus et al. 2008), and positively associated with disclosure quality (Dunn and Mayhew 2004). Together, these studies document a link between high audit quality and the presence of industry-specialist auditors.

Prior research (Krishnan 2003; Balsam et al. 2003) also provides evidence that firms engaging high-quality auditors are associated with constrained accrual-based earnings management. These studies document that clients of industry specialists have lower discretionary accruals than clients of non-industry specialists, consistent with industry specialists constraining opportunistic accrual choices.[3] These studies suggest the presence of high audit quality potentially constrains the attempted use of accruals-based earnings management because of the higher risk that the behavior will be discovered.

In contrast, auditors do not scrutinize real earnings management since their responsibility is to provide reasonable assurance that financial statements are presented in accordance with generally accepted accounting principles. Auditors ensure that the financial statements faithfully represent the real activities of firms, but are not required to evaluate managers' motivations for decisions related to real activities. Although Roychowdhury (2006) asserts that real earnings management potentially imposes greater long-term costs on firms because it has a negative effect on future cash flows, he also argues that accrual-based earnings management imposes significantly greater short-term costs on managers. This makes real earnings management an attractive choice for managers who are motivated to achieve short-term earnings goals, but are inhibited by high audit quality and therefore limited in their ability to use accrual-based earnings management techniques.

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[3] Balsam et al. (2003) and Krishnan (2003) use the absolute value of discretionary total accruals. We model "as-if" earnings by subtracting discretionary current accruals from reported net income. We posit that discretionary current accruals better reflect managerial efforts to manage short-term earnings benchmarks such as quarterly analysts' forecasts.

Evidence relating audit quality to a trade-off between the use of accrual-based and real earnings management is mixed. Cohen and Zarowin (2010) provide evidence that firms with Big N auditors are more likely to engage in real earnings management than accrual-based earnings management when involved in a seasoned equity offering. Chi et al. (2011) find that auditor specialization, and to a lesser extent, Big N auditors, are positively associated with real earnings management and negatively associated with accrual-based earnings management in a combined sample of firms that have strong incentives to manage earnings in order to meet or just beat earnings benchmarks or to support seasoned equity offerings. Conversely, using earnings benchmarks, Zang (2012) fails to document a positive association between Big N auditors and real earnings management.

Cohen et al. (2011) examine the estimation models used to construct proxies for real earnings management and raise concerns that the models are likely to be misspecified. They suggest matching on performance as a partial solution, but note that this approach is also subject to specification concerns. Our use of stock repurchases circumvents these issues because they do not involve estimation models and therefore are less subject to misspecification. Additionally, Lawrence et al. (2011) suggest that the effect of Big N auditors may reflect fundamental differences in client characteristics rather than differential audit quality. We address this concern by using industry-specialist auditors as our proxy for high audit quality and also use propensity score matching as an additional test to ensure that our results are not driven by differences between firms with and without industry-specialist auditors.

Accretive Stock Repurchases as a Real Earnings Management Tool

Stock repurchases with the potential to increase reported EPS typically take the form of open-market repurchases, which occur when firms buy shares of their own stock on the open market (Vermaelen 2005). The transaction for open-market repurchases involves an actual stock buyback and corresponding cash expenditure. Open-market repurchases accounted for 94.3 percent of all repurchases and 95.2 percent of the total dollar value of shares repurchased during the 1990s (Grullon and Ikenberry 2000). Open-market repurchase programs must be formally approved by a firm's board of directors, typically target 5 percent of its total share base, last two to three years, and on average result in the repurchase of 77 percent of the targeted number of shares initially approved (Stephens and Weisbach 1998). Our tests focus on open-market repurchases.

The net effect of stock repurchases on EPS depends jointly on three factors: (1) the timing of the repurchase, (2) the number of shares repurchased, and (3) the forgone future returns from the cash used to repurchase stock.[4] The first two factors increase EPS by decreasing the EPS denominator. Stock repurchases that are larger and occur earlier in a quarter have a greater influence on the denominator of reported EPS because of their stronger effect on the time-weighted average of shares outstanding. The third factor decreases EPS by decreasing the EPS numerator because of forgone returns that the cash used in the repurchase could have generated. Repurchases are accretive only when the timing and amount are sufficient to outweigh the decretive impact of the forgone returns.

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[4] The following discussion assumes that EPS is positive and that the numerator and denominator effects of a repurchase increase reported EPS by at least \$0.01. In our sample selection, we eliminate firm-quarters reporting non-positive EPS and round EPS amounts to the nearest \$0.01.

Similar to other real earnings management activities, stock repurchases are likely to be costly to firms because the cash used in the repurchase is not available for other income-generating activities. Firms that have more profitable uses of cash are less likely to engage in repurchases as an EPS management tool. As a result, managers must consider the trade-offs between using accretive stock repurchases and other earnings management activities.

Although boards of directors approve open-market repurchase programs, managers are left with considerable discretion with respect to the timing of a stock repurchase (Vermaelen 2005; Cook et al. 2003). In addition, the Securities and Exchange Commission (SEC) requires limited disclosure about repurchases by public firms. Adopted in 1982, SEC Rule 10b-18 originally required firms to disclose amounts spent on stock repurchases. The rule was amended in December 2003 to provide additional disclosures about the average price per share, the number of shares repurchased as part of a repurchase program, and the maximum number of shares that may be repurchased in the future under a repurchase program.

The SEC disclosure requirements are limited to reporting required in quarterly financial reports filed after the quarter end. Therefore, analysts and investors are unlikely to be able to precisely anticipate current-quarter repurchase effects on EPS. The combination of managerial flexibility and limited mandatory disclosures enables firms to use open-market stock repurchases as an effective real earnings management device.

Hypotheses

We focus our study on two hypotheses incorporating accretive stock repurchases, accrual-based earnings management, and audit quality. We argue that high audit quality is likely to constrain the use of discretionary accruals to meet or beat analysts' forecasts and, therefore, firms with high audit quality are more likely to use accretive stock repurchases as a substitute. While H1 focuses on the general association between audit quality and accretive stock repurchases, H2 examines the association between audit quality and the potential trade-off between accretive stock repurchases and accruals-based earnings management. Both are formally stated as null hypotheses as follows:

H1: High audit quality *is unrelated* to the use of accretive stock repurchases to meet or beat consensus analysts' forecasts.

H2: High audit quality *is unrelated* to a trade-off between the use of accretive stock repurchases and accrual-based earnings management to meet or beat consensus analysts' forecasts.

III. METHODOLOGY

Identification of Firms Suspected of Using Accretive Stock Repurchases for Real Earnings Management

Firms repurchase common stock for multiple reasons,[5] so it is important that our research design identifies firms that likely use accretive stock repurchases to manage EPS. Therefore, we identify firms that would have missed their quarterly consensus analysts' forecasts, but actually meet or beat forecasts because they made an accretive stock repurchase. We term these suspect repurchases.

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[5] Skinner (2008) provides examples of competing motivations, including the signaling of future prospects, the issuing of stock options, and using stock swaps as part of merger and acquisition activities. These competing motivations may result in accretive repurchases, but we distinguish them from efforts to manage EPS.

Since firms do not formally disclose the exact timing of stock repurchases in their financial reports, we must estimate the effects of stock repurchases on reported EPS. We follow Hribar et al. (2006) in estimating the weighted average common shares outstanding absent a stock repurchase. We estimate the EPS denominator as the beginning common shares outstanding plus 50 percent times the number of shares issued during the quarter.[6] This procedure assumes that shares are issued evenly over the quarter and allows the repurchase timing parameter to vary across firm-quarters. If a firm does not employ a stock repurchase for a particular period, we set common shares outstanding equal to the number of common shares reported outstanding at the end of that period.

Alternative uses for the cash used to repurchase stock (i.e., growth opportunities and debt refinancing) affect the numerator of EPS and can greatly influence managers' decisions to use repurchases as an EPS management tool (Hribar et al. 2006). Therefore, we follow Hribar et al. (2006) in estimating opportunity costs associated with stock repurchases by calculating the forgone dollar return on cash used for repurchases (C_t). We estimate excess cash as cash balances in excess of 6 percent of assets for retail firms and 2 percent of assets for all other firms (Copeland et al. 2000). If excess cash is greater than or equal to the repurchase dollar amount, then we calculate the forgone return by multiplying the repurchase dollar amount by the three-month Treasury bill rate for the quarter. If excess cash is less than the repurchase dollar amount, we assume the firm borrows cash in order to execute the repurchase. In such cases, we apply a firm's average interest rate to the incremental funds borrowed.[7] We convert the estimated forgone dollar return into an after-tax amount using Graham and Mills' (2008) simulated marginal tax rates, which are highly correlated with marginal rates based on actual tax returns.[8]

We calculate both the actual and pre-repurchase forecast error to identify suspect repurchases where firm-quarters meet or beat consensus analysts' forecasts, but would have missed the forecasts absent accretive stock repurchases. Actual forecast error (*ACTUAL_FE*) is reported EPS minus the final consensus analysts' forecasts for each firm-quarter. Consistent with Hribar et al. (2006) and Roychowdhury (2006), we assume that managerial expectations at the beginning of a quarter are represented by the final actual consensus forecasts for the quarter.

To construct the pre-repurchase forecast error, we first compute EPS "as if" firms did not repurchase stock during a quarter. We calculate *ASIF_EPS1* by estimating the denominator and numerator effects of accretive stock repurchases on EPS as follows:

$$ASIF\_EPS1_{i,t} = \frac{NI_{i,t} + C_{i,t}}{SHARESOUT_{i,t-1} + 0.5{*}SHARESISSUED_{i,t}}.$$

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[6] Prior to December 17, 2003, SEC Rule 10b-18 did not require firms to disclose the number of shares issued or the number of shares repurchased as part of a repurchase program. Therefore, it is necessary to estimate the number of shares issued and repurchased for a significant number of observations that occur prior to Rule 10b-18. As necessary, we follow Stephens and Weisbach (1998) to estimate the number of shares repurchased by dividing the dollar amount of common stock repurchased by the average monthly closing stock price for the quarter. We then calculate the number of shares issued as ending shares outstanding minus beginning shares outstanding plus shares repurchased. To address concerns over estimation error, we apply the estimation procedure to firm-quarters where actual repurchases are available and find that the two are highly correlated (> 0.95, p-value < 0.0001). We use actual repurchases when available, but this additional test reduces concerns over estimation error when actual repurchases are unavailable. Our results remain robust when using the estimation method used by Hribar et al. (2006).

[7] We estimate each firm's borrowing rate following Francis et al. (2005). We calculate a firm's borrowing rate by taking interest expense divided by average debt for the quarter. If the information is not available to estimate a firm's borrowing rate, then we use the average industry (two-digit SIC) borrowing rate for that quarter.

[8] We thank John Graham for providing data on marginal tax rates.

In this calculation, $ASIF\_EPS1_{i,t}$ represents the estimated EPS absent accretive stock repurchases. $NI_{i,t}$ is reported earnings before extraordinary items available to common shareholders for the firm-quarter. $C_{i,t}$ is the estimated forgone dollar return on cash used for accretive stock repurchases. $SHARESOUT_{i,t-1}$ is the reported number of diluted common shares outstanding if analysts' forecast diluted EPS, otherwise primary common shares outstanding, at the beginning of the firm-quarter. Finally, $SHARESISSUED_{i,t}$ is the actual or estimated number of common stock shares issued during a firm-quarter depending on data availability. All variable descriptions are provided in Appendix A.

Next, we remove the EPS effect of stock repurchases from the actual forecast error. We label the EPS effect of stock repurchases as *EPSImpact1* and calculate it as follows:

$$EPSImpact1_{i,t} = ReportedEPS_{i,t} - ASIF\_EPS1_{i,t}.$$

EPSImpact1 is positive for accretive stock repurchases and negative for decretive stock repurchases.

The pre-repurchase forecast error is calculated as the actual forecast error less the EPS effect of share repurchases. Specifically:

$$ASIF\_FE1_{i,t} = ACTUAL\_FE_{i,t} - EPSImpact1_{i,t}.$$

ASIF_FE1 is negative for firm-quarters that would have missed consensus analysts' forecasts absent the effects of accretive stock repurchases. For firms that do not repurchase common stock, *ASIF_FE1* equals *ACTUAL_FE*.

Finally, we define suspect repurchases as those that would have missed analysts' forecasts by five cents or less, but are able to meet or beat forecasts by no more than five cents with a stock repurchase. Specifically, we construct an indicator variable ($SUSPECT\_REPURCHASE1_{i,t} = 1$) as our measure for suspect repurchases, where *ASIF_FE1* is $\geq -\$0.05$, but $< \$0.00$ *and ACTUAL_FE* is $\geq \$0.00$, but $\leq \$0.05$ for a firm-quarter.[9] *SUSPECT_REPURCHASE1* is equal to 0 for all other firm-quarters. Consistent with prior research (Dhaliwal et al. 2004; Cook et al. 2008; Gleason and Mills 2008), we limit the construction of *SUSPECT_REPURCHASE1* to within five cents of analysts' forecasts to reduce the influence of motivations for stock repurchases unrelated to efforts to manage EPS within our setting.

Our final identification of suspect repurchases is based on (1) *ex ante* managerial expectations of falling short of consensus analysts' forecasts and (2) *ex post* use of accretive stock repurchases by managers used specifically to meet or beat consensus analysts' forecasts of EPS. As *ex ante* expectations of falling short or *ex post* reported EPS deviates further from analysts' forecasts, repurchases that increase EPS are likely less motivated by or are a noisier measure of earnings management behavior. Since real earnings management in the form of accretive stock repurchases expends significant amounts of firm resources, it can be a very costly form of earnings management. Therefore, accretive repurchases are most likely to occur as an EPS-management tool as negative values of *ASIF_FE1* approach 0, making it more likely they are helpful in meeting or beating analysts' forecasts. Similarly, as *ACTUAL_FE* becomes increasingly positive, repurchase behavior is less likely to be an effort to manage EPS to meet analysts' forecasts.

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[9] This definition of suspect repurchases captures 85 percent of repurchase firm-quarters with *ex ante* managerial expectations of falling short of analysts' forecasts and *ex post* use of accretive repurchases to meet or beat analysts' forecasts. Our main results are robust when we expand the definition to 95 percent of possible suspect repurchases. As expected, our results become less robust as we further increase the forecast error limits.

Identification of Firms Suspected of Using Accrual-Based Earnings Management

We similarly identify firm-quarters during which firms appear to be using accrual-based earnings management to meet or beat analysts' forecasts and term these suspect accruals. We identify suspect accrual firms as those that would have missed analysts' forecasts absent the use of discretionary current accruals by five cents or less, but were able to meet or beat the forecasts by no more than five cents with their use. Our tests employ discretionary current accruals (see Guenther 1994; Teoh et al. 1998) because earnings management efforts focused on small changes in EPS and one-time quarterly benchmarks are more likely to be focused on current operating accruals.

Following Louis (2004), we measure quarterly discretionary current accruals using a two-step approach. First, we model normal current accruals within each two-digit SIC industry year as a function of both the change in revenue and the change in receivables for each firm-quarter using the following equation:

$$\frac{Current\ Accruals_{i,t}}{ATQ_{i,t-1}} = \beta_1\left(\frac{1}{ATQ_{i,t-1}}\right) + \beta_2\left(\frac{\Delta REVQ_{i,t} - \Delta RECQ_{i,t}}{ATQ_{i,t-1}}\right) + \varepsilon_{i,t}.$$

Current accruals are measured as the difference between quarterly income before extraordinary items and quarterly operating cash flows less depreciation and amortization expense. The change in revenue and change in receivables is the difference between the current and prior quarter of each measure, respectively. The residual from the model, $\varepsilon_{i,t}$, represents discretionary current accruals and is equal to actual current accruals less estimated current accruals. The model scales discretionary current accruals by lagged total assets ($ATQ_{i,t-1}$) so we multiply the regression residual by its corresponding lagged total assets to calculate an unscaled measure of discretionary current accruals ($DACC_{i,t}$).

Similar to our approach for constructing suspect repurchases, we calculate "as-if" EPS absent discretionary current accruals (net of income taxes) as follows:

$$ASIF\_EPS\_ACCRUALS_{i,t} = \frac{NI_{i,t} - DACC_{i,t}(1 - T_{i,t})}{Weighted\ Average\ SHARESOUT_{i,t}}.$$

$NI_{i,t}$ is income before extraordinary items available to common shareholders, $DACC_{i,t}$ is as defined above, and $T_{i,t}$ is the firm's estimated marginal tax rate. The numerator provides an estimate for net income absent the effects of discretionary current accruals. *Weighted Average SHARESOUT*$_{i,t}$ is the shares used by the firm to calculate EPS, and is diluted shares when analysts' forecast diluted EPS, and primary shares otherwise. Next, we isolate the impact of discretionary current accruals on EPS (*EPSImpact1_ACCRUALS*) as follows:

$$EPSImpact1\_ACCRUALS_{i,t} = ReportedEPS_{i,t} - ASIF\_EPS\_ACCRUALS_{i,t}.$$

EPSImpact1_ACCRUALS is positive for income-increasing discretionary current accruals and negative for income-decreasing discretionary current accruals.

We use the actual forecast error (*ACTUAL_FE*) and "as-if" forecast error absent discretionary current accruals (*ASIF_FE_ACCRUALS*) to identify suspect current accruals. Actual forecast error (*ACTUAL_FE*) is calculated as before and the "as-if" forecast error absent discretionary accruals is calculated as the actual forecast error less the EPS impact from discretionary current accruals. Specifically:

$$ASIF\_FE\_ACCRUALS_{i,t} = ACTUAL\_FE_{i,t} - EPSImpact1\_ACCRUALS_{i,t}.$$

ASIF_FE_ACCRUALS is negative for firm-quarters that would have missed consensus analysts' forecasts absent the effects of discretionary current accruals.

Finally, we define suspect accruals as those that would have missed analysts' forecasts by five cents or less, but are able to meet or beat forecasts by no more than five cents with discretionary current accruals. We construct an indicator variable ($SUSPECT\_ACCRUALS_{i,t} = 1$) as our measure for suspect accruals, where $ASIF\_FE\_ACCRUALS \geq -\$0.05$, but $< \$0.00$ *and* $ACTUAL\_FE$ is $\geq \$0.00$, but $\leq \$0.05$ for a firm-quarter. *SUSPECT_ACCRUALS* is equal to 0 for all other firm-quarters.

Audit Quality

We use an industry-specialist auditor designation as our measure of high audit quality. Consistent with prior research (e.g., Hogan and Jeter 1999; Krishnan 2003; Gul et al. 2009), we define industry specialization as the Big N audit firm with the largest market share (in terms of clients' total assets[10]) within each two-digit SIC industry group on an annual basis.[11] High audit quality is equal to 1 ($HAQ_{i,t} = 1$) when the audit firm is classified as an industry-specialist auditor, and 0 otherwise.

Empirical Models

Our empirical model for suspect repurchases uses the following logistic regression:

$$\begin{aligned} SUSPECT\_REPURCHASE1_{i,t} = {} & \alpha + \beta_1 HAQ_{i,t} + \beta_2 Repurchase_{i,t-1} + \beta_3 Repurchase_{i,t-2} \\ & + \beta_4 Forecast\ Dispersion_{i,t} + \beta_5 Cash_{i,t-1} + \beta_6 CAPEX_{i,t-4,t-1} \\ & + \beta_7 Dividend\ Yield_{i,t-1} + \beta_8 Debt_{i,t-1} + \beta_9 Size_{i,t-1} \\ & + \beta_{10} Size^2_{i,t-1} + \beta_{11} Size^3_{i,t-1} + \beta_{12} ROA_{i,t} + \beta_{13} MTB_{i,t} \\ & + e_{i,t}. \end{aligned} \tag{1}$$

Suspect repurchases ($SUSPECT\_REPURCHASE1_{i,t} = 1$) are modeled as a function of high audit quality as well as a firm's prior stock repurchases, cash levels, capital expenditures, dividends, debt levels, size, performance, and growth. We use nonlinear firm size terms consistent with Gul et al. (2009). We also include *Forecast Dispersion*, calculated as the standard deviation of the individual analyst' forecasts for each firm-quarter, as a control for the predictability of a firm's EPS and the definitiveness of the firm's EPS target in a quarter (Payne and Robb 2000).

$\beta_1(HAQ_{i,t} = 1)$ captures the relation between high audit quality and the likelihood that accretive stock repurchases are used to manage EPS. A positive and significant estimate for β_1 is consistent with a positive association between high audit quality and EPS management through accretive stock repurchases.

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[10] Some past studies use audit fees within industry to define "expert," but fee data are only available after the year 2000. We use the total assets measure since both measures are used in the literature and our sample predates 2000. As a sensitivity test, we do not require the industry expert be a Big N auditor. The results of our tests are qualitatively similar using this alternative measure.

[11] Alternatively, Reichelt and Wang (2010) provide evidence that audit quality is systematically associated with joint national and city-specific auditor industry expertise. They use two definitions of industry specialist. The first definition requires an audit firm to have the largest market share of audit fees in an industry (two-digit SIC) by at least 10 percent. The second definition requires an audit firm to have a significant market share in an industry (30 percent of the market for national expertise and 50 percent of the market for city-specific expertise). Our results are qualitatively similar based on the second definition, but are not robust based on the first definition.

Our empirical model for suspect accruals uses the following logistic regression:

$$\begin{aligned} SUSPECT\_ACCRUALS_{i,t} = {} & \alpha + \beta_1 HAQ_{i,t} + \beta_2 Forecast\ Dispersion_{i,t} + \beta_3 Cash_{i,t-1} \\ & + \beta_4 CAPEX_{i,t-4,t-1} + \beta_5 Dividend\ Yield_{i,t-1} + \beta_6 Debt_{i,t-1} \\ & + \beta_7 Size_{i,t-1} + \beta_8 Size^2_{i,t-1} + \beta_9 Size^3_{i,t-1} + \beta_{10} ROA_{i,t} + \beta_{11} MTB_{i,t} \\ & + e_{i,t}. \end{aligned} \tag{2}$$

Suspect accruals ($SUSPECT\_ACCRUALS_{i,t} = 1$) are modeled similar to suspect repurchases, except that we exclude prior stock repurchases as control variables. Once again, $\beta_1(HAQ_{i,t} = 1)$ captures the relation between high audit quality and the likelihood that discretionary accruals are used to manage EPS. A negative and significant estimate for β_1 is consistent with a negative association between high audit quality and EPS management through discretionary accruals.

Sample Selection

We derive our sample from quarterly financial statement data obtained from the Compustat Xpressfeed North America database for the 22-year period from 1988 to 2009. Our analysis is limited to the post-SFAS 95 period since we require information from cash flow statements to identify stock repurchases and to estimate discretionary accruals. We use lagged data to estimate our models, which limits our sample period to 1989–2009. We eliminate financial institutions, transportation companies, and utilities because regulatory restrictions are likely to constrain their ability to repurchase stock (Bens et al. 2003). In addition, quarterly EPS forecast data must be available in the unadjusted consensus International Brokers' Estimate System (I/B/E/S) file. We require at least two analysts' forecasts for each firm-quarter to construct our forecast dispersion measure. We also eliminate firm-quarters reporting non-positive EPS since accretive stock repurchases do not increase EPS for firms reporting zero income or losses. Finally, we remove firm-quarters with repurchases that are more than 20 percent of common shares outstanding. These are unlikely to be open-market repurchases (Hribar et al. 2006) or repurchases associated with EPS management.

We further refine the sample to more directly address H1 and H2. For our first two main tests, we individually compare suspect repurchases and suspect accrual firm-quarters to firm-quarters that are able to meet or beat consensus analysts' forecasts absent an accretive stock repurchase or discretionary accruals. Therefore, we limit observations to firm-quarters where $SUSPECT\_REPURCHASE1_{i,t} = 1$, $SUSPECT\_ACCRUALS_{i,t} = 1$, or $ACTUAL\_FE$ is $\geq$ \$0.00, $\leq$ \$0.05 absent stock repurchases or discretionary accruals. This reduces the sample to 23,290 observations, of which 1,894 are suspect repurchase firm-quarters and 3,232 are suspect accrual firm-quarters.

Our third test directly compares suspect repurchase to suspect accrual firm-quarters. This analysis includes only firm-quarters where $SUSPECT\_REPURCHASE1_{i,t} = 1$ or $SUSPECT\_ACCRUALS_{i,t} = 1$. The final sample size for this test equals 4,987 total firm-quarters, of which 1,894 are suspect repurchase firm-quarters. The final sample includes 139 firm-quarters designated as both $SUSPECT\_REPURCHASE1_{i,t} = 1$ and $SUSPECT\_ACCRUALS_{i,t} = 1$. Our results are qualitatively similar if we eliminate these firm-quarters from the sample.

IV. RESULTS

Descriptive Statistics

Table 1 presents descriptive statistics for the three samples relevant to our tests. Column 1 presents descriptive statistics for suspect repurchase firm-quarters ($SUSPECT\_REPURCHASE1_{i,t} = 1$), column 2 presents suspect accrual firm-quarters ($SUSPECT\_ACCRUALS_{i,t} = 1$), and column 3

TABLE 1

Descriptive Statistics by Firm-Quarter Type

	$SUSPECT\_REPURCHASE1_{i,t} = 1$ n = 1,894		$SUSPECT\_ACCRUALS_{i,t} = 1$ n = 3,232		$ACTUAL\_FE_{i,t} = [\$0.00,\$0.05]$ (Non-Suspect Control Group) n = 18,303	
	Mean	**Median**	**Mean**	**Median**	**Mean**	**Median**
$HAQ_{i,t}$	0.28	0.00	0.22***	0.00	0.24***	0.00
$REPURCHASE_{i,t-1}$	0.58	1.00	0.25***	0.00	0.26***	0.00
$REPURCHASE_{i,t-2}$	0.56	1.00	0.25***	0.00	0.27***	0.00
$Forecast\ Dispersion_{i,t}$	0.02	0.01	0.01***	0.01	0.02	0.01
$Cash_{i,t-1}$	0.12	0.06	0.22***	0.16	0.17***	0.08
$CAPEX_{i,t-1,t-4}$	0.01	0.01	0.02***	0.01	0.02***	0.01
$Dividend\ Yield_{i,t-1}$	1.63	0.12	0.57***	0.00	0.66***	0.00
$Debt_{i,t-1}$	0.21	0.20	0.14***	0.08	0.19***	0.15
$Size_{i,t-1}$	6,709	1,716	2,006***	279	2,609***	468
$ROA_{i,t}$	0.03	0.02	0.03***	0.02	0.02***	0.02
$MTB_{i,t}$	4.34	3.17	4.10***	3.04	3.63***	2.66

*** Denotes the difference between suspect repurchase firm-quarters and either suspect discretionary accruals or the non-suspect control group are significant at less than 1 percent.

Variable Definitions:

$SUSPECT\_REPURCHASE1_{i,t} = 1$ where $ASIF\_FE1_{i,t}$ is $\geq -\$0.05$, but $< \$0.00$ *and* $ACTUAL\_FE_{i,t}$ is $\geq \$0.00$ but $\leq \$0.05$ for a firm-quarter, otherwise 0;

$SUSPECT\_ACCRUALS_{i,t} = 1$ where $ASIF\_FE\_ACCRUALS_{i,t} \geq -\$0.05$, but $< \$0.00$ *and* $ACTUAL\_FE_{i,t}$ is $\geq \$0.00$ but $\leq \$0.05$ for a firm-quarter, otherwise 0;

$HAQ_{i,t} = 1$ where the auditor is identified as the industry specialist, otherwise 0;

$REPURCHASE_{i,t-1(t-2)} = 1$ where a firm was a net repurchaser in the previous quarter ($t-1$) or two quarters prior ($t-2$), otherwise 0;

$Forecast\ Dispersion_{i,t}$ = standard deviation of analysts' forecasts that comprise the consensus analysts' forecasts. Firm-quarters with only one analyst forecast are removed from the test samples;

$Cash_{i,t-1}$ = cash and cash equivalents (CHEQ) at the beginning of the quarter scaled by total assets (ATQ);

$CAPEX_{i,t-1,t-4}$ = capital expenditures during the previous year ending at the beginning of the quarter scaled by total assets (ATQ);

$Dividend\ Yield_{i,t-1}$ = dividend payment (DIVY) during the previous year ending at the beginning of the quarter scaled by quarter end price (PRCCQ);

$Debt_{i,t-1}$ = total debt (DLTTQ + DLCQ) at the beginning of the quarter scaled by total assets (ATQ);

$Size_{i,t-1}$ = total assets (ATQ) at the beginning of the quarter;

$ROA_{i,t}$ = earnings before extraordinary items (IBQ) scaled by total assets (ATQ); and

$MTB_{i,t}$ = end-of-quarter market value of equity (CSHOQ * PRCCQ) scaled book value of equity (ATQ – LTQ – PSTKQ).

presents non-suspect control firm-quarters that, *ex post*, meet or beat consensus analysts' forecasts without accretive stock repurchases or discretionary accruals.

We find that 28 percent of suspect repurchase firm-quarters employ industry-specialist auditors (*HAQ*) versus 22 percent of suspect accrual firm-quarters and 24 percent of non-suspect control firm-quarters. These differences are statistically significant at the 0.01 level based on a t-test of differences in means.

We also find that 58 percent of suspect repurchase firm-quarters consummate a repurchase in the previous quarter ($REPURCHASE_{i,t-1} = 1$) as compared to 25 and 26 percent in the two comparison firm-quarters. Similar differences are present for repurchase behavior two quarters prior ($REPURCHASE_{i,t-2} = 1$). Combined, these findings indicate that suspect repurchase firm-quarters are approximately twice as likely to repurchase stock in previous quarters when compared to other firm-quarters in our sample.

For all other control variables, differences between suspect repurchase and suspect accrual firm-quarters are statistically significant at the 0.01 level. Similarly, all differences for the control variables between suspect repurchase firm-quarters and non-suspect control firm-quarters are statistically significant at the 0.01 level except for forecast dispersion. Most differences for the control variables between suspect discretionary accrual and the non-suspect control firm-quarters are also statistically significant, with the exception of previous quarter repurchases and capital expenditures. Combined, the significance of these differences suggests the need for multivariate testing.

Multivariate Analysis

Table 2 presents the results for the multivariate tests addressing H1. In Model 1, the coefficient on *HAQ* (β_1) is positive and significant, which supports our hypothesis that managers of firms with higher audit quality are more likely to use accretive stock repurchases to meet or beat analysts' forecasts. The marginal effect for *HAQ* indicates that high audit quality firm-quarters are 17 percent[12] more likely to make an accretive repurchase in comparison to firm-quarters that are able to meet or beat analysts' forecasts without an accretive repurchase *ex post*. These results are consistent with rejecting H1.

Table 2 also presents results for H2. In Model 2, the coefficient for *HAQ* (β_1) is negative and significant, providing evidence that firm-quarters with high audit quality are less likely to use discretionary current accruals to meet or beat analysts' forecasts. The marginal effect for *HAQ* suggests that firm-quarters with high audit quality are 11 percent less likely to use discretionary current accruals in comparison to firm-quarters that meet or beat analysts' forecasts without discretionary current accruals *ex post*.

In Model 3, we directly compare suspect repurchase firm-quarters with suspect accrual firm-quarters by including only observations for which $SUSPECT\_REPURCHASE1_{i,t} = 1$ or $SUSPECT\_ACCRUALS_{i,t} = 1$. The coefficient for *HAQ* (β_1) is positive and significant, which, along with results from Models 1 and 2, supports our hypothesis that firms with higher audit quality are more likely to use accretive stock repurchases and less likely to use discretionary current accruals to meet or beat analysts' forecasts. The marginal effect for *HAQ* indicates that firm-quarters with high audit quality are 23 percent more likely to use accretive stock repurchases than discretionary current accruals to meet or beat analysts' forecasts. The evidence presented in Table 2 is consistent with rejecting H2.

In addition to the results for high audit quality, most control variables are statistically significant in all three models. Prior repurchases are very strongly associated with suspect repurchases. As noted in prior research (Hribar et al. 2006), stock repurchases are serially correlated, which may confound our identification of suspect repurchase firm-quarters. We further address this concern through supplementary tests that explicitly control for expected repurchases.

Both suspect repurchase firm-quarters (Model 1) and suspect accrual firm-quarters (Model 2) are negatively associated with forecast dispersion, indicating that as the variance of analysts' forecasts increases, accretive repurchases and discretionary accruals are less likely to be used to

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[12] We apply an odds ratio to evaluate the marginal effect of $HAQ_{i,t} = 1$ for all models. Since the variable of interest is dichotomous, the marginal effect is calculated as exp(coefficient estimate). Using a coefficient estimate = 0.16 as an example, exp(0.16) = 1.174 results in an odds ratio of 1.174/1.00. Interpreting this, firm-quarters engaging industry-specialist auditors (i.e., $HAQ_{i,t} = 1$) are 17 percent more likely to be identified as suspect repurchase firm-quarters.

TABLE 2

Multivariate Results for the Association between High Audit Quality and Suspect Repurchases and Suspect Accruals

	Model 1		Model 2		Model 3	
	Estimate	p-value	Estimate	p-value	Estimate	p-value
Intercept	−3.52***	< 0.001	−1.72***	< 0.001	−1.70***	< 0.001
$HAQ_{i,t}$	0.16***	0.004	−0.10**	0.026	0.21***	0.010
$REPURCHASE_{i,t-1}$	0.84***	< 0.001			0.92***	< 0.001
$REPURCHASE_{i,t-2}$	0.65***	< 0.001			0.73***	< 0.001
$Forecast\ Dispersion_{i,t}$	−3.53**	0.015	−8.59***	< 0.001	1.49	0.481
$Cash_{i,t-1}$	−1.92***	< 0.001	0.60***	< 0.001	−2.20***	< 0.001
$CAPEX_{i,t-1,t-4}$	−12.12***	< 0.001	2.61**	0.019	−13.71***	< 0.001
$Dividend\ Yield_{i,t-1}$	−0.04***	< 0.001	0.04***	0.003	−0.09***	< 0.001
$Debt_{i,t-1}$	0.37**	0.024	−0.69***	< 0.001	1.05***	< 0.001
$Size_{i,t-1}$	0.00***	< 0.001	0.00***	< 0.001	0.00***	< 0.001
$Size^{2}_{i,t-1}$	0.00***	< 0.001	0.00***	< 0.001	0.00***	< 0.001
$Size^{3}_{i,t-1}$	0.00***	< 0.001	0.00***	< 0.001	0.00***	< 0.001
$ROA_{i,t}$	22.79***	< 0.001	1.90	0.141	22.22***	< 0.001
$MTB_{i,t}$	0.02**	0.014	0.01*	0.054	−0.01	0.214
Likelihood Ratio	1,645.9***	< 0.001	373.22***	< 0.001	1,390.6***	< 0.001
Total Observations	23,290		23,290		4,987	
Observations where:						
$SUSPECT\_REPURCHASE1_{i,t} = 1$	1,894		—		1,894	
$SUSPECT\_ACCRUALS_{i,t} = 1$	—		3,232		—	

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*,**,*** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

The likelihood ratio test statistic indicates the overall significance of the model and has a Chi-square distribution. All models are significant at less than 0.01. All variables are defined in Table 1. Results reflect the following logistic regressions:

Models 1 and 3:

$$\begin{aligned} SUSPECT\_REPURCHASE1_{i,t} = {} & \alpha + \beta_1 HAQ_{i,t} + \beta_2 Repurchase_{i,t-1} + \beta_3 Repurchase_{i,t-2} \\ & + \beta_4 Forecast\ Dispersion_{i,t} + \beta_5 Cash_{i,t-1} + \beta_6 CAPEX_{i,t-4,t-1} \\ & + \beta_7 Dividend\ Yield_{i,t-1} + \beta_8 Debt_{i,t-1} + \beta_9 Size_{i,t-1} + \beta_{10} Size^2_{i,t-1} + \beta_{11} Size^3_{i,t-1} \\ & + \beta_{12} ROA_{i,t} + \beta_{13} MTB_{i,t} + e_{i,t}. \end{aligned}$$

Model 2:

$$\begin{aligned} SUSPECT\_ACCRUALS_{i,t} = {} & \alpha + \beta_1 HAQ_{i,t} + \beta_2 Forecast\ Dispersion_{i,t} + \beta_3 Cash_{i,t-1} + \beta_4 CAPEX_{i,t-1} \\ & + \beta_5 Dividend\ Yield_{i,t-1} + \beta_6 Debt_{i,t-1} + \beta_7 Size_{i,t-1} + \beta_8 Size^2_{i,t-1} + \beta_9 Size^3_{i,t-1} \\ & + \beta_{10} ROA_{i,t} + \beta_{11} MTB_{i,t} + e_{i,t}. \end{aligned}$$

Each model sample reflects the following restrictions:

Model 1: Firm-quarters that meet or beat the consensus analysts' forecasts ($\$0.00 \leq ACTUAL_{FEi,t} \leq \$0.05$). The sample compares firm-quarters suspected of using accretive stock repurchases to meet or beat consensus analysts' forecasts to those that that meet or beat without accretive stock repurchases.

Model 2: Firm-quarters that meet or beat the consensus analysts' forecasts ($\$0.00 \leq ACTUAL_{FEi,t} \leq \$0.05$). The sample compares firm-quarters suspected of using discretionary accruals to meet or beat consensus analysts' forecasts to those that meet or beat without discretionary accruals.

Model 3: Firm-quarters identified as $SUSPECT\_REPURCHASE1_{i,t} = 1$, or $SUSPECT\_ACCRUALS_{i,t} = 1$. The sample compares firm-quarters suspected of using accretive stock repurchases to meet or beat consensus analysts' forecasts to firm-quarters suspected of using accruals to meet or beat consensus analysts' forecasts.

meet the consensus analysts' forecasts. In addition, both suspect repurchase and suspect accrual firm-quarters are positively associated with ROA, although ROA is much higher for suspect repurchase firm-quarters. The signs of cash, capital expenditures, dividend yield, and debt switch between Models 1 and 2, indicating that suspect repurchase firm-quarters exhibit lower levels of each, while suspect accrual firm-quarters exhibit higher levels of each when compared to non-suspect firm-quarters. Together, these findings suggest that firm characteristics are likely to affect firm choices between earnings management types, but also indicate possible underlying firm differences. We address this concern through supplementary tests that apply propensity score matching based on auditor industry specialization to better control for firm differences.

Supplementary Tests

Controlling for Expected Repurchases

Our main tests estimate pre-repurchase forecast error (i.e., *ASIF_FE1*) assuming that firms' open-market repurchases are not anticipated by analysts and investors. If firms previously announce open-market repurchase programs or if analysts anticipate the serial correlation in stock repurchase behavior, then our approach likely understates analysts' and investors' repurchase expectations. Our main tests control for this by including prior stock repurchases in the suspect repurchase models. However, to further address this concern, we next directly estimate expected repurchases for use in the suspect repurchase models (see Hribar et al. 2006, 18–19).

We follow Hribar et al. (2006) and estimate expected repurchases by a firm in a given quarter using a two-stage approach that models expected repurchases as the product of the estimated probability that firms will repurchase stock (stage 1) and the expected level of repurchases given that one occurs (stage 2). The first-stage estimation uses both repurchase and non-repurchase firms to estimate a repurchase expectation for every firm-quarter in the sample. The first stage models the repurchase decision as a binary variable, and estimates the following probit model:

$$\begin{aligned} REPURCHASE_{i,t} = {} & \alpha + \beta_1 Repurchase_{i,t-1} + \beta_2 Repurchase_{i,t-2} + \beta_3 Cash_{i,t-1} \\ & + \beta_4 CAPEX_{i,t-4,t-1} + \beta_5 Dividend\,Yield_{i,t-1} + \beta_6 Debt_{i,t-1} + \beta_7 Size_{i,t-1} \\ & + \gamma_k Industry_{i,k} + \delta_j Year_{i,j} + \varphi_q Qtr_{i,q} + e_{i,t}, \end{aligned} \tag{3}$$

where $REPURCHASE_{i,t}$ equals 1 if firm i repurchased shares during quarter t, and 0 otherwise. Independent variables are measured at the beginning of the quarter when expectations are more likely formed about potential stock repurchases. Most control variables are the same as those used in the suspect repurchases model. $Industry_{i,k}$, $Year_{i,j}$, and $Qtr_{i,q}$ are included as fixed effects.

Untabulated results for the first-stage regression reveal that the probability of a repurchase is positive and significantly associated with prior repurchases, dividend yield, and size, and negative and significantly associated with cash, capital expenditures, and debt. The pseudo-R^2 from the first-stage analysis is 27.7 percent.

The second-stage model provides an estimate for dollars spent on stock repurchases based on firm characteristics conditioned on the probability that a repurchase occurs. The second-stage regression is similar to the first-stage, except that the actual dollar value of stock repurchases replaces the binary repurchase variables from the first stage and information about the probability of a repurchase is incorporated into the model. Information about the probability of a repurchase for all firms, not just repurchasing firms, is incorporated in the second-stage regression through two variables calculated from the first stage, the standard normal probability density function (φ) and the cumulative density function (Φ). The variable φ is included in the second-stage regression, while Φ is multiplied by each of the independent variables from the first stage. Untabulated results for the second-stage regression show that all variables except capital expenditures are significantly related in the expected direction to the dollar value of stock repurchases. The adjusted R^2 in the second stage is 57.1 percent.

The second-stage estimates generate an unconditional expectation of the dollar amount of repurchases for firm-quarters based on information known at the beginning of each quarter. The expected number of shares repurchased is calculated by dividing the estimated dollar amount of shares repurchased by the beginning of the quarter stock price. From this estimate of the expected number of shares repurchased, we create a second "as-if" EPS estimate that incorporates expected stock repurchase activity:

$$ASIF\_EPS2_{i,t} = \frac{(NI_{i,t} + C_{i,t})}{\left[SHARESOUT_{i,t-1} + (0.5*SHARESISSUED_{i,t}) - \left(0.5*E(REPURCHASE_{i,t})\right)\right]},$$

where $E(REPURCHASE_{i,t})$ is the expectation of shares to be repurchased at the beginning of the quarter. *ASIF_EPS2* is similar to *ASIF_EPS1*, except that it removes the EPS effects of expected repurchases. The difference between reported EPS and *ASIF_EPS1* includes the EPS effects of both expected and unexpected repurchases, whereas the difference between reported EPS and *ASIF_EPS2* only reflects the EPS effects of unexpected repurchases.

To obtain the pre-repurchase forecast error based on unexpected repurchases, we isolate the impact of unexpected stock repurchases on EPS (*EPSImpact2*) by comparing *ASIF_EPS2* and reported EPS, where:

$$EPSImpact2_{i,t} = ReportedEPS_{i,t} - ASIF\_EPS2_{i,t}.$$

EPSImpact2 is positive for accretive stock repurchases and negative for non-accretive stock repurchases.

The pre-repurchase forecast error based on unexpected repurchases is calculated as the actual forecast error less the EPS effect of unexpected repurchases. Specifically:

$$ASIF\_FE2_{i,t} = ACTUAL\_FE_{i,t} - EPSImpact2_{i,t}.$$

ASIF_FE2 is negative for firm-quarters that would have missed consensus analysts' forecasts absent the effects of accretive stock repurchases. For firms that do not repurchase stock, *ASIF_FE2* equals *ACTUAL_FE*.

Finally, we define suspect repurchases as those that would have missed analysts' forecasts by five cents or less, but are able to meet or beat forecasts by no more than five cents with unexpected stock repurchases. We construct an indicator variable ($SUSPECT\_REPURCHASE2_{i,t} = 1$) as an alternative measure for suspect repurchases based on unexpected repurchases, where *ASIF_FE2* is $\geq -\$0.05$ and $< \$0.00$ *and ACTUAL_FE* is $\geq \$0.00$ and $\leq \$0.05$ for a firm-quarter. *SUSPECT_REPURCHASE2* is equal to 0 for all other firm-quarters.

Table 3 replicates our main tests for H1 and H2, but substitutes *SUSPECT_REPURCHASE2* for *SUSPECT_REPURCHASE1* to control for expected repurchases. The results from these supplementary tests are consistent with our main tests. In Model 4, the coefficient on *HAQ* (β_1) is positive and significant, which provides support that managers of firms with higher audit quality are more likely to use accretive stock repurchases to meet or beat analysts' forecasts. In Model 5, the coefficient for *HAQ* (β_1) is negative and significant, providing evidence that firm-quarters with high audit quality are less likely to use discretionary current accruals to meet or beat analysts' forecasts. Finally, in Model 6, the coefficient for *HAQ* (β_1) is positive and significant, which supports our hypothesis that firms with higher audit quality are more likely to use accretive stock repurchases and less likely to use discretionary current accruals to meet or beat analysts' forecasts.

TABLE 3

Multivariate Results for the Association between High Audit Quality and Suspect Repurchases and Suspect Accruals after Controlling for Expected Repurchases

	Model 4		Model 5		Model 6	
	Estimate	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**
Intercept	−4.53***	< 0.001	−1.61***	< 0.001	−2.70***	< 0.001
$HAQ_{i,t}$	0.27***	0.000	−0.12**	0.012	0.30***	0.003
$REPURCHASE_{i,t-1}$	0.65***	< 0.001			0.69***	< 0.001
$REPURCHASE_{i,t-2}$	0.65***	< 0.001			0.70***	< 0.001
$Forecast\ Dispersion_{i,t}$	−2.12	0.275	−8.38***	< 0.001	0.73	0.778
$Cash_{i,t-1}$	−1.33***	< 0.001	0.52***	< 0.001	−1.47***	< 0.001
$CAPEX_{i,t-1,t-4}$	−11.76***	< 0.001	1.77	0.116	−12.35***	0.000
$Dividend\ Yield_{i,t-1}$	−0.03***	0.005	0.04***	0.004	−0.08***	< 0.001
$Debt_{i,t-1}$	−0.47*	0.057	−0.68***	< 0.001	−0.22	0.490
$Size_{i,t-1}$	0.00***	< 0.001	0.00***	< 0.001	0.00***	< 0.001
$Size^{2}_{i,t-1}$	0.00***	< 0.001	0.00***	< 0.001	0.00***	< 0.001
$Size^{3}_{i,t-1}$	0.00***	< 0.001	0.00***	< 0.001	0.00***	< 0.001
$ROA_{i,t}$	24.98***	< 0.001	2.39*	0.068	23.45***	< 0.001
$MTB_{i,t}$	0.05***	< 0.001	0.01	0.233	0.02*	0.099
Likelihood Ratio	1,197.4***	< 0.001	337.83***	< 0.001	1,052.3***	< 0.001
Total Observations	21,310		21,310		4,035	
Observations where:						
$SUSPECT\_REPURCHASE2_{i,t} = 1$	936		—		936	
$SUSPECT\_ACCRUALS_{i,t} = 1$	—		3,172		—	

*,**,*** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

The likelihood ratio test statistic indicates the overall significance of the model and has a Chi-square distribution. All models are significant at less than 0.01. All variables are as defined in Table 1, except *SUSPECT_REPURCHASE2*, which is equal to 1 where $ASIF\_FE2_{i,t}$ is ≥ −\$0.05, but < \$0.00 *and* $ACTUAL\_FE_{i,t}$ is ≥ \$0.00, but ≤ \$0.05 for a firm-quarter, otherwise 0.

Models 4–6 mirror Table 2, Models 1–3. Each model sample reflects the same restrictions identified in Table 2 except the restrictions to *SUSPECT_REPURCHASE2*.

Propensity Score Matching

Our main tests use control variables to reduce the effects of firm characteristics on the association between suspect repurchases, suspect accruals, and high audit quality. However, the results from our main tests may still be influenced by fundamental differences in firm characteristics. Prior research (Lawrence et al. 2011; Armstrong et al. 2010) finds that the regression framework is subject to misspecification of the functional relationship between control variables and the dependent variable for different treatment levels and recommends using propensity score matching to address this concern. In our study, size is of particular concern since industry-specialist auditors are identified based on market share. Therefore, we employ propensity score matching to match firms with industry-specialist auditors to firms without industry-specialist auditors based on relevant observable firm characteristics.

First, following Gul et al. (2009), we estimate propensity scores using a logistic regression in which the dependent variable is either an industry-specialist auditor (1) or not an industry-specialist auditor (0). We model the choice of an industry-specialist auditor based on size, debt, return on assets, market-to-book, asset turnover, litigation risk, lagged absolute value of discretionary

TABLE 4

Propensity Score Matching Analysis of the Association between High Audit Quality and Suspect Repurchases and Suspect Accruals

	HAQ Mean n = 5,153	Non-HAQ Mean n = 5,153	Difference in Means	p-value
$SUSPECT\_REPURCHASE1_{i,t}$	0.063	0.055	0.008*	0.095
$SUSPECT\_REPURCHASE2_{i,t}$	0.038	0.028	0.010**	0.012
$SUSPECT\_ACCRUALS_{i,t}$	0.115	0.133	−0.018***	0.007
$Size_{i,t-1}$	3,659.8	3,695.5	−35.7	0.863
$Size^2_{i,t-1}$	7,000.0[a]	7,100.0[a]	−100.0[a]	0.909
$Size^3_{i,t-1}$	2,300.0[a]	2,300.0[a]	0.000	0.927
$Debt_{i,t-1}$	0.190	0.191	−0.001	0.887
$ROA_{i,t}$	0.022	0.022	0.000	0.374
$MTB_{i,t}$	3.934	3.977	−0.043	0.541
$Asset\ Turnover_{i,t}$	0.297	0.296	0.001	0.670
$Litigation_{i,t}$	0.036	0.035	0.001	0.791
$Abs.\ DACC_{i,t-1}$	0.016	0.016	0.000	0.967
$Age_{i,t}$	2.684	2.700	−0.016	0.319

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*,**,*** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed t-tests.

The table presents means and differences in means for firms with *HAQ* and propensity score matched firms without *HAQ*. Using the full sample from the main tests in Table 2, we match *HAQ* firms, without replacement, to non-*HAQ* firms with the closest propensity score and within a maximum distance of 0.03.

[a] $Size^2$ is scaled by one million and $Size^3$ is scaled by one billion.

All other variables are defined in Table 1.

Variable Definitions:

Asset Turnover = quarterly sales divided by total assets;

Litigation = 1 for firms with the following SIC codes: (2833–2836), (3570–3577), (3600–3674), (5200–5961), or (7370–7370), otherwise 0;

Abs. DACC = previous quarter's absolute value of discretionary accruals; and

Age = log of the number of years a firm has been on Compustat beginning with 1950.

accruals, age, industry, and year. Next we match, without replacement, an industry-specialist firm to the non-industry-specialist firm with the closest propensity score within a maximum distance of 0.03. This results in 5,153 matched pairs.

Table 4 reports tests of differences in means of observable firm characteristics between the *HAQ* and matched non-*HAQ* firms for suspect repurchases and suspect accruals. The two-tailed t-tests of differences in means indicate that none of the observable firm characteristics are significantly different between the sample and control firms, suggesting that the firms are matched appropriately. As described next, we find that firms with industry-specialist auditors are more likely to use accretive repurchases to meet or beat analysts' forecasts than firms with non-industry-specialist auditors. The difference between *HAQ* and matched non-*HAQ* firms for *SUSPECT_REPURCHASE1* (*SUSPECT_REPURCHASE2*) indicates that firms with industry-specialist auditors execute 14.5 percent (35.7 percent) more suspect repurchases than firms without industry-specialist auditors. The p-values for the difference in means between *HAQ* and matched non-*HAQ* firms for *SUSPECT_REPURCHASE1* and *SUSPECT_REPURCHASE2* are 0.095 and 0.012, respectively. Consistent with a trade-off between accretive repurchases and accrual-based earnings management, we find that firms with industry-specialist auditors use 13.5 percent less suspect accruals to meet or beat analysts' forecasts than the same matched non-industry-specialist auditor firms. The p-value of the difference in means for *SUSPECT_ACCRUALS* is 0.007. The evidence from propensity score matching supports our main results and helps alleviate concerns that differences in observable firm characteristics are driving our results.

V. SENSITIVITY TESTS

One concern is that our main tests could be capturing a spurious relation between high audit quality and accretive stock repurchases and discretionary accruals. We address this issue in two ways. First, we test whether there is a relation between accretive stock repurchases and discretionary current accruals absent incentives to meet or beat analysts' forecasts. We identify both accretive stock repurchase firm-quarters and EPS increasing discretionary current accruals not classified as suspect in our main tests. In other words, we test whether our hypothesized trade-off persists for non-suspect repurchase and non-suspect accrual firm-quarters. Untabulated results show that high audit quality is not significantly related to non-suspect repurchase or accrual firm-quarters, providing evidence that our main tests are not capturing a general relation.

Second, we adopt an approach similar to Ayers et al. (2006) by examining whether a trade-off between suspect repurchases and suspect accruals exists where the change in EPS is the difference between meeting or beating *pseudo* earnings targets set at five cents above or below analysts' forecasts. If the results from our main tests are unrelated to analysts' forecasts, we would expect the results around *pseudo* targets to be similar to the results occurring around analysts' forecasts. Unlike the results from our main tests, untabulated results for high audit quality are insignificant using *pseudo* targets, providing further evidence that efforts to meet or beat analysts' forecasts are an important aspect of our study.

A second concern stems from the fact that auditor scrutiny is likely more rigorous during the fourth quarter (Brown and Pinello 2007) and, therefore, fourth quarter observations could be driving our main results. However, audit quality is likely to affect the first three quarters for at least two reasons. First, technological advancements in auditing reduce the seasonality of auditing by facilitating timely interim reviews and reducing year-end workloads (Shu 2000). Second, managers' accrual choices during the first three quarters are likely affected by expectations of higher audit scrutiny at year-end. We ensure that our main results are not driven by observations from the fourth quarter by re-estimating our main tests using only the first three fiscal quarters during which auditor oversight may be less stringent. Results are qualitatively similar for the first three quarters, which provides evidence that high audit quality is associated with our suggested trade-off even in quarters with less stringent auditor scrutiny.

VI. IMPLICATIONS AND CONCLUSION

We examine whether audit quality affects managers' choices between accrual-based earnings management and accretive stock repurchases. We provide consistent evidence that firms that engage high-quality auditors are more likely to use accretive stock repurchases and less likely to manage accruals to meet or beat consensus analysts' forecasts. Combined, our findings suggest that managers faced with high audit quality trade off the use of accretive stock repurchases with accrual-based earnings management to meet or beat analysts' forecasts.

Our findings have at least three implications for corporate governance. First, our findings provide evidence that the decision to hire high audit quality to combat accrual-based earnings management is associated with an unintended consequence: the increased use by managers of at least one real earnings management mechanism—accretive stock repurchases. Firms choose high audit quality to signal earnings quality and corporate governance effectiveness. This signal becomes obfuscated when the use of real earnings management is elevated.

Second, if managers circumvent high audit quality through the use of this real earnings management technique, then other governance mechanisms, especially those limiting management's ability to use real earnings management to influence firm performance measures, might be considered concurrently with the decision to engage a high-quality auditor. Corporate boards and audit committees may wish to consider compensation-related issues and other contractual features for managers that constrain firm value-decreasing behaviors of management.

Finally, the use of accretive repurchases to meet or beat EPS benchmarks potentially deviates from "business as usual," which may have negative consequences to the short- and long-term performance of the firm. Often, accretive stock repurchases involve a substantial number of shares and firms face significant opportunity costs from the loss of resources used in accretive stock repurchases. This is especially problematic when repurchases alter normal business practices. Although not formally tested in this paper, accretive repurchases used as a real earnings management tool could potentially have unfavorable implications for a firm's current and future operating performance and could impose unnecessary costs on the firm and its shareholders.[13]

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[13] Cohen and Zarowin (2010) find that firms that engage in real earnings management prior to a seasoned equity offering suffer significant performance declines after the offering period. Conversely, Gunny (2010) provides evidence that real earnings management to meet earnings benchmarks signals positive future performance.

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APPENDIX A
VARIABLE DESCRIPTIONS

$Abs.\ DACC_{i,t-1}$ = absolute value of a firm's previous quarter discretionary accruals;

$ACTUAL\_FE_{i,t}$ = firm's reported EPS minus its final consensus analysts' forecast;

$Age_{i,t}$ = log of the number of years a firm has been on Compustat, beginning in 1950;

$ASIF\_EPS1_{i,t}$ = firm's estimated EPS absent accretive stock repurchases calculated as:

$$\frac{NI_{i,t} + C_{i,t}}{SHARESOUT_{i,t-1} + 0.5*SHARESISSUED_{i,t}};$$

$ASIF\_EPS2_{i,t}$ = firm's estimated EPS absent accretive stock repurchases that incorporates expected repurchases, calculated as:

$$\frac{(NI_{i,t} + C_{i,t})}{\left[SHARESOUT_{i,t-1} + (0.5*SHARESISSUED_{i,t}) - \left(0.5*E(REPURCHASE_{i,t})\right)\right]};$$

$ASIF\_EPS\_ACCRUALS_{i,t}$ = firm's estimated EPS absent current discretionary accruals, net of income taxes, calculated as:

$$\frac{NI_{i,t} - DACC_{i,t}(1 - T_{i,t})}{Weighted\ Average\ SHARESOUT_{i,t}};$$

$ASIF\_FE1_{i,t}$ = firm's estimated pre-repurchase forecast error, calculated as: $ACTUAL\_FE_{i,t} - EPSImpact1_{i,t}$;

$ASIF\_FE2_{i,t}$ = firm's estimated pre-repurchase forecast error that incorporates expected repurchases, calculated as: $ACTUAL\_FE_{i,t} - EPSImpact2_{i,t}$;

$ASIF\_FE\_ACCRUALS_{i,t}$ = firm's estimated pre-discretionary current accrual forecast error, calculated as: $ACTUAL\_FE_{i,t} - EPSImpact1\_ACCRUALS_{i,t}$;

$Asset\ Turnover_{i,t}$ = firm's quarterly sales divided by total assets (ATQ);

$C_{i,t}$ = estimated forgone dollar return on cash used for accretive stock repurchases. When a firm has excess cash, the forgone return is estimated as excess cash multiplied by the three-month Treasury bill rate. Excess cash is estimated as cash balances in excess of 6 percent of assets for retail firms and 2 percent of assets for all other firms (Copeland et al. 2000). If excess cash is less than the repurchase dollar amount, then the forgone return is estimated as the firm's average interest rate multiplied by the incremental funds borrowed;

$CAPEX_{i,t-4,t-1}$ = firm's capital expenditures during the previous year ending at the beginning of the quarter, scaled by total assets (ATQ);

$Cash_{i,t-1}$ = firm's cash and cash equivalents (CHEQ) at the beginning of the quarter, scaled by total assets (ATQ);

$Current\ Accruals_{i,t}$ = difference between quarterly income before extraordinary items (IBQ) and quarterly operating cash flows (OANCFY, adjusted to quarterly) less depreciation and amortization expense (DPQ);

$DACC_{i,t}$ = difference between the actual reported current accruals and the estimated current accruals from the current discretionary accrual model rescaled by lagged total assets ($ATQ_{i,t-1}$);

$Debt_{i,t-1}$ = firm's total debt (DLTTQ + DLCQ) at the beginning of the quarter, scaled by total assets (ATQ);

$Dividend\ Yield_{i,t-1}$ = firm's dividend payment during the previous year ending at the beginning of the quarter, scaled by its quarter-end share price (PRCCQ);

$EPSImpact1_{i,t}$ = effect of share repurchases on EPS, calculated as: $ReportedEPS_{i,t} - ASIF\_EPS1_{i,t}$;

$EPSImpact2_{i,t}$ = effect of unexpected share repurchases on EPS, calculated as: $ReportedEPS_{i,t} - ASIF\_EPS2_{i,t}$;

$EPSImpact1\_ACCRUALS_{i,t}$ = effect of discretionary accruals on EPS, calculated as: $ReportedEPS_{i,t} - ASIF\_EPS\_ACCRUALS_{i,t}$;

$E(REPURCHASE_{i,t})$ = expected number of shares to be repurchased at the beginning of the quarter based on the two-stage estimation procedure detailed in the "Controlling for Expected Repurchases" section;

$Forecast\ Dispersion_{i,t}$ = standard deviation of analysts' forecasts that comprise the consensus forecast;

$HAQ_{i,t}$ = 1 where the auditor is identified as the industry specialist, otherwise 0;

$Litigation_{i,t}$ = 1 for firms with the following SIC codes (2833–2836), (3570–3577), (3600–3674), (5200–5961), or (7370), otherwise 0;

$MTB_{i,t}$ = firm's end-of-quarter market value of equity (CSHOQ * PRCCQ), scaled by its book value of equity (ATQ − LTQ − PSTKQ);

$NI_{i,t}$ = firm's reported earnings before extraordinary items available to common shareholders for the firm-quarter (IBCOMQ);

$ReportedEPS_{i,t}$ = firm's earnings per share as reported for the quarter;

$REPURCHASE_{i,t,(t-1),\{t-2\}}$ = 1 where a firm was a net repurchase in the previous quarter (t–1) or two quarters prior (t–2), otherwise 0;

$ROA_{i,t}$ = earnings before extraordinary items (IBQ), scaled by total assets (ATQ);

$SHARESISSUED_{i,t}$ = actual or estimated number of common stock shares issued during a firm-quarter, depending on data availability;

$SHARESOUT_{i,t-1}$ = reported number of diluted common shares outstanding if analysts' forecast diluted EPS, otherwise primary common shares outstanding, at the beginning of the firm-quarter;

$Size_{i,t-1}$ = firm's total assets (ATQ) at the beginning of the quarter;

$SUSPECT\_REPURCHASE1_{i,t}$ = indicator variable indicating whether the firm meets or beats analysts' consensus forecasts using share repurchases, equal to 1 if *ASIF_FE1* is $\geq -\$0.05$, but $< \$0.00$ *and ACTUAL_FE* is $\geq \$0.00$, but $\leq \$0.05$, otherwise 0;

$SUSPECT\_REPURCHASE2_{i,t}$ = indicator variable indicating whether the firm meets or beats analysts' consensus forecasts using unexpected share repurchases, equal to 1 if *ASIF_FE2* is $\geq -\$0.05$ and $< \$0.00$ *and ACTUAL_FE* is $\geq \$0.00$ and $\leq \$0.05$, otherwise 0;

$SUSPECT\_ACCRUALS_{i,t}$ = indicator variable indicating whether the firm meets or beats analysts' consensus forecasts using discretionary accruals, equal to 1 if *ASIF_FE_ACCRUALS* $\geq -\$0.05$, but $< \$0.00$ *and ACTUAL_FE* is $\geq \$0.00$, but $\leq \$0.05$, otherwise 0; and

Weighted Average $SHARESOUT_{i,t}$ = weighted average diluted common shares outstanding if analysts' forecast diluted EPS (CSHFDQ), otherwise primary common shares outstanding (CSHPRQ).

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Reward System Design and Group Creativity: An Experimental Investigation

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ABSTRACT: In an environment where three-person groups develop a creative solution to an important problem, we examine whether the efficacy of either individual- or group-based creativity-contingent incentives depends on whether they take the piece-rate or tournament form. We predict and find that group (intergroup) tournament pay increases group cohesion and collaborative efforts, which ultimately lead to a more creative group solution, relative to group piece-rate pay. While individual (intragroup) tournament pay increases individual efforts, we find that it does not enhance the creativity of group solutions relative to individual piece-rate pay. Our results advance the burgeoning management accounting literature on creativity-contingent incentives by demonstrating that reward systems are more likely to promote group creativity through collaborative efforts rather than independent individual efforts. We also provide important insights into when and why tournament pay can boost group creativity in organizations. In doing so, we contribute to a better understanding of observations from practice suggesting that organizations valuing creativity often induce intergroup competition.

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I. INTRODUCTION

Organizations often look to employee groups to provide a creative solution to an important problem (Nussbaum 2005; Thompson and Choi 2006; Adler and Chen 2011). As such, a challenge for management accountants is designing performance evaluation and reward systems to promote group creativity (Kaplan and Norton 1996, Ch. 5). While a frequently proposed solution to this challenge is to reward either groups for the creativity of their solution or individuals for the creativity of their input to the group solution, prior research suggests creativity-contingent incentives often do not lead to the desired result. For example, consistent with theory suggesting that creativity is a dimension of performance that does not respond well to individuals' independent efforts (Amabile 1996), prior research demonstrates that reward systems that motivate individuals to independently work harder at being creative often do not lead to more creative output (Kachelmeier et al. 2008; Kachelmeier and Williamson 2010). Tying rewards to a group-based measure of creativity can potentially enhance group creativity by increasing collaborative efforts (Toubia 2006). However, prior research highlights that groups often do not capitalize on these opportunities due to obstacles such as evaluation apprehension (i.e., anxieties that fellow group members will evaluate one's ideas unfavorably) and free-riding (which is exacerbated by the considerable cognitive effort required to process, synthesize, and build on the diverse ideas of others) (Paulus 2000; Paulus and Yang 2000).

These conclusions, however, are limited by the fact that prior research often considers only a single form of creativity-contingent incentive—most often piece-rate—where pay for either groups or individuals strictly increases in creativity (Sprinkle 2008). However, tournament pay, which can induce either intergroup or intragroup competition, is quite pervasive in organizations (Lambert et al. 1993; McGregor 2006). Moreover, in environments where a measure of group (individual) creativity is available, the decision of whether to induce intergroup (intragroup) competition or make pay an increasing function of the creativity measure is often an important choice faced by managers. As such, we examine whether moving from the piece-rate to the tournament form of compensation better promotes group creativity across environments in which individuals receive pay based on either (1) the creativity of the group's solution, or (2) the creativity of their individual input to the group's solution.

Drawing on theory from psychology, we develop a process model suggesting that group tournament pay helps overcome obstacles, such as those noted earlier, that would otherwise limit the effectiveness of creativity-contingent incentives relative to group piece-rate pay. When a group feels challenged or threatened by outsiders, such as other groups competing with it for an organization's scarce resources, members feel a greater sense of cohesion or solidarity with one another. Members of cohesive groups feel stronger psychological bonds with each other and believe that other members are more open to their ideas, which enhances the ease with which group members share ideas, thus reducing evaluation apprehension. Moreover, members of cohesive groups feel more united in trying to achieve group objectives and, therefore, work harder toward achieving them, which decreases free-riding (Brewer 1979; Tajfel and Turner 1986; van Knippenberg 2000; Bornstein 2003). Thus, to the extent that intergroup competition enhances cohesion, we expect that group members will not only more freely share their ideas, but also they will exert the cognitive efforts necessary to process, synthesize, and build on each others' ideas, which can ultimately enhance the creativity of a group's solution (Tagger 2002; Van der Vegt and Bunderson 2005).

In addition to testing our process model, which suggests that tournament pay increases the efficacy of group-based creativity-contingent incentives, we further validate our model by examining whether tournament pay increases the efficacy of individual-based creativity-contingent incentives. Relative to individual piece-rate pay, research suggests that intragroup competition sparked by individual tournaments motivates more independent, individual efforts, to the detriment of collaboration (Drake et al. 1999). However, to the extent that (1) research suggesting that one cannot be more creative by simply independently working harder generalizes to a group setting, and (2) reward systems better motivate group creativity by increasing cohesion (as our process model suggests) rather than simply motivating greater effort *per se*, individual tournament pay will likely not increase the creativity of a group's solution relative to individual piece-rate pay. As such, we predict that group tournament pay will increase and individual tournament pay will not increase the efficacy of creativity-contingent incentives relative to group and individual piece-rate pay, respectively.

To test our predictions, we use a laboratory experiment in which three-person groups of undergraduate students developed a creative solution (i.e., a solution that is "original, innovative, and implementable within a reasonable budget") to an assigned campus problem. During the idea generation and development phase, groups utilized a computer program that allowed participants to easily generate new ideas and develop others' and individuals' own ideas. Groups then selected one of the developed ideas to serve as their group solution.

We manipulated two factors between-subjects at two levels each. First, we manipulate the *level of incentives* by tying participant pay to a measure of either the creativity of the group's solution or the creativity of individual input to the group's solution, both as assessed by an independent panel of three raters. Second, we manipulate the *form of incentives* by providing participants with either a linear piece-rate based on their assigned measure or a winner-take-all tournament. The winner-take-all tournament takes the form of either an intergroup tournament, where groups competed against two other groups in their session based on the creativity of the group's solution, or an intragroup tournament, where individuals competed against their other two group members based on the creativity of their individual input to the group solution. Across all four conditions, we held average participant compensation constant.

Our results support our predictions. Group tournament pay leads to more creative group solutions than does group piece-rate pay. We further show that group cohesion encompassing measures regarding group members' psychological bond, openness to each others' ideas, and the extent that group members feel united in achieving group objectives partially mediates the positive creativity effect of group tournament pay relative to group piece-rate pay. Individual tournament pay fails to generate more creative group solutions relative to individual piece-rate pay, either in our initial experiment where we rewarded individuals based solely on the creativity of their inputs to the selected group solution or in a supplemental experiment where we rewarded individuals based on the creativity of their input to all group ideas.

In addition to creativity ratings and self-reported measures, we also explore how our experimental conditions affect within-group interactions while generating initial ideas, developing these ideas, and selecting one of these ideas as the group solution. While groups across all conditions consider a similar number/diversity of initial ideas and exhibit similar propensities to select their most creative idea as the group solution, we observe important differences in how groups develop their ideas. On the one hand, group tournament pay increases the extent to which group members build on each others' thoughts and ideas when developing proposals relative to those in the group piece-rate condition. On the other hand, individual tournament pay increases the extent to which individuals build on their own thoughts and ideas relative to the average of the other conditions, but consistent with prior research, these independent individual efforts do not appear to enhance individual or group creativity.

Our study makes important contributions. First, we show that tournament pay that induces intergroup competition can help overcome obstacles identified by prior research that limit the effectiveness of creativity-contingent incentives. In doing so, we also shed light on the process of idea generation, development, and selection that takes place within groups under various reward systems. We use a unique software program to capture a rich, objective dataset that reflects these within-group processes, and our analysis of this dataset yields important insights into how aspects of these creative processes differ under various incentive schemes. Although research from individual settings often suggests that creativity is invariant to reward system design, our results demonstrate that reward systems can play a vital role in promoting group creativity by encouraging individuals to collaborate and build on the diverse ideas of others.

Second, our study contributes more generally to research investigating the efficacy of tournament pay (e.g., Young et al. 1993; Drake et al. 1999; Hannan et al. 2008). By simultaneously studying how group and individual tournaments affect group creativity as well as illuminating the processes through which intergroup competition promotes higher group performance, we gain a better understanding of when tournament pay can be effective (Luft and Shields 2003). For example, while Drake et al. (1999) suggest that tournament pay does not lead to higher group performance on a collaborative task, they focus solely on individual (intragroup) tournaments.

Our results also contribute to a better understanding of observations from practice. Case and field studies demonstrate that organizations seeking group creativity often induce intergroup competition (Amabile and Gryskiewiecz 1987; Arnold 1998; Simons 2000). We develop and test a theory that can help explain why these practices have evolved in organizations.

We describe the background of our study and develop hypotheses in Section II. Section III explains our experimental method and design, Section IV presents results, and Section V concludes.

II. BACKGROUND, THEORY, AND HYPOTHESES

Background

Creative organizational endeavors, such as developing a proposal to address an important organizational problem, often take place in group settings (Thompson and Choi 2006). In a group setting, creativity can arise not only from the innovativeness of an individual group member, but also through collaborative efforts of multiple group members. The opportunity to observe, synthesize, and build on each others' ideas can make group members see the situation from multiple perspectives and thus increase the creativity of individual inputs to the group (Toubia 2006). Moreover, groups can coordinate their efforts to further develop the most creative individual inputs to transform them into a more creative group solution (Adler and Chen 2011).

A potential technique for promoting group creativity is to tie employee pay to a group-based measure of creativity (Toubia 2006).[1] However, prior research identifies obstacles that potentially limit the effectiveness of group-based incentives. For example, evaluation apprehension can limit individual contributions in group-creativity settings. That is, many individuals are anxious about sharing creative ideas, fearing that others will evaluate them unfavorably (Paulus et al. 2006). Moreover, even to the extent that individuals share their ideas, free-riding can limit the benefits from the diverse ideas of group members (Paulus 2000). That is, individuals under group-based creativity-contingent incentives are typically not willing to exert the significant cognitive efforts required to process, synthesize, and build on the diverse ideas of their group members (Paulus and Yang 2000).

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[1] In our setting, the group's objective is to develop a single creative solution. Throughout the remainder of the paper, we refer to the creativity of this solution as group creativity. We operationalize group creativity in this fashion for two primary reasons. First, given the amount of time we can reasonably keep participants in our laboratory setting, asking them to submit one solution rather than many solutions has a greater chance of encouraging collaboration among members (i.e., building on each other's inputs while developing solutions), which allows us to more powerfully test our theory. Second, because evaluating ideas is costly for management, organizations often require groups to put forth only their best or most creative solution to the problem being addressed (Sutton and Hargadon 1996).

To overcome these concerns, organizations may have the opportunity to measure and reward creativity at the individual rather than the group level. That is, organizations may tie rewards to the creativity of employee input to the group in order to motivate employees to independently work harder at being creative (Pirola-Merlo and Mann 2004). However, prior research suggests that obstacles may also limit the effectiveness of individual-based creativity incentives. For example, theory suggests that creativity does not arise simply from motivating individuals to independently work harder (Amabile 1996).[2] Recent research in accounting supports this premise. For example, in settings without opportunities for collaboration, reward systems motivating individuals to work harder at being creative often do not lead to more creative output (Kachelmeier et al. 2008; Kachelmeier and Williamson 2010).

While prior research suggests that obstacles limit both individual- and group-based creativity-contingent incentives, prior research typically only considers a single form of creativity-contingent incentive, something akin to piece-rate pay where better performance results in strictly higher rewards (Sprinkle 2008). We next develop a theory-based process model suggesting that group-based tournaments can help overcome obstacles found to limit the effectiveness of creativity-contingent incentives and ultimately lead to a more creative group solution relative to group piece-rate pay.[3] Figure 1 summarizes this process model.

Group Tournament Pay Relative to Group Piece-Rate Pay

In this section, we discuss the effects of group tournament pay that induces intergroup competition relative to group piece-rate pay. An example of a tournament scheme that induces intergroup competition in practice is a competitive budgeting system that funds and provides higher pay to the most creative production teams (Kanter et al. 1997; Simons 2000; Nussbaum 2005). As another example, organizations such as Boeing and DuPont post pressing problems and request teams to submit proposals to address them. Each team is often constrained to submit a single proposal in competition with other teams for a pre-specified cash reward (Brabham 2008). We next develop how organizations can use intergroup competition to help overcome obstacles that limit the effectiveness of group-based creativity-contingent incentives.

Theory in psychology posits that when a group is challenged or threatened by outsiders, such as other groups competing with it for an organization's scarce resources, group members feel an increased sense of cohesion or solidarity with one another (Fiedler 1967; Friedkin 2004). These feelings result because intergroup competition makes salient in-group and out-group categorizations

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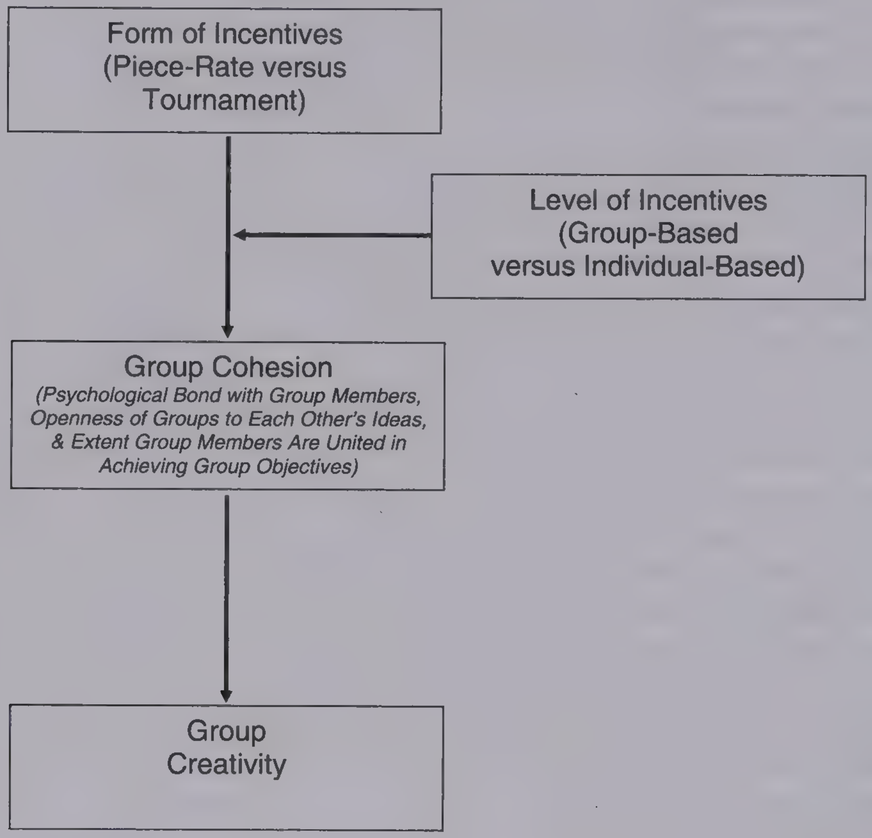
[2] Of course, measuring creativity at the individual level would likely also be more costly than measuring creativity at the group level. However, using a laboratory experiment allows us to control for these measurement issues so that we can better isolate the behavioral effects of individual relative to group-based creativity-contingent incentives.

[3] Given that prior research identifies obstacles that limit the effectiveness of both individual- and group-based creativity-contingent incentives, we do not make a prediction as to the main effect of incentive level (individual versus group) on group creativity. Rather, our focus is on the potential differential effect of incentive form (piece-rate versus tournament) on group relative to individual creativity-contingent incentives. In environments where a measure of group (individual) creativity is available, we examine the implications of either using the measure to induce intergroup (intragroup) competition or making pay an increasing function of this measure.

FIGURE 1
Process Model


Form of Incentives
(Piece-Rate versus
Tournament)
Level of Incentives
(Group-Based
versus Individual-Based)
Group Cohesion
(Psychological Bond with Group Members,
Openness of Groups to Each Other's Ideas,
& Extent Group Members Are United in
Achieving Group Objectives)
Group
Creativity

and thus encourages group members to categorize themselves as a single group rather than a collection of individuals (Tajfel and Turner 1979; Turner et al. 1987). Supporting this theory, prior empirical research demonstrates that competition and rivalry among groups enhance group solidarity and the psychological bond among group members (e.g., Bornstein and Erev 1994; Mulvey and Ribbens 1999).

To the extent that group tournament pay enhances group cohesion relative to group piece-rate pay, it can help mitigate evaluation apprehension and free-riding that can limit the effectiveness of group-based creativity incentives. First, members of cohesive groups feel that other group members are more open to their ideas and, thus, feel less anxious that group members will negatively evaluate their thoughts and ideas. Ultimately, these attitudes could lead members to more willingly share their ideas about problems faced by the group and comment on the thoughts and ideas of others.

Second, members of cohesive groups feel more united in trying to achieve group objectives. That is, individuals are more likely to adopt group goals as personal goals, thus blurring the distinction between self-interest and group interest (Bornstein and Erev 1994; Kramer and Brewer 1984). This attitude could drive them to exert greater effort to achieve collective goals, focus on collective outcomes rather than individual rewards, and maintain their commitment to the collective even when it does not benefit them individually (Brewer 1979; Chen et al. 1998; van Knippenberg 2000; Towry 2003; Ellemers et al. 2004). If so, then individuals in a group-creativity setting could be more willing to exert the cognitive efforts necessary to process, synthesize, and build on each others' ideas.

Ultimately, the more open exchange of ideas and efforts to synthesize and build on these ideas can enhance the effective integration of divergent perspectives and lead to more creative individual inputs (Paulus 2000; Van der Vegt and Bunderson 2005). Moreover, because group members are more receptive to each others' ideas, they can better coordinate efforts to further develop or build on the most creative individual inputs to transform them into more creative group solutions (Tagger 2002).

Individual Tournament Pay Relative to Individual Piece-Rate Pay

In this section, we discuss the effects of individual tournament pay based on intragroup competition relative to individual piece-rate pay. Examples of individual-based (intragroup) tournaments, in which individuals compete against members of their own departments or teams, include competition for scarce promotion opportunities or a fixed bonus pool to be distributed among managers (Milgrom and Roberts 1992, Ch. 11). Relative to individual piece-rate pay, research suggests that intragroup competition sparked by individual tournaments motivates more independent individual efforts. For example, in a laboratory experiment where three-person groups each produced a subcomponent of a castle from toy blocks that can then be joined together, Drake et al. (1999) identified conditions where intragroup tournament pay increased independent individual efforts such that individuals produced more units of their castle subcomponent.

These individual efforts, however, are unlikely to facilitate group cohesion and collaborative efforts among group members, which are essential for group creativity. As such, examining the effects of individual tournament pay in a group-creativity setting provides the opportunity to further validate our theory-based process model. If motivating independent individual efforts does not enhance group creativity, then we provide further support for our model suggesting that reward systems better promote group creativity by fostering group cohesion. Ultimately, to the extent that (1) results from individual-creativity settings suggesting that independent efforts do not enhance creativity generalize to a group-creativity setting (Kachelmeier et al. 2008; Kachelmeier and Williamson 2010), and (2) our model suggesting that reward systems spark group creativity by promoting group cohesion is descriptive, we do not expect individual-based tournaments to have a positive effect on group creativity relative to individual piece-rates.

Hypotheses

Based on the preceding discussion, we expect that group tournament pay, but not individual tournament pay, will lead to a more creative group solution than group and individual piece-rate pay, respectively. Accordingly, we predict an ordinal interaction between the level and form of creativity-contingent incentives. For clarity, we use two hypotheses to describe the form of the expected interaction.

H1: Group tournament pay will lead to a more creative group solution than group piece-rate pay.

H2: Individual tournament pay will not lead to a more creative group solution than individual piece-rate pay.

In addition to testing our formal hypotheses, we also examine the underlying processes driving our results. In particular, we provide a test of our process model in Figure 1 by examining whether group cohesion at least partially mediates the positive group creativity effect of group tournament pay. Moreover, we explore how our experimental conditions affect aspects of groups' idea generation, development, and selection processes.

III. METHOD

Participants

We recruited 180 undergraduate student volunteers from upper-level business classes of a large state university to participate in one of 14 75-minute experimental sessions (9 to 18 participants per session). The participants included 17 percent sophomores, 73.5 percent juniors, and 9.5 percent seniors, and 48 percent of the participants were female. We randomly assigned participants to three-person groups and all group interaction occurred via computer. To ensure anonymity, we assigned each participant a code consisting of a group number and one of three letters (A, B, or C).

Experimental Procedures

At the start of each session, participants read through a set of instructions on their computer screen. Appendix A provides the experimental instructions, including a screen shot summarizing the screen functions of the computer program that facilitated group-member interactions. After reading the instructions, individuals practiced using the software. No communication with group members took place during the practice session. We resolved any questions or confusion regarding the software during the practice session.

After the practice session, participants read about their incentive scheme, which was located on the top right corner of their computer screen. As a manipulation check, participants answered a pre-experimental question regarding the incentive scheme immediately after reading the incentive scheme. We checked every participant's answer to confirm that they understood their incentive scheme before we started the idea generation session.

In the idea generation session, each three-person group spent 30 minutes developing a specific proposal for a *creative use* for an abandoned house located on their college campus. Consistent with the psychology literature (e.g., Amabile 1996), the instructions defined a *creative use* as "a use that is original, innovative, and implementable within a reasonable budget." We provided each group with a brief history and a floor plan of the two-story house.

During the 30-minute proposal development phase, group members interacted via a collaborative "ideation program" (Toubia 2006). The ideation program organizes group member inputs in trees where initial thoughts or ideas ("parent-ideas") appear on the far left of the screen and follow-up ideas ("child-ideas") appear to the right of the associated parent-ideas. At any time during the experiment, participants could choose to either contribute a new parent-idea or build on an existing parent or child idea. Each idea was labeled with the identification code of the group member providing the input.

For example, a group member initiated the parent-idea, "Experimental house for energy efficiency." Under this parent-idea, a group member added the following child-idea, "Moreover, part of the project could be addressing how to modernize energy consumption while keeping historical significance intact." While the group developed this proposal, another group member added another parent-idea, "historical museum for children," which was also subsequently developed further with child-ideas.

Participants could also provide comments on any idea in a tree. Group members often used comments to coordinate group efforts. For example, participants used comments such as, "we need to discuss what to put in the [museum]," to focus the group's efforts. Additionally, groups used the comment function for general discussion about the ideas such as one group member commenting, "there are already so many [restaurants] on campus, do you think it would work?" Another group member responded, "that could work because [the house] is on the other side of campus where there aren't many eating places." Each comment was labeled with the identification code of the group member providing it.

After the 30-minute proposal development phase, the experimenter disabled the ideation program. Each group then selected one of the proposals it developed as the group solution. Specifically, each group submitted a parent-idea with all its associated child-ideas as the group's solution. During the idea selection period, group members could communicate only via the comment function and they could not add any new parent- or child-ideas. Each group member had to agree on the specific proposal to submit. To ensure that the group members reached an agreement, the last comment of each group member had to identify the same selected idea. All groups reached consensus.

Panels A and B of Exhibit 1 provide a sample submitted proposal from the group and individual tournament conditions, respectively. Notice that in the proposal from the group tournament pay condition, group members built extensively on each other's parent- and child-ideas. By contrast, in the proposal from the individual tournament condition, the individual initiating the selected idea performed the bulk of the idea development. As illustrated in the "Results" section, these proposals are representative of their respective experimental conditions.

After selecting a proposal, each participant independently completed a post-experimental questionnaire addressing aspects of group cohesion and collecting demographic and other information. Participants received a $5 show-up fee after the session. In addition to this show-up fee, as described below, participants also received performance-contingent compensation approximately four weeks after the experimental session, the nature of which depended on the experimental condition of the group.[4] The experiment involved no deception of any kind.

Experimental Design

We randomly assigned each three-member group to one of four experimental conditions created by manipulating two dimensions of participants' reward system in a between-subjects design. We describe these two factors below.

Level of Creativity-Contingent Incentives

Our first manipulated factor is the level of the creativity-contingent incentive provided to participants. Half of the participant-groups received compensation based on the creativity of their group's solution. The other half received compensation based on the creativity of their individual input to the group's solution.[5]

A panel of three graduate students from the same business school as our participants rated the creativity of proposals. Working independently and blind to our experimental manipulations, each rater read through the same set of instructions as our participants and the submitted solutions of all groups. They then rated the creativity of each group solution on a scale from 0 (lowest creativity) to 10 (highest creativity). We instructed the raters to ensure the average rating across all solutions was a 5.

These raters also evaluated the creativity of each individual group member's input to the group's submitted solution. To facilitate these evaluations, we labeled each input to the solution with the code of the participant who provided the input. Again, raters evaluated the creativity of

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[4] We informed participants of this delay when recruiting them.

[5] By rewarding participants for their individual contribution to the selected group solution, we attempt to give the individual-based incentive condition its best chance of increasing the creativity of the group solution. That is, since groups would most likely select one of their best ideas as the group solution, participants in the individual reward condition would have an incentive to creatively develop the group's best ideas in order to increase their assigned performance measure. However, we subsequently conducted a supplemental experiment in which we measured and rewarded individuals based on the creativity of their input to all group ideas. Our results are robust to this alternative measure of individual creativity.

EXHIBIT 1

Submitted Group Proposals from the Group and Individual Tournament Pay Conditions

Panel A: Example of a Group Proposal from the Group Tournament Pay Condition

B: Bed & Breakfast

B: More precisely, we would have to hire some personnel to manage the B&B. this may be too costly considering staff, maids, person to do laundry

C: However, people would pay a lot to stay there, especially the elderly

B: On the other hand, would people pay a lot to stay in a run-down place?

C: On the other hand, it's gonna get fixed up

A: Indeed, people may pay more, but what is to say that the iHotel is does not offer more?

B: However, people may pay more for the more quaint experience, so maybe our target would be the elderly

C: More precisely, yes, old people have lots of money. Staying in the oldest building on campus would be more enjoyable than writing a check. Perhaps a contest that allows the winning donors to stay in the house for a weekend?

B: Indeed, having a contest for incentive is a good idea and we can advertise the house as very fixed up and comfortable to make the elderly feel more at home.

C: yep we have to think creativity here

A: Indeed, is keeping the integration of the museum and the bed n breakfast out of the question here?

C: well that is a great idea. Having the b and b themed like a museum for the old people. they would like that

A: In particular, need to discuss how and what to fill the house with artifacts, artwork, displays, holographic theaters, What is the underlying theme of the museum? University over the past 150 years, development of state over that time, or both?

A: Indeed, get school to pull things out of the archives or off the walls of libraries or something to put into it.

C: More precisely, I think we need to drop the robotic holographic talking theater idea. too futuristic, especially for the elderly. The theme of the museum would be the history of the university, with rotating displays

C: so are we leaning toward this for our creative pick. It is getting tricky to follow all of these threads

A: More precisely, I think since we have two creative ideas we should incorporate both. Unless anyone has a better idea. time is kind of limited

C: More precisely, could not agree more

A: Indeed, You could have all types of incentive and sponsoring events depending on the time of year to allow big donors to invite who every the like to stay there for the weekend or you could have large donors for the football stadium stay there at no char

A: Another way to look at this is what if we incorporate the bed n breakfast with the museum idea and have it be decorated with artwork and artifacts from around the state and University. Possible have an interactive room. It would incorporate the two lar

B: Indeed, nice, take two ideas and put it into one. It would be a good idea to decorate it with artwork and artifacts when renovating.

C: should we create a new thread for the combined art museum and b and b idea

(continued on next page)

EXHIBIT 1 (continued)

B: Bed & Breakfast

- **B**: In particular, we would also have to hire cooks to make the breakfast
  - **A**: Instead, Have the mangers who run the house cook like an actual bed n breakfast
    - **B**: On the other hand, i don't think it's a good idea to have managers be the cooks, because then who would manage the place and the staff?
      - **C**: Another way to look at this is a general manager of the property would
- **B**: In particular, how much would we charge the guests? Would this be enough to cover all of the costs? How would we advertise the B&B?
  - **A**: More precisely, is the location of the house in proper city zone for it it be converted into a bed n breakfast for the public's use?
    - **B**: However, I'm sure location is fine considering it is on-campus. It would be ideal for grandparents visiting their grandchildren who attend the University because they would be on-campus
      - **A**: In particular, could we try an move the house to a better or more convenient location?
- **B**: More precisely, so just to make it clear, this B&B would be renovated and decorated with historical artwork and artifacts to provide a comfortable and quaint experience to the elderly.
  - **B**: More precisely, what other details do we need to figure out? We figured out general labor expenses for personnel to maintain the place. We need to get washing and dryer machines to clean linens.
    - **A**: More precisely, I'm sure we could get a majority of this to get donated
  - **B**: In particular, we could also get donations from the university or alumni to support the continued renovation of this historical landmark

Panel B: Example of a Group Proposal from the Individual Tournament Pay Condition

C: A coffee shop

- **C:** More precisely, it could be themed as a university café
- **C:** More precisely, This coffee shop idea would go hand-in-hand with 1799, because if it operated as a cafe, it would be a place for students and faculty to lounge and relax
- **C:** More precisely, It could be themed as University Cafe- and host various significant University memorabilia on the walls/upstairs, while functioning as a cafe where students and faculty could relax. It could also feature a specialty dessert or coffee drink theme
- **A:** One problem with this approach is Moreover, we can also maybe incorporate idea 1814 with the cafe feel.
- **C:** More precisely, To go along with the coffee shop idea, it is something that students and alumni alike could come to- enjoy the ambiance, and see the University memorabilia, the upstairs could feature an University museum paying homage to the school mascot.
- **C:** More precisely, It would be a way to connect students- because not many might consider visiting a museum on a regular basis, however they might consider frequently visiting the Campus Café where they can relax, meet new friends, visit with alumni,
  - **A:** More precisely, it would also create a way for alumni to feel at home. A place that has so much history. Maybe it can even house all of the University sports pictures and etc.
  - **C:** More precisely, I agree, it would be such a great way to connect students to their heritage here at the University. Alumni would also love to come back and visit and gain the nostalgia of reliving the Big 4 basketball memories, the College Bowl Game.

each individual's input on a scale from 0 (lowest creativity) to 10 (highest creativity). We instructed the raters to ensure the average rating across all individual ratings was a 5. Cronbach's alpha for the group and individual ratings was 0.89, indicating a reasonable level of consistency in the ratings. Thus, we averaged the three ratings to obtain our measure of group and individual creativity.[6]

To help validate the graduate student ratings, we also obtained creativity ratings from an expert. Specifically, a manager from the campus historic preservation office that manages the abandoned house ranked six group solutions that we chose to represent the full range of our actual creativity ratings. These rankings corresponded perfectly with the rank order of the average creativity ratings by the graduate students.

Form of Creativity-Contingent Incentives

Our second manipulated factor is the form of the creativity-contingent incentive. We paid half of our participant-groups a linear piece-rate based on their assigned performance measure. The other half of our participants received tournament pay where the participant or group performing best among three competitors received the highest compensation. Here, participants rewarded based on group creativity competed against two other groups in their session (i.e., an intergroup tournament), and participants rewarded based on individual creativity competed against the two other individuals in their group (i.e., an intragroup tournament).

Across all four conditions, we held average creativity-contingent compensation constant at $15 per participant.[7] In our individual piece-rate condition, participants received $3 multiplied by their individual input creativity rating where, as discussed above, the average rating was a 5.[8] In our group piece-rate condition, groups received $9 multiplied by their group creativity rating, where the average rating was a 5, and the three group members equally shared this piece-rate. In our individual (intragroup) tournament condition, the group member with the highest individual input creativity rating of the three group members received $45 and the other two group members received $0. Finally, in the group (intergroup) tournament condition, the group with the highest group creativity rating of the three groups received $135, where the three group members equally shared this tournament pay, and the other two groups received $0.

IV. RESULTS

We now present analyses that investigate the effects of the level (group versus individual) and form (piece-rate versus tournament) of the creativity-contingent incentive on the creativity of the group solution and the underlying processes driving these effects. We conduct all analyses at the three-person group level. As such, we average the individual scores of each group's three members when applicable. For our hypotheses tests and supplemental analyses, we control for the percentage of females in each group, calculated by dividing the number of female group members by 3. We do so because prior research suggests that females both tend to participate more in computer-mediated discussions (Hsi and Hoadley 1997) and ultimately tend to be more creative on discussion-based tasks (Picariello 1994).[9] Therefore, as discussed in Kinney (1986), we use the percentage of females in each group as a covariate to increase the power of our statistical tests by removing variance that is unrelated to our research question of interest.[10]

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[6] We asked the creativity raters to provide a justification for each of their ratings. Based on these justifications, proposals receiving a high creativity rating often developed either an original/innovative overall theme that was implementable or an ordinary overall theme with original/innovative aspects such as creative ways to interface with student groups on campus. Examples of creative proposals include: (1) a Civil War Visitors Center serving food and presenting theater/other entertainment consistent with the time period, giving a creative outlet to student groups across campus, and (2) allowing student groups from each college to turn a room into an exhibit in order to introduce prospective students to each college's offering, where, in each year, student groups with the best exhibit receive funding for their organizations. Proposals receiving lower creativity ratings typically developed a use associated with a college campus without original/innovative aspects such as (1) a coffee shop, or (2) a space that can be rented out by groups on campus.

[7] Participants received an average compensation of $20 (i.e., $5 show-up fee + $15 average creativity-contingent compensation). They also received extra credit from the courses in which we recruited them for participating in the experiment.

[8] We communicated the creativity rating range and average in participant instructions.

Test of Hypotheses—Creativity Ratings

Our hypotheses predict an ordinal interaction such that group tournament pay will lead to a more creative group solution than group piece-rate pay (H1) and individual tournament pay will not lead to a more creative group solution than individual piece-rate pay (H2). Panel A of Table 1 provides descriptive statistics for group creativity ratings. With group creativity ratings as our dependent measure, Panel B of Table 1 provides the results of an ANCOVA with form and level of incentives as our independent variables.[11] Consistent with our hypotheses, this ANCOVA yields a significant form × level interaction ($F = 12.30$, one-tailed $p = 0.04$).

Follow-up analyses on this interaction, reported in Panel C of Table 1, provide formal tests for our hypotheses. Consistent with H1, participants with group tournament pay developed more creative group solutions than those with group piece-rate pay ($F = 2.34$; one-tailed $p = 0.06$). Consistent with H2, participants with individual tournament pay did not develop more creative group solutions than those with individual piece-rate pay ($F = 1.18$; two-tailed $p = 0.28$). Moreover, while not statistically significant at conventional levels, average group creativity ratings drop from 5.47 for participants with individual piece-rate pay to 4.87 for participants with individual tournament pay, suggesting that, if anything, individual tournament pay is detrimental to group creativity. Collectively, these results fully support our hypotheses.[12]

Test of Process Model

Development of our first hypothesis argues that group tournaments lead to more creative group solutions than group piece-rates by promoting greater group cohesion. Group cohesion encapsulates beliefs about the psychological bond among group members, the openness of group members to the ideas of others, and the extent to which members are united in trying to achieve the group objective. We capture judgments about group cohesion using eight post-experimental questions adapted from the Group Environment Questionnaire (GEQ) scale (Widmeyer et al. 1985). Panel A of Table 2

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[9] Consistent with this reasoning, females contributed more words during the group discussion than males ($t = 1.87$; one-tailed $p = 0.03$) and our percentage of female covariate is positively related with group creativity ($F = 14.14$; $p < 0.01$). This covariate is significant in most analyses. However, for expositional convenience, we only report the statistic on this covariate for tabulated results.

[10] Other demographic information we collected such as the percentage of native English speakers and the average age of the group is not significantly related to group creativity and, therefore, we do not include these variables as covariates in our analyses.

[11] Given the same three individuals provided creativity ratings for all groups and individuals, we include creativity rater as a within-subjects factor when analyzing both group and individual creativity ratings. We find neither a main effect of creativity rater nor any significant creativity rater by between-subjects factor interactions (all p's $>$ 0.50). Thus, we neither tabulate nor report these results.

[12] After removing the effects of the percentage of females in the group covariate, adjusted group creativity means by condition are as follows: 5.86 for the group tournament, 5.41 for the group piece-rate, 5.09 for the individual piece-rate, and 4.87 for the individual tournament. Without the covariate, our form × level interaction is still significant ($F = 12.30$; one-tailed $p = 0.04$). Moreover, we find group tournaments increase group creativity ($F = 1.87$; one-tailed $p = 0.086$) and individual tournaments have no effect on group creativity ($F = 1.33$; two-tailed $p = 0.250$) relative to group and individual piece-rates, respectively. Though slightly weaker, these results do not materially change our inferences.

TABLE 1

The Effects of Level and Form of Creativity-Contingent Incentives on Group Creativity Ratings

Panel A: Means (Standard Deviations) for Group Creativity Ratings[a]

	Individual-Based Incentive	Group-Based Incentive
Piece-Rate Incentive	5.47 (2.67) (n = 15)	5.09 (2.52) (n = 15)
Tournament Incentive	4.87 (2.53) (n = 15)	5.80 (1.91) (n = 15)

Panel B: Analysis of Variance

Factor	df	Mean Square	F	p-value[b]
Form of Incentives[c]	1	0.55	0.29	0.64
Level of Incentives[d]	1	4.98	2.76	0.24
Form × Level of Incentives	1	19.34	12.30	**0.04**
Percent of Female Group Members	1	79.73	14.14	<0.01
Error	167	5.64		

Panel C: Simple Effects

	df	F	p-value[b]
Effect of Incentive Form *within* Group-Level Incentives	1	2.34	**0.06**
Effect of Incentive Form *within* Individual-Level Incentives	1	1.18	0.28

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[a] An independent panel of three raters assessed the creativity of group solutions.

[b] Reported p-values are two-tailed unless testing a one-tailed prediction, as indicated in bold.

[c] We manipulated *Form of Incentives* between subjects at two levels. Participants received either linear piece-rate pay or participated in a winner-take-all tournament.

[d] We manipulated *Level of Incentives* between subjects at two levels. Participants received pay based on either the creativity of the group solution (Group-Based Incentive) or the creativity of their individual input to the group solution (Individual-Based Incentive).

summarizes the specific statements used to capture group cohesion and the descriptive statistics by condition for participants' responses to eight 11-point Likert scale statements with "0" labeled "not at all" and "10" labeled "completely." As reported in Panel B of Table 2, factor analysis of individual responses indicates that all eight items load on one factor (all loadings > 0.50). Moreover, the Cronbach's alpha on these eight measures is 0.87, which exceeds typical reliability thresholds (Peterson 1994). Accordingly, we base the analyses in this section on the average of all eight items.

Untabulated results suggest that, consistent with our theoretical model, group tournament pay increases group cohesion relative to group piece-rate pay ($F = 2.88$; $p = 0.05$, one-tailed), and that individual tournament pay has no effect on group cohesion relative to individual piece-rate pay ($F = 0.02$; $p > 0.50$, two-tailed). Collectively, these results suggest that group tournament pay, but not

TABLE 2

Descriptive Statistics and Factor Analysis of Group Cohesion Measure

Panel A: Descriptive Statistics of Statements Used to Capture Group Cohesion

	Group-Based Incentive Mean (Standard Deviation)		Individual-Based Incentive Mean (Standard Deviation)	
Statement	**Piece-Rate**	**Tournament**	**Piece-Rate**	**Tournament**
1 As a whole, I liked the members in my group.	7.51 (8.13)	8.13 (1.78)	7.02 (2.15)	7.04 (2.04)
2 Our group was united in trying to generate a creative idea.	7.73 (2.44)	8.69 (1.41)	7.13 (2.79)	7.16 (2.36)
3 My group did not give me enough opportunities to contribute my ideas. (Reverse Coded)	8.93 (1.78)	8.91 (1.82)	8.24 (2.16)	8.07 (2.40)
4 I did not like the style of work in this group. (Reverse Coded)	6.87 (3.01)	7.87 (2.28)	7.07 (2.05)	7.27 (2.28)
5 I felt attached to the group.	5.24 (3.18)	5.58 (2.79)	4.33 (3.19)	4.64 (2.99)
6 I felt quite similar to others in my group in terms of general attitudes and opinions.	6.64 (1.98)	7.60 (1.44)	6.13 (2.12)	6.11 (1.91)
7 I think my group was relatively more cohesive (united) compared to other groups in this study.	6.33 (2.42)	7.27 (1.53)	5.58 (2.20)	5.67 (2.33)
8 My group members were open to the ideas of others.	7.89 (1.80)	8.02 (1.99)	7.51 (2.83)	7.27 (2.21)
Average	7.14	7.76	6.63	6.65

Panel B: Factor Analysis of Statements Used to Capture Group Cohesion

Statement	Loading
1	0.55
2	0.83
3	0.53
4	0.90
5	0.66
6	0.83
7	0.81
8	0.76

Adapted from the Group Environment Questionnaire (Widmeyer et al. 1985).

Panel A of this table summarizes the statements used to capture group cohesion and the descriptive statistics of participant responses to these statements by condition. We elicited each statement using an 11-point Likert scale with "0" labeled "not at all" and "10" labeled "completely." We averaged responses across each group such that n = 15 for each condition.

Panel B of this table presents factor analysis for participants' responses to each statement.

individual tournament pay, enhances group cohesion relative to group and individual piece-rates, respectively.

We further investigate whether group cohesion partially mediates the positive group creativity effect of our creativity-contingent incentive conditions. We establish partial mediation if the following three conditions are met: (1) group tournament pay significantly increases group creativity as we demonstrate in Panel C of Table 1, (2) group tournament pay significantly increases group cohesion as we demonstrate in the paragraph above, and (3) controlling for the group creativity effect of group tournament pay, group cohesion still significantly increases group creativity (Baron and Kenny 1986; MacKinnon et al. 2002).[13,14] To test this final condition, we re-estimate the ANCOVA and follow-up analyses summarized in Table 1 after adding our measure of group cohesion as a covariate.[15] Consistent with condition (3), untabulated results show that group cohesion is still significantly associated with group creativity after controlling for group tournament pay ($F = 4.38$; one-tailed $p = 0.02$). Collectively, these analyses are consistent with group cohesion partially mediating the positive group creativity effect of group tournament pay.[16]

Did Group Tournament Pay Affect Idea Generation and\or Idea Development?

Given that group tournament pay enhances cohesion and ultimately leads to more creative group solutions relative to group piece-rate pay, it is also useful to explore whether groups in this condition went about forming their solution in discernibly different ways. In particular, groups across organizations differ in the extent that they are responsible for some or all aspects of the creative process such as generating an initial set of ideas and developing these ideas (West et al. 2006). While some groups such as those in our experiment are responsible for both generating and developing ideas, others are merely responsible for one piece of the creative process such as brainstorming an initial undeveloped set of potential problem solutions. As such, exploring where in the creative process group tournament pay influences behavior may provide a better understanding of whether group tournament pay can promote more creative group solutions across groups with these varying levels of responsibilities. That said, while theory suggests that participants in the group tournament condition exhibit greater tendencies to share ideas and build on each other's ideas, it is not clear *a priori* precisely how these tendencies systematically affect groups' solution-formation processes. Thus, we explore whether and, if so, how these tendencies materialize in the generation of initial ideas and the development of these initial ideas.

First, we examine whether group tournament pay increases either the number or diversity of initial ideas considered. To examine the number of ideas considered, we simply count the number of parent (initial) ideas generated by each group. Untabulated results highlight that group

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[13] See also David A. Kenny's mediation website for a very helpful discussion (http://davidakenny.net/cm/mediate.htm).

[14] Similarly, the joint significance test argues that if the effect of group tournament pay is significantly related to group cohesion (α) and group cohesion is significantly related to group creativity after controlling for group tournament pay (β) as we demonstrate in steps 2 and 3, then the indirect (mediating) effect captured by $\alpha \times \beta$ is also significant. MacKinnon et al. (2002) compares different methods to test mediation and concludes that the joint significance test provides the best balance of Type I error and statistical power.

[15] That is, with group creativity rating as our dependent measure, we estimate an ANCOVA with form of incentives (piece-rate versus tournament pay) and level of incentives (individual-based versus group-based) as our independent measures, as well as the percentage of females in each group and group cohesion as covariates.

[16] If group cohesion were to fully mediate the relation between our reward conditions and group creativity, we would no longer observe a significant group creativity effect of our reward conditions after controlling for group cohesion (Baron and Kenny 1986; MacKinnon et al. 2002). However, after controlling for cohesion, our level × form interaction continues to be significant ($F = 10.10$; one-tailed $p = 0.04$). Thus, although the significance of the follow-up comparison between group tournament and group piece-rate is reduced ($F = 1.59$; one-tailed $p = 0.105$), our results suggest that group cohesion partially, rather than fully, mediates this relation.

tournament pay did not affect the number of parent-ideas generated relative to group piece-rate pay (7.93 versus 8.80 average parent-ideas; $F = 0.53$; two-tailed $p = 0.47$). Second, we examine the diversity of these initial ideas by having a research assistant who was blind to the study's hypotheses categorize all parent-ideas into common categories or themes. This exercise generated ten categories, of which the two most common were "history, museum, and gallery" and "academic support services."[17] We then counted the number of distinct categories included in each group's set of initial ideas. Again, untabulated results demonstrate that group tournament pay did not affect the number of distinct parent-idea categories considered relative to the number generated under group piece-rate pay (5.93 versus 5.87 average categories; $F < 0.01$; two-tailed $p > 0.50$). Collectively, these results suggest that group tournament pay affected neither the number nor the diversity of initial ideas generated relative to group piece-rate pay.[18]

Second, we examine whether group tournament pay increases the extent to which group members build on each others' ideas when developing initial ideas. To capture this aspect, we examine the depth of idea development. We measure this construct as the maximum number of layers of ideas and comments contained in a parent-idea. For example, a parent-idea with two child-ideas both building on the parent-idea has a depth of 2, whereas a second parent-idea with two child-ideas, the first building on the parent-idea and the second building on the first child-idea, has a depth of 3.[19] We interpret the latter structure of idea development as reflecting a higher degree of collaboration among group members and greater extent of elaboration on a particular idea. Untabulated results suggest that group tournament pay leads to greater idea depth relative to group piece-rate pay (12.00 versus 7.87 average depth; $F = 3.51$; one-tailed $p = 0.03$).[20,21] Collectively, while participants with group tournament pay did not consider a greater number/diversity of initial ideas, they did build off each others' thoughts to a greater extent when developing these initial ideas.[22]

Did Reward Systems Affect Propensities to Select the Most Creative Idea?

The creativity of a group solution is a joint function of idea generation/development and idea selection. Because group members must ultimately select one of their ideas to submit, idea selection could affect the group's creativity rating. The magnitude of verbiage in our group interaction transcripts makes any meaningful evaluation of unsubmitted thoughts and proposals difficult. However, to explore the effect of our experimental conditions on potential variations in idea selection, we had a graduate student from the participants' university (not one of our original creativity raters) read the transcript of each group's interaction during the 30-minute proposal development phase. This rater then selected and ranked the three most creative solutions of each group without knowing which idea the group actually selected. We compare the idea our rater identified as most creative with the idea actually selected by each group.

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[17] The ten categories from most to least common include: (1) history, museum, and gallery; (2) academic support services; (3) dining and drinks; (4) hotel and related; (5) outreach services; (6) student lounge; (7) entertainment; (8) community related (e.g., church, community center); (9) store; and (10) experimental house for energy efficiency.

[18] Comparing across the individual tournament and piece-rate conditions, we observed differences for neither the number (7.93 versus 6.67 average parent-ideas; $F = 0.75$; two-tailed $p = 0.39$) nor diversity ($F < 0.01$; two-tailed $p > 0.50$) of ideas considered by the group. Also, when comparing across our group and individual reward conditions, we do not observe significant differences for either the number of parent-ideas ($F = 0.95$; $p = 0.33$) or the number of distinct categories of parent-ideas ($F = 1.07$; $p = 0.31$).

[19] For this measure, a comment immediately following (building on) another comment also increases depth.

[20] Additionally, we find that our group cohesion measure is positively correlated with idea depth ($\rho = 0.22$; $p = 0.04$).

[21] Individual tournament pay did not lead to greater idea depth relative to individual piece-rate pay (5.67 versus 5.47 depth; $F = 0.01$; two-tailed $p > 0.50$).

[22] This result suggests that group tournament pay is more likely to increase the creativity of group solutions relative to group piece-rate pay for groups responsible for the idea-development aspect of the creative process. However, future research that investigates the efficacy of group tournament pay across groups with varying responsibilities for aspects of the creative process is needed to further validate this conclusion.

Across the 15 groups in each of our four conditions, the idea our rater identified as most creative was actually selected by the group 11 times in the group tournament, eight times in the group piece-rate, eight times in the individual tournament, and seven times in the individual piece-rate conditions. These relative proportions did not statistically differ across our four conditions ($\chi^2 = 2.43$; $p = 0.49$). Moreover, one of the two most creative ideas identified by our rater was selected by the group 13 times in the group tournament, 11 times in the group piece-rate, 12 times in the individual tournament, and 12 times in the individual piece-rate conditions. Again, these relative proportions did not differ across conditions ($\chi^2 = 0.83$; $p > 0.50$). Overall, we did not observe differences across our experimental conditions in groups' propensities to select the most creative ideas, suggesting that idea selection is unlikely to have affected our primary results.

Effects of Individual-Based Creativity-Contingent Incentives

We conclude our discussion of results by examining the implications of rewarding participants for the creativity of individual inputs. As already discussed, rewarding the creativity of individual inputs does not enhance group creativity irrespective of the incentive's form. Furthermore, consistent with our theoretical development, rewarding individual creativity also does not enhance individual creativity irrespective of the incentive's form. Panel A of Table 3 provides descriptive statistics for creativity ratings of individual inputs. With these individual creativity ratings as the dependent measure, Panel B of Table 3 provides the results of an ANCOVA with form and level of incentives as our independent variables. We find a significant main effect of the level of incentives suggesting that individual-based incentives decreased the creativity of individual inputs relative to group-based incentives ($F = 10.41$; two-tailed $p = 0.08$). This main effect is qualified by a significant form × level interaction ($F = 24.08$, one-tailed $p = 0.02$).

Follow-up analyses reported in Panel C of Table 3 shows that group tournament pay leads to more creative individual inputs relative to group piece-rate pay ($F = 5.11$; one-tailed $p = 0.01$).[23] This result is consistent with our theoretical development suggesting that group tournament pay increases the creativity of individual input by encouraging participants to synthesize and build on the many perspectives of group members. We did not observe an effect of tournament pay on individual creativity across our individual-based incentive conditions ($F = 0.14$; two-tailed $p = 0.71$).

While individual-based creativity-contingent incentives did not increase creativity, they did motivate greater independent individual effort. We assess independent individual efforts by assessing group members' energy devoted to developing their own ideas. Specifically, we count the number of words contained in parent-ideas (i.e., initial ideas) and the number of words in child-ideas that build on group members' *own* ideas. Untabulated results suggest that individual-based incentives did increase individual efforts relative to group based incentives (431.54 versus 309.07 words developing own ideas; $F = 9.26$; $p < 0.01$). Further, our theoretical development suggests that individual tournament pay would increase individual efforts relative to individual piece-rate pay. While the pattern of means is in the expected direction (443.47 versus 419.60 words developing own ideas), this

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[23] To ensure that low creativity scores of individuals who did not contribute to the selected group idea are not driving our results, we replicated our results using the maximum individual creativity rating for each group. Using this measure does not change the inferences we make.

TABLE 3

The Effects of Level and Form of Creativity-Contingent Incentives on Individual Creativity Ratings

Panel A: Means (Standard Deviations) for Individual Creativity Ratings[a]

	Individual-Based Incentive	Group-Based Incentive
Piece-Rate Incentive	5.22 (2.48) (n = 15)	4.89 (2.28) (n = 15)
Tournament Incentive	5.00 (2.40) (n = 15)	5.89 (1.71) (n = 15)

Panel B: Analysis of Variance

Factor	df	Mean Square	F	p-value[b]
Form of Incentives[c]	1	8.56	5.67	0.14
Level of Incentives[d]	1	4.98	10.41	0.08
Form × Level of Incentives	1	16.60	24.08	**0.02**
Percent of Female Group Members	1	69.31	14.46	<0.01
Error	167	4.79		

Panel C: Simple Effects

	df	F	p-value[b]
Effect of Incentive Form *within* Group-Level Incentives	1	5.11	**0.01**
Effect of Incentive Form *within* Individual-Level Incentives	1	0.14	0.71

[a] An independent panel of three raters assessed individual creativity. We averaged responses across each group such that n = 15 for each condition.

[b] Reported p-values are two-tailed unless testing a one-tailed prediction, as indicated in bold.

[c] We manipulated *Form of Incentives* between subjects at two levels. Participants received either linear piece-rate pay or participated in a winner-take-all tournament.

[d] We manipulated *Level of Incentives* between subjects at two levels. Participants received pay based on either the creativity of the group solution (Group-Based Incentive) or the creativity of their individual input to the group solution (Individual-Based Incentive).

difference is not significant ($F = 0.20$; one-tailed $p = 0.33$).[24] Nevertheless, the fact that this measure is higher in the individual tournament condition relative to the average of this measure across the other three conditions provides some limited evidence that individual tournament pay tends to promote independent individual efforts ($F = 4.87$, one-tailed $p = 0.02$).

While this latter result is admittedly only suggestive, it is relevant to the objective of examining individual-based creativity-contingent incentives. Specifically, we desire to investigate whether findings from prior management accounting research, suggesting that motivating individual efforts does not lead to more creative output, generalize to a group setting. Consistent with this argument, neither individual nor group creativity ratings increase in our individual tournament condition relative to our other conditions. Moreover, these creativity ratings did not increase in our individual-based conditions relative to our group conditions. Collectively, our results suggest that motivating independent individual efforts does not appear to enhance individual or group creativity.[25]

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[24] Group tournament pay also did not increase individual efforts relative to group piece-rate pay (278.27 versus 330.87 words; $F = 1.54$; two-tailed $p = 0.22$).

Supplemental Experiment

A potential concern is that our finding that individual tournament pay does not enhance creativity relative to individual piece-rate pay could be driven by how we measured and rewarded individual creativity in our original experiment. Specifically, while we chose to measure the creativity of individual input to only the selected group solution in order to encourage individuals to develop the group's best ideas, the resulting individual tournament scheme could be demotivating. For example, to the extent that individuals under the individual tournament scheme believed that the group member who offered the initial parent-idea of the selected solution would win the intragroup tournament, such individuals may have had very limited incentive to creatively develop the group's best or any other idea.

Therefore, we conducted a supplemental experiment replicating our two individual-based incentive treatments using a different performance measure, the creativity of each group member's input to the entire group discussion. Specifically, in the supplemental experiment, the participants were told that "each group member will receive a single rating based on the creativity of his/her input to both the group's selected idea and any ideas not selected by the group." With this one exception, we used the same set of instructions and experimental material as in our original experiment.[26]

Table 4 reports descriptive statistics of the resulting creativity ratings. Not only does the pattern of means suggest that individual tournament pay is detrimental to group creativity relative to individual piece-rate pay, as in our original experiment, but also this decrease is statistically significant ($t = 2.72$; two-tailed $p < 0.01$). Moreover, as in our original experiment, the form of the individual reward did not affect individual creativity ($t = 1.22$; two-tailed $p = 0.23$). Collectively, results from our initial experiment suggesting that individual tournament pay does not increase creativity relative to individual piece-rate pay are robust to this alternative way of measuring and rewarding the creativity of individual input.

V. CONCLUSIONS

We report the results of an experiment where three-person groups develop a creative solution to an important problem via computer interaction. In this environment, we examine whether the efficacy of either individual- or group-based creativity-contingent incentives depends on whether the incentives take the form of a piece-rate or tournament. Consistent with our theory-based process model, we find that intergroup tournament pay increases group cohesion and ultimately leads to more creative group solutions relative to group piece-rate. We further illustrate that group tournament pay increases the extent to which group members build on each others' ideas relative to group piece-rate pay. Further validating our model, we find that, while intragroup tournament pay increases individual efforts relative to the average of the other conditions, it does not enhance the creativity of group solutions or individual inputs to group solutions.

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[25] Consistent with this argument, our measure of individual effort is correlated with neither individual nor group creativity ratings (both p's > 0.42)

[26] We recruited an additional 90 undergraduate student volunteers from upper-level business classes of the same university as our initial experiment. As such, identical to our original experiment, 15 three-person groups per condition participated in our supplemental experiment.

TABLE 4

Creativity Ratings in a Supplemental Experiment[a]

	Piece-Rate Incentive[b]	Tournament Incentive[c]	t-statistic	Two-Tailed p-value
Creativity Rating of Group's Submitted Solution[d]	5.69 (1.25) (n = 15)	4.67 (0.74) (n = 15)	2.72	0.01
Creativity Rating of Individual Input to All Group Ideas	5.46 (1.02) (n = 15)	5.00 (1.05) (n = 15)	1.22	0.23

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[a] This table summarizes creativity ratings for a supplemental experiment we conducted to replicate our two individual-based incentive treatments using a different performance measure, the creativity of individual input to all group ideas. With this one exception, we used the same set of instructions and experimental material as in our original experiment (see Appendix A).

[b] Participants in the Piece-Rate Incentive condition received a linear piece-rate based on the creativity of their individual input to all group ideas.

[c] Participants in the Tournament Incentive condition received tournament pay where the participant who received the highest creativity rating, out of three competitors, for his/her individual input to all group ideas, received the highest compensation while the two other competing participants received nothing.

[d] Both the creativity of group's submitted solution and the creativity of individual input to all group ideas were assessed by an independent panel of three raters.

Our results contribute to a better understanding of observations from practice suggesting that organizations desiring group creativity often induce intergroup competition. For example, innovative companies such as Johnson & Johnson and Procter & Gamble invest heavily in competitive budgeting systems, rewarding members of the most innovative project teams with greater funding and pay (Simons 2000; Nussbaum 2005). Our findings also contribute to a better understanding of academic practices in that research teams and universities are becoming increasingly reliant on competitive grants for the funding of innovative projects (Geuna 2001). Beyond formal organizational practices, our results also provide potential insight with respect to the recent trend of "crowdsourcing." Here, organizations make open calls to groups of people requesting creative solutions to important problems and often pay groups with the most innovative solution. For example, InnoCentive, launched with funding from Eli Lilly, provides a platform where companies such as Boeing and DuPont post pressing R&D challenges to the scientific community and provide financial rewards to teams with the best solutions (Brabham 2008).

More generally, our results highlight the importance of gaining a better understanding of linkages between performance evaluation and reward system design and creativity in group settings. Our existing knowledge of these linkages comes predominantly from individual settings where research often suggests creativity is invariant to reward system design. Consistent with this literature, our results suggest that simply encouraging individual group members to independently work harder at being creative may not lead to the desired result. However, our results suggest that performance evaluation and reward systems can play a vital role in promoting group creativity by encouraging individuals to collaborate and build on the diverse ideas of others. These promising results will hopefully encourage future research in the area.

Limitations of our study provide opportunities for future research. First, we examined a setting in which groups interact for only a single period. Whereas this design choice is representative of some environments, such as the reliance on cross-functional teams formed to creatively address a specific problem, it may not be representative of other environments in which longstanding departments or teams must creatively address on-going problems and, thus, may already be a cohesive and collaborative unit. Second, since we measured cohesion before revealing groups' creativity ratings and compensation in order to help explain behaviors during our experiment, our results do not necessarily speak to feelings experienced after revealing tournament winners. An interesting question for future research is whether groups that ultimately lose an intergroup tournament in a given period continue to both feel highly cohesive and exert collaborative effort. Groups that lose a group tournament may feel discouraged and less cohesive, which could lead to less collaborative effort in future tournaments. Alternatively, groups losing a tournament in a given period may become even more cohesive and determined to win future tournaments, which could promote even more collaborative effort in the future. As such, future research could address the moderating role of multiple-period tournaments on the link between incentive system design, cohesion, and group creativity.

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APPENDIX A

EXPERIMENTAL INSTRUCTIONS

Ground Rules

Before describing the experiment, it is important to establish two ground rules.

1. NO TALKING WITHIN OR COMMUNICATION BETWEEN SESSIONS

While we hope that you find this experiment to be fun, it is also serious research. Please help us maintain control over the experiment by refraining from verbal communication with your fellow participants in this session or any communication with other students who might be participating in future sessions. If you have any questions at any time during today's session, just raise your hand and we will assist you.

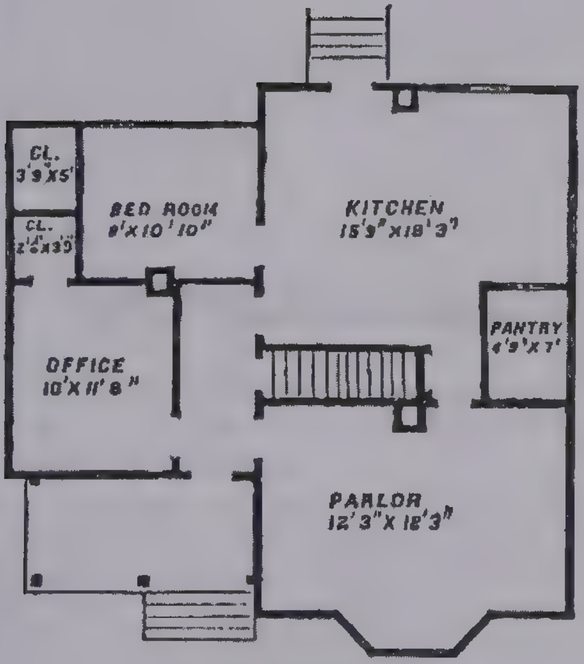
2. NO DECEPTION

We promise to carry out the experiment in the manner described in these instructions, with no deception of any form. You have already received a $5 show-up fee. As will be explained later, you can earn additional compensation from this experiment, which will be paid at a later date (in about four weeks). We promise that your compensation will be determined exactly as described in the rules explained later for this session.

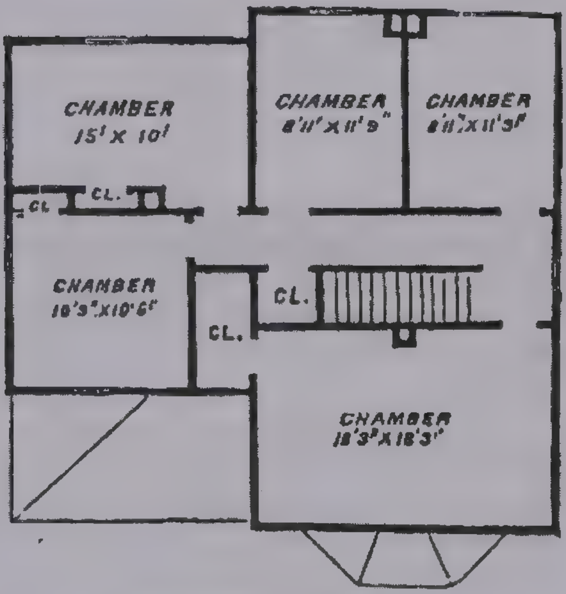
Experimental Task

Participants in today's session have been randomly assigned into groups of three persons each. Each three-person group will spend 30 minutes developing specific proposals for a creative use for the Mumford house located in the South Quad (i.e., a use that is original, innovative, and implementable within a reasonable budget).

Built just after the Civil War in 1870, the Mumford House is the oldest building on our campus. Despite being listed on the National Register of Historic Places, the building has been poorly maintained and, as a result, is currently uninhabited. In order to renew interest in this historical landmark, groups from around the state want to develop a creative use for the building which will help secure money for its renovation.



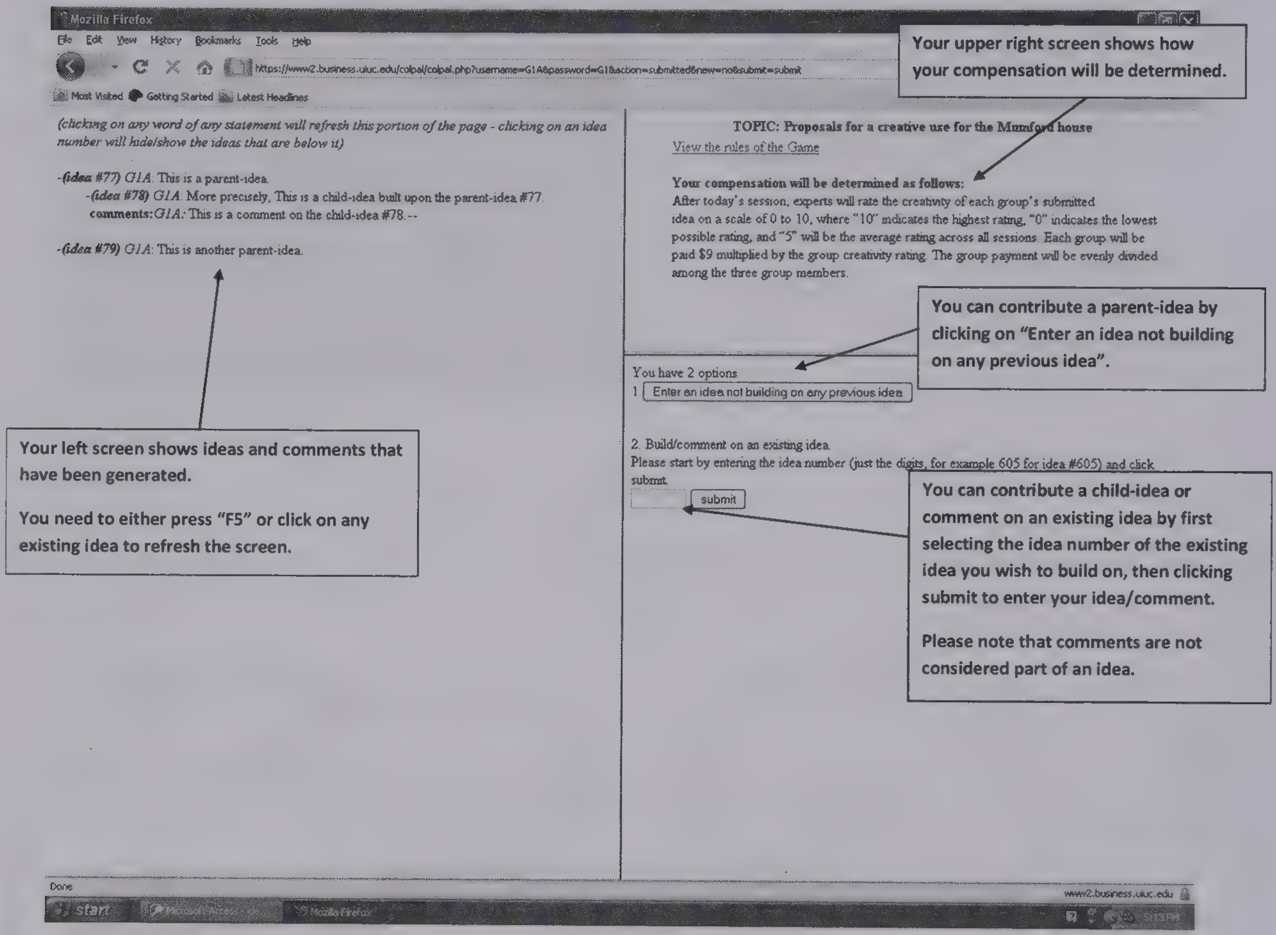
FIRST FLOOR.



SECOND FLOOR.

Developing Creative Proposals for the Mumford House

For 30 minutes, each group will interact and develop their creative proposals electronically via an ideation program. The ideation program organizes group ideas in "trees" where the original ideas, "parent-ideas," are placed far left of the screen and the follow-up ideas, "child-ideas," are placed below the existing parent-ideas that the child-idea relates. Here is what the ideation program looks like:


Mozilla Firefox
File Edit View History Bookmarks Tools Help
https://www2.business.uiuc.edu/colpal/colpal.php?username=G1A&password=G1&action=submitted&new=no&submit=submit
Most Visited Getting Started Latest Headlines
Done
www2.business.uiuc.edu
start
Microsoft Access
Mozilla Firefox

(clicking on any word of any statement will refresh this portion of the page - clicking on an idea number will hide/show the ideas that are below it)

-***(idea #77)*** *G1A*: This is a parent-idea.
  -***(idea #78)*** *G1A*: More precisely, This is a child-idea built upon the parent-idea #77.
  comments: *G1A:* This is a comment on the child-idea #78.--

-***(idea #79)*** *G1A*: This is another parent-idea.

TOPIC: Proposals for a creative use for the Mumford house

View the rules of the Game

Your compensation will be determined as follows:
After today's session, experts will rate the creativity of each group's submitted idea on a scale of 0 to 10, where "10" indicates the highest rating, "0" indicates the lowest possible rating, and "5" will be the average rating across all sessions. Each group will be paid $9 multiplied by the group creativity rating. The group payment will be evenly divided among the three group members.

You have 2 options
1. Enter an idea not building on any previous idea

2. Build/comment on an existing idea
Please start by entering the idea number (just the digits, for example 605 for idea #605) and click submit.
submit

Your upper right screen shows how your compensation will be determined.

You can contribute a parent-idea by clicking on "Enter an idea not building on any previous idea".

Your left screen shows ideas and comments that have been generated.

You need to either press "F5" or click on any existing idea to refresh the screen.

You can contribute a child-idea or comment on an existing idea by first selecting the idea number of the existing idea you wish to build on, then clicking submit to enter your idea/comment.

Please note that comments are not considered part of an idea.

Each group's idea-trees are recorded on the left-hand side of the screen. As seen in the image above, all child-ideas that build on a specific parent-idea have the same color and the same indentation.

At any time during the 30-minute interactive group session, group members can choose to either contribute a new parent-idea or build/comment upon an existing parent-idea by clicking on either "Enter an idea not building on any previous idea" or "Enter an idea to react to or build on idea #" on the right-hand side of the screen. When building on ideas, you should generally use "conjunctive phrases" like "More precisely," "In particular." These conjunctive phrases make sure that the ideas in a tree are linked not only verbally, but also conceptually.

> **Note:** New ideas and comments won't appear automatically on the screen. To see new ideas, you have to refresh the page, either by pressing F5 or clicking on any previous idea.

Each group will only be able to observe the ideas and comments of its three members (i.e., not the ideas and comments of participants in other groups).

Submitting a Creative Proposal for the Mumford House

After the 30-minute interactive session, each group will have 5 minutes to select one idea from the idea(s) it developed. Specifically, each group will submit the number of the parent-idea where the discussion and development of its most creative idea began. **Recall:** Parent-ideas are the initial ideas at the far left of the screen.

During this 5-minute period, group members will only be able to communicate via the comment function (i.e., no new parent- or child-ideas can be added).

Remember, comments won't appear automatically on the screen. To see new comments, you have to refresh the page, either by pressing F5 or clicking on any previous comment.

The final comment of the group should indicate the number of the parent-idea selected.

YOUR COMPENSATION

Group Piece-Rate

After today's session, doctoral students will rate the creativity of each group's submitted idea on a scale of 0 to 10, where "10" indicates the highest rating, "0" indicates the lowest possible rating, and "5" will be the average rating across all sessions. Each group will be paid $9 multiplied by the group creativity rating. The group payment will be evenly divided among the three group members.

Group Tournament

After today's session, doctoral students will rate the creativity of each group's submitted idea on a scale of 0 to 10, where "10" indicates the highest rating, "0" indicates the lowest possible rating, and "5" will be the average rating across all sessions. The group receiving the highest group creativity rating out of the three groups in today's session on your side of the lab will receive $135 and the other two groups with receive $0. The group payment will be evenly divided among the three group members.

Individual Piece-Rate

After today's session, doctoral students will rate the creativity of each group member's input to the group's submitted idea on a scale of 0 to 10, where "10" indicates the highest rating, "0" indicates the lowest possible rating, and "5" will be the average rating across all sessions. Each individual will receive $3 multiplied by his/her individual rating.

Individual Tournament

After today's session, doctoral students will rate the creativity of each group member's input to the group's submitted idea on a scale of 0 to 10, where "10" indicates the highest rating, "0" indicates the lowest possible rating, and "5" will be the average rating across all sessions. The individual from each group with the highest rating will receive $45 and the other two individuals will receive $0.

All Conditions

We will pay you your compensation in about four weeks, after we receive the creativity ratings. We promise that you and all others with this version of the research will receive cash compensation as described above for participating today.

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Sticks and Carrots: The Effect of Contract Frame on Effort in Incomplete Contracts

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ABSTRACT: In this study, we examine the effect of incentive contract framing on agent effort in an incomplete contract setting. Prior research suggests that when governed by complete incentive contracts, agents exert greater effort under penalty contracts relative to bonus contracts. However, in an incomplete contract setting, in which the incentive contract does not govern all tasks for which the agent is responsible, the agent's trust in the principal is relevant. In this setting, we predict that bonus contracts create a more trusting environment, and this effect spills over to tasks not governed by the incentive contract, such that bonus contracts elicit greater effort on these tasks as compared to penalty contracts. We develop and experimentally validate a theoretical model of the effects of contract frame on trust and effort in this incomplete contract setting. The main intuition behind the model is that the framing of an incentive contract affects the degree to which the contract terms are interpreted by the agent as a signal of mistrust. More specifically, penalty contracts engender greater distrust than do bonus contracts and, therefore, when contracts are incomplete, penalty contracts lead to lower effort on tasks not governed by the contract than do bonus contracts.

Keywords: *framing; incomplete contracts; control systems; trust; formal control; informal control; bonus; penalty.*

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We dedicate this study to the memory of John Dickhaut in honor of his "profound influence on a generation of graduate students and faculty colleagues" (G. Waymire).

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I. INTRODUCTION

Incentive contracts are ubiquitous as a means of motivating agent effort and improving firm performance. An incentive contract can be framed as either a bonus that offers an agent a monetary reward if performance goals are met, or as a penalty that reduces monetary payouts if performance goals are not met. Historically, bonus contracts have been more common in practice than penalty contracts.[1] Accounting research provides initial insights into agent *preferences* for bonus contracts (i.e., positively framed contracts) relative to penalty contracts (i.e., negatively framed contracts). Luft (1994) finds that agents prefer bonus contracts to economically equivalent penalty contracts. A potential explanation for this preference is loss aversion (Kahneman and Tversky 1979), the notion that the disutility experienced from a loss is greater than the utility experienced from a gain of the same magnitude. While agent preferences for bonus contracts are consistent with the predominance of bonus contracts observed in practice, principals choose incentive contracts based not solely on the preferences of agents, but rather on the efficiency with which various contracts induce desired agent effort levels.

While a large literature in psychology, economics, and accounting examines the effort-inducing effects of bonus contracts, few studies examine the effort-inducing effects of penalty contracts. One exception is Hannan et al. (2005), who find that despite stated preferences for bonuses, agents exert greater effort under penalty contracts so they can avoid paying the penalty. Hossain and List (2012) similarly find field evidence of greater effort under a penalty contract relative to a bonus contract. Authors of both studies conclude that the same loss aversion that induces a preference for bonus contracts induces higher effort under penalty contracts. Given the effort-inducing advantage of penalty contracts over bonus contracts documented by prior research, it is unclear why most organizations employ bonus contracts instead of penalty contracts. Indeed, Hannan et al. (2005) call for additional research to further understand the costs and benefits of each type of incentive contract. The purpose of this study is to examine whether contract completeness affects the effort-inducing efficiency of bonus contracts relative to penalty contracts. Specifically, we examine whether, in incomplete contract settings where trust is paramount to the agent's effort decision, bonus contracts, by creating a more trusting environment, can elicit greater effort as compared to penalty contracts.

Importantly, prior literature examining the effects of incentive contracts on agent effort (and agency theory in general) assumes a complete contract setting. A complete contract is one that fully specifies the obligations of the principal to provide monetary rewards to the agent for each potential future performance outcome. Moreover, these contractual terms can be enforced in a court of law (Baiman 1982). In such a setting, there is no discretion and, hence, no role for trust between the principal and agent (Casadesus-Masanell and Al-Najjar 2001). However, in practice, many incentive contracts are incomplete, allowing the principal some degree of discretion over the agent's monetary rewards. Discretion allows for opportunism on the part of the principal (e.g., Fisher et al. 2005) and, thus, introduces the possibility that the agent's trust in the principal will affect agent effort. Therefore, to the extent that contract frame—either bonus or penalty—affects the trust environment, the prior literature examining the effect of contract frame on employee effort is incomplete.

In this paper, we extend prior research by examining the effect of incentive contract framing in an incomplete contract setting. Whereas contract incompleteness can occur in various forms, in our setting, the contract is incomplete because it governs only one of two tasks for which the employee is responsible. Such settings are quite common because modern jobs are usually multi-faceted, with employees required to perform multiple tasks. These tasks vary in the degree to which their performance can be measured, and so when incentive contracts are used, they generally govern only a subset of the employees' tasks. In our laboratory setting, the agent performs an initial task for which performance-based payouts are enumerated by an incentive contract. We manipulate the incentive contract frame—either a bonus contract or a penalty contract—while maintaining economic equivalence between these contract types. We then introduce a second task that is not governed by the incentive contract and for which the principal has full discretion over the agent's compensation. With such discretion, trust becomes a potentially important determinant of agent effort on the second task. In this incomplete contract setting, we find that the contract frame manipulated in the first task affects effort measured in the second task in the opposite direction as that found in prior literature (e.g., Hannan et al. 2005), and this effect can be attributed to the effect of contract frame on the trust environment. Specifically, both trust and agent effort is lower in the penalty contract condition as compared to the bonus contract condition.

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[1] While the use of bonus contracts has historically dominated that of penalty contracts, interest in and the use of contracts that combine elements of both bonuses and penalties, so-called "bonus-malus" contracts, is on the rise. For example, UBS AG executives and board members have been working under a bonus-malus contract since 2009 (Associated Press 2008). Use of bonus-malus contracts in the U.S. is also increasing as a result of U.S. legislators' promotion of "clawback provisions" aimed at excessive CEO compensation (e.g., Hosking 2008; Lublin 2010).

This study contributes to the framing literature by documenting that the effect of contract frame on employee effort depends on the nature of the contractual setting. Prior literature shows that under complete contracting, where there is no role for trust, employee effort is likely to be higher under a penalty contract as compared to a bonus contract (Hannan et al. 2005). However, in settings of incomplete contracting (e.g., where there is discretion in monetary payouts), trust plays an important role. Bonus contracts create a more trusting environment, and this effect can spill over to tasks not governed by the incentive contract, such that bonus contracts elicit greater effort on these tasks as compared to penalty contracts. This result is important to managers implementing incentive contracts because it suggests that both contract frame (e.g., bonus versus penalty) and the degree of contract incompleteness (e.g., the use of discretion) must be considered in designing incentive compensation plans.

This study also contributes to a second, separate stream of literature that examines the effect of formal controls, such as incentive contracts, on trust, an important informal control. In general, this literature documents the unintended negative effects of formal controls on trust (e.g., Das and Teng 1998, 2001; Enzle and Anderson 1993; Malhotra and Murnighan 2002). However, this literature generally focuses on negatively framed formal controls. By contrast, our results show that the effect of formal controls on trust will depend on whether the formal controls are *framed* positively or negatively, with the result that positively framed controls can actually increase agents' trust relative to no control.

The remainder of the paper is organized as follows: In Section II, we review relevant literature and develop the hypotheses. In Section III, we describe the experimental method. Results are provided in Section IV. Section V discusses implications of the study and provides areas for further research.

II. BACKGROUND AND THEORY DEVELOPMENT

A large body of literature examines how individuals' decisions are influenced by the way in which information is framed (e.g., Tversky and Kahneman 1981; Levin et al. 1998). Framing can occur on many dimensions. For example, Tenbrunsel and Messick (1999) examine whether decision makers frame a decision to invest in pollutant-reducing technology as a business decision or an ethical decision. They demonstrate that a formal sanction for the undesirable behavior of violating pollutant regulations causes decision makers to frame the technology investment decision as a business decision rather than an ethical decision, resulting in less technology investment and more regulatory violations.[2]

Accounting research focuses primarily on the positive versus negative framing of incentive contracts, providing insights into agent preferences for, and responses to, positively framed bonus contracts relative to negatively framed penalty contracts.[3,4] Luft (1994) shows that experimental participants have a propensity to choose a bonus contract over a monetarily equivalent penalty contract. She offers several interpretations of this result. First, participants may prefer a bonus over a penalty contract because of loss aversion (Kahneman and Tversky 1979), the notion that the disutility experienced from a loss is greater than the utility experienced from a gain of the same magnitude. Thus, participants have preferences for bonus contracts because subjective valuations of the monetary payoffs are greater under the gain frame induced by the wording of the bonus contract. Second, Luft posits that there are different *nonmonetary* benefits associated with bonus and penalty wording that affect agent preferences. Bonuses are associated with praise and reward, while penalties are associated with condemnation and disapproval, the former being preferred over the latter.

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[2] For a thorough discussion of framing effects, see Levin et al. (1998).

Hannan et al. (2005) extend Luft (1994) by examining the effects of bonus versus penalty contracts on agent effort. The authors find that despite the agent's stated preferences for bonuses, as documented in Luft (1994), agents exert greater effort under penalty contracts. Hossain and List (2012) similarly find field evidence that workers in a Chinese high-tech manufacturing facility exert greater effort under a penalty contract relative to a bonus contract.

In both Hannan et al. (2005) and Hossain and List (2012), agents' effort choices are consistent with the loss aversion explanation of agent preferences for bonus contracts offered by Luft (1994). Specifically, the authors conclude that agents exert greater effort to avoid the loss associated with paying a penalty as compared to effort aimed at receiving the gain of a bonus. Importantly, however, Hannan et al. (2005) and Hossain and List (2012) are both conducted in a complete contract setting such that the contracts fully specify the agent payouts for each potential outcome; that is, the principal has no discretion over payouts. Yet, as Kreps (1990) notes, it is more characteristic of firms to have incomplete contracts, in which agents take actions without completely specified payouts from the principal. Luft (1994) uses the concept of incomplete contracts to offer a third interpretation for her finding of agent preferences for bonus contracts. Consistent with Kreps (1990), she acknowledges that contracts are typically incomplete, and that agents take actions, in part, based on potential monetary rewards, such as a discretionary bonus, for increased effort and performance beyond the contracted monetary rewards.

When contracts are complete, there is no discretion on the part of the principal and, hence, no effort-inducing role for trust between the principal and agent (Casadesus-Masanell and Al-Najjar 2001). However, when contracts are incomplete, as they are in most firms (Kreps 1990), the principal has some degree of discretion over the agent's monetary rewards, which allows for opportunism on the part of the principal (e.g., Fisher et al. 2005). In this setting—and in contrast to the complete contract setting of Hannan et al. (2005) and Hossain and List (2012)—the agent's trust in the principal becomes paramount in the agent's effort choice.[5] The question then becomes whether bonus and penalty contracts differentially affect the agent's trust in the principal. In the following paragraphs, we develop a theoretical model that describes the mechanisms by which

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[3] See Bonner (2008) for a discussion describing accounting research focused on framing effects.

[4] The importance of framing as a characteristic of formal controls, more generally, is evident in the extant management control frameworks. For example, Jensen and Meckling (1992) include both positively framed outcome controls (i.e., "rewards") and negatively framed outcome controls (i.e., "punishments") as important control tools that organizations use to align incentives of managers with owners. Similarly, Ouchi and Maguire's (1975) outcome controls may be framed either positively (a bonus for goal achievement) or negatively (a penalty for lack of achievement). These and other frameworks (e.g., Merchant and Van der Stede 2007; Simons 1995) suggest that control framing is an important characteristic of formal control.

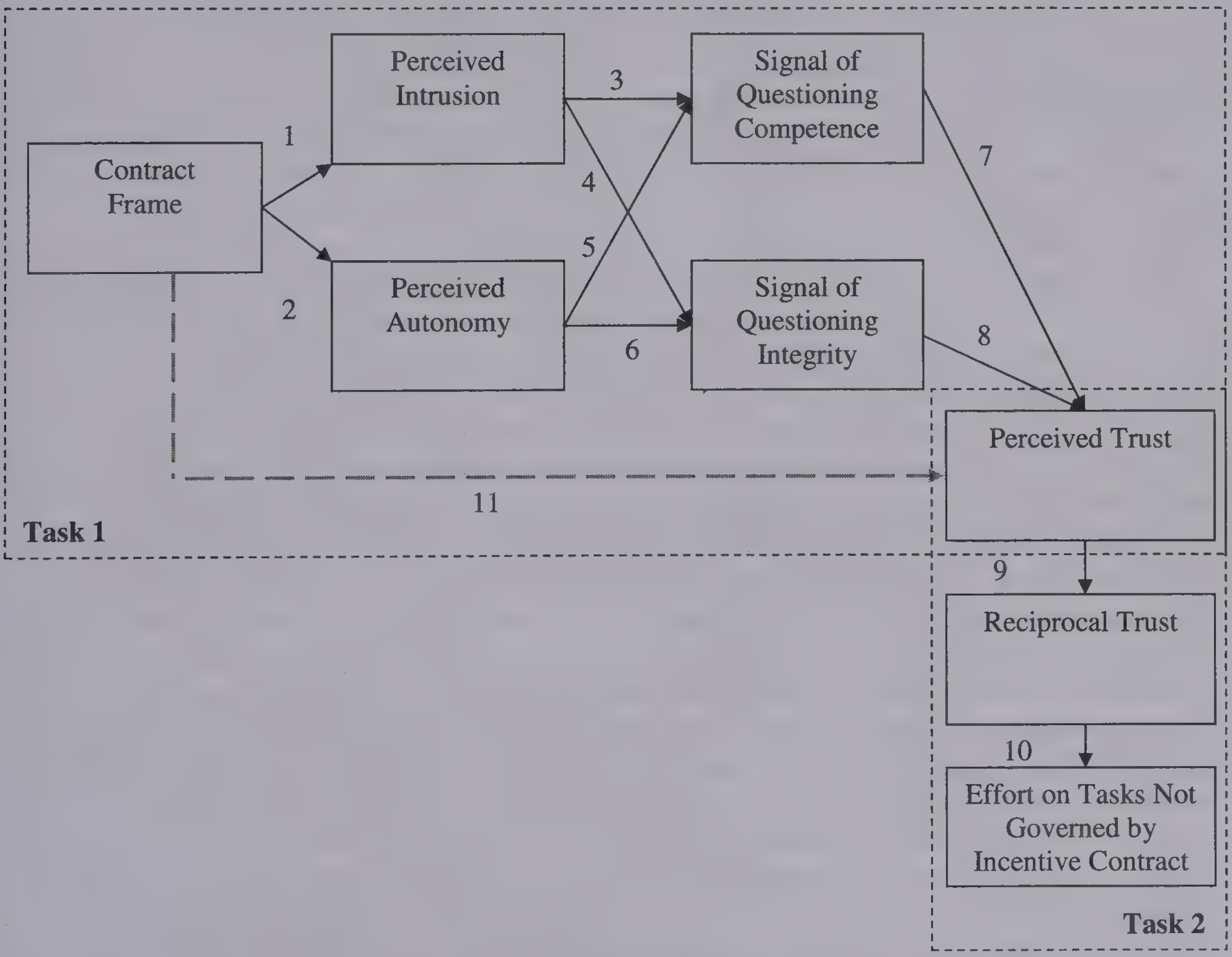
[5] While our primary construct of interest is trust, we acknowledge that this construct is highly related to other affect-based constructs such as fairness, perceived justice, or "liking" (Huang and Murnighan 2010). For example, accounting research (Kadous et al. 2011) demonstrates that auditors are more likely to trust advisors with whom they feel a strong personal affiliation. Therefore, it would be difficult, if not impossible, to fully separate trust from these other related constructs.

contract frame is expected to affect the trust environment and, ultimately, agent effort (see Figure 1).

Prior trust research has generally examined the effects of negatively framed formal controls. This research indicates that the implementation of a negatively framed formal control damages the trust environment (e.g., Das and Teng 1998; Enzle and Anderson 1993; Inkpen and Currall 1998; Malhotra and Murnighan 2002). Negatively framed controls damage the trust environment because they are perceived as being intrusive and as reducing the autonomy of the agent (Christ et al. 2008). Negatively framed controls often involve close observation or significant oversight, both of which may interfere with the agent's normal processes and activities and cause him or her to feel intruded upon. Further, negatively framed controls may limit the agent's decision rights, either directly, by curtailing certain behaviors, or indirectly, by attaching incentives to the outcomes of those decisions, leading to a perceived loss of autonomy.

Based on these general results, we likewise predict that framing an incentive contract negatively as a penalty contract will lead to perceived intrusion and a loss of autonomy by the agent. However, we posit that framing an incentive contract positively as a bonus contract will reduce, and perhaps even reverse, these adverse effects. Specifically, we expect that the agent's perception of intrusion (Figure 1, Path 1) and loss of autonomy (Path 2) will be lower under a bonus

FIGURE 1
Theoretical Model for the Effect of Contract Frame on Effort


Contract Frame
Perceived Intrusion
Perceived Autonomy
Signal of Questioning Competence
Signal of Questioning Integrity
Perceived Trust
Reciprocal Trust
Effort on Tasks Not Governed by Incentive Contract
1
2
3
4
5
6
7
8
9
10
11
Task 1
Task 2

contract than under a penalty contract. In fact, the agent may interpret the implementation of a bonus contract as a signal of the principal's intent to reward the agent for increased effort by sharing the resulting gains.

To the degree that an agent feels that controls intrude upon his or her actions and restrict his or her autonomy, these controls signal the principal's beliefs regarding the agent. Specifically, the principal's control choices signal the degree to which he or she questions the agent's integrity (Paths 4 and 6) and/or competence (Paths 3 and 5). First, by implementing controls that are more intrusive or more constraining on the agent's autonomy, a principal signals the expectation that the agent will intentionally engage in opportunistic behavior, such as shirking, cheating, stealing, etc. In this manner, the principal is sending a signal to the agent that he or she is questioning the agent's integrity. Second, more intrusive and more constraining controls may also indicate that the principal questions the agent's competence; that is, whether the agent is able to sufficiently complete the task or meet the required standards because of potential deficiencies in the necessary skill, aptitude, or tools, despite the agent's best intent and sufficient effort. Thus, the effect of contract frame will flow through, such that under a bonus contract, the agent will be less inclined to believe that the principal is questioning his or her competence and integrity than under a penalty contract.

To the degree that the agent believes his or her competence and/or integrity are being questioned, he or she will perceive less trust from the principal (Paths 7 and 8) (Das and Teng 2001). Further, because trust is reciprocal (Bradach and Eccles 1989; Gambetta 1988; Zand 1981), we expect the agent's trust in the principal to be higher under a bonus contract as compared to a penalty contract. Specifically, social projection theory suggests that individuals have a tendency to expect that others will behave similarly to themselves (Krueger 1998). According to this theory, the principal expects the agent to behave as he or she would and, thus, the principal's signal of trust in the agent reveals the principal's own trustworthiness to the agent. Therefore, by implementing a penalty contract that signals mistrust in the agent, the principal signals that he or she is likewise not trustworthy. In contrast, by implementing a bonus contract that signals trust, the principal provides a signal that he or she is trustworthy. Thus, agents working under a penalty contract will be less likely to trust the principal than will agents working under a bonus contract. That is, trust will be reciprocal (Path 9).

This reciprocity effect has been described in prior research that suggests that when one party feels (mis)trusted, it is likely to feel (mis)trust in return (e.g., Bradach and Eccles 1989; Gambetta 1988). For example, Christ et al. (2008) use an inter-organizational strategic alliance scenario to experimentally examine the effects of formal controls on trust. They show that formal controls have a negative effect on the controlled party's perceived trust (i.e., the extent to which the controlled party feels trusted by the controlling party), and a reciprocal negative effect on the controlled party's reciprocal trust in the controlling party. We extend this finding to the single firm, principal-agent setting. We predict that agents working under a penalty contract will perceive less trust and will, thus, reciprocate less trust relative to those working under a bonus contract.

In contrast to the complete contract setting of Hannan et al. (2005) and Hossain and List (2012), in our setting, the incentive contract does not govern all tasks. The principal has discretion over the agent's monetary rewards for the ungoverned tasks, allowing for opportunism on the part of the principal (e.g., Fisher et al. 2005). As a result, the agent's trust in the principal to reward the agent for effort becomes paramount in the agent's effort choice for the ungoverned tasks (Path 10). Because the agent's trust in the principal is damaged when the principal implements a penalty contract, the agent will have a pessimistic expectation of the extent to which effort will be rewarded by the principal's discretionary payout. By contrast, because the agent's trust in the principal is less damaged and instead is preserved or even enhanced when the principal implements a bonus contract, the agent will have a more positive expectation of the extent to which effort will be rewarded by the principal's discretionary payout.

Taken together (see Figure 1), we predict that agents working under a bonus contract will exert greater effort on tasks not governed by the incentive contract than will those working under a penalty contract.[6] This leads to our primary research hypothesis:

H1: In an incomplete contract setting, agent effort on tasks not governed by the incentive contract will be greater under a bonus contract as compared to a penalty contract.

III. EXPERIMENTAL METHOD

Experimental Design

We test our hypothesis and theoretical model using a computer-based experiment in an incomplete contract setting. Our experiment has two tasks, with performance-based pay for the first task defined by an incentive contract framed as either a bonus or penalty, depending on the experimental condition. Pay for the second task is at the complete discretion of the principal. We recruited 220 participants from graduate and undergraduate accounting classes of a highly ranked U.S. business school to participate in this study. Fifty-two percent of participants were female and 48 percent were male, and the average age was 20.4 years.

We use a 2 (Contract Frame) × 2 (Contract Implementation) plus 1 (Baseline) between-subjects experimental design to test our hypotheses. For our first variable—Contract Frame—we manipulate the type of contract that the principal has the option to implement for the first task: a positively framed contract that rewards agents for meeting performance targets with a bonus payment (hereafter, *bonus contract*), and a negatively framed contract that penalizes agents for missing the target (hereafter, *penalty contract*). We maintain monetary equivalence across contract frame conditions so that we can isolate the hypothesized framing effects from our model. The Contract Frame conditions are described in detail in the "Task 1 of the Experiment" subsection, below. The second variable—Contract Implementation (*Yes*, *No*)—is measured rather than manipulated, and represents the principal's decision to implement the incentive contract. Allowing the principal to select the contract is important because this choice signals the principal's trust in the agent (Christ 2012). As we will describe, our primary analysis relates only to the conditions in which the principal chose to implement the incentive contract. As a result, we do not analyze observations in the *bonus contract/no implementation* and *penalty contract/no implementation* conditions.

Finally, the "plus 1" condition is a baseline condition (hereafter, *fixed wage*) in which principals are not given the opportunity to implement an incentive contract for Task 1, and no bonuses or penalties are mentioned to the participants. The growing stream of literature examining the effects on trust of formal controls, including incentive contracts, compares agent behavior with and without formal control, taking as given a negatively framed formal control. To better reconcile our contract frame findings to this stream of literature, we include this *fixed wage* condition so that we can compare the effects on trust of the two contract frames versus a condition in which no incentive contract is imposed. Note that the agents in this *fixed wage* condition faced the same fixed wage contract as those in bonus and penalty conditions for whom the principals chose not to implement the incentive contract.

At the beginning of the study, each participant was randomly assigned to the role of either principal or agent and remained in the same role throughout the entire study.[7] Before beginning, the computer randomly matched participants into anonymous pairs of one principal and one agent.

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[6] Lazear (1991) also suggests that bonus wording conveys an expectation of effort beyond a desired level, while penalty wording conveys an expectation for effort only up to a certain level. The differential expectations conveyed by the bonus contract will further serve to motivate greater effort on the part of the agent.

[7] To ensure that labels did not unnecessarily influence the behavior of participants, agent and principal participants were referred to as Participant A and Participant B, respectively, throughout the experiment.

Participants earned points based on the decisions that they and their partners made, as described below. The points were converted to cash and paid to the participants at the end of the study according to the following formula: U.S.$ Payment = (Points earned × 0.025). At the beginning of the study, each principal was endowed with 250 points, and each agent with 850 points in the *bonus contract* condition, and 1,000 points in the other conditions. These differential endowments reflect that over the course of the experiment, the principals had the opportunity to gain more net points than did the agents. Further, these different endowments ensure that when the principal chooses to implement an incentive contract, the *bonus contract* and *penalty contract* conditions are monetarily equivalent from the agent's perspective.

Task 1 of the Experiment

In Task 1, the agent selected a portfolio of investments for the principal.[8] That is, the principal earned the returns from the portfolio, but the agent chose the specific investments to be included in the portfolio and paid for the chosen investments from his or her endowment. While we acknowledge that in a real-world investment setting, the agent would not normally pay the cost of investments, this setting provides a useful laboratory scenario for testing our theory.[9] Specifically, we follow prior literature (e.g., Fehr et al. 1993) and use the cost of the investment to operationalize the agent's effort, which also allows us to vary that cost across the various investment opportunities.

Each agent was presented with 30 possible investments. From these 30 investment choices, the agent's task was to select and pay for one share each of ten investments. That is, the agent selected ten different investments, each of which earned points for the principal. The principal's expected returns were, thus, maximized if the agent selected shares of the ten investments with the highest expected returns in the next period.

There were two types of investments from which the agent could choose, "Bell" investments and "Whistle" investments.[10] Each Bell share cost the agent 10 points and each Whistle share cost the agent 25 points. Agents were provided with a graphical representation of the previous 19 periods of returns in the form of points per share for each of the 30 possible investments. Figure 2 depicts the history of points per share returned for one representative Bell (Panel A) and one representative Whistle (Panel B) investment.[11]

Whistle shares, while more expensive, had a higher average expected return. This was the basis of the control problem in the scenario. Specifically, the principals preferred that shares with higher returns, the Whistle shares, be selected, but the agents, who paid for the investments, preferred to

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[8] For Task 1 of the experiment, we designed a cognitively challenging and engaging task for the agents, instead of using an abstract task as in the previous contract framing literature (e.g., Hannan et al. 2005). We chose this task because of the prior finding that formal controls influence employee trust because they are perceived to be intrusive and to limit the agent's autonomy (Christ et al. 2008). It is unlikely that these effects would manifest if agents engaged in an abstract effort choice.

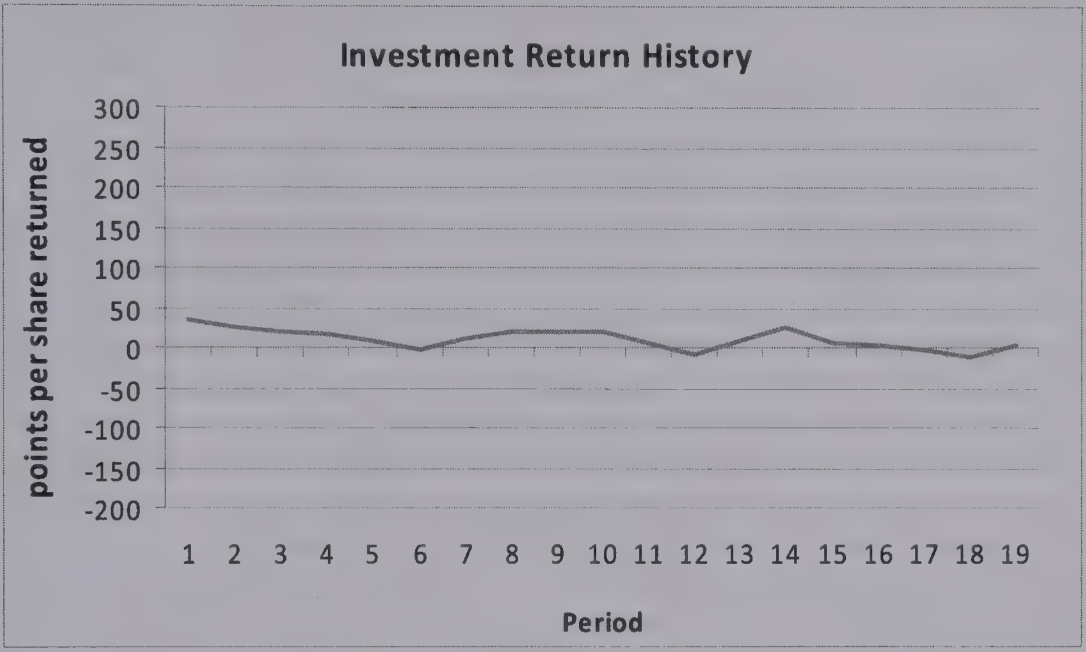
[9] The contextualized economic games used to examine our research question are not intended as a literal representation of how firms do (or should) organize investment decisions. While the experimental task is an abstraction from reality, it is the theory that we wish to generalize. The design of this task is important in that it provides evidence about the theoretical constructs of interest despite any shortcomings it has in its reflection of mundane realism. For further discussion, see Friedman and Sunder (1994, 10–12) and Kachelmeier and King (2002).

[10] An alternative approach would have been to have the agent select between one Whistle and one Bell investment. We chose to have agents select ten investments (from a total of 30 investments) because we wanted the task to be intellectually stimulating to the agents. Specifically, in order to examine agents' beliefs about the intrusiveness and autonomy-limiting nature of the contract, it was necessary for them to be actively engaged in the task. Therefore, the task itself needed to be a significant undertaking and cognitively difficult.

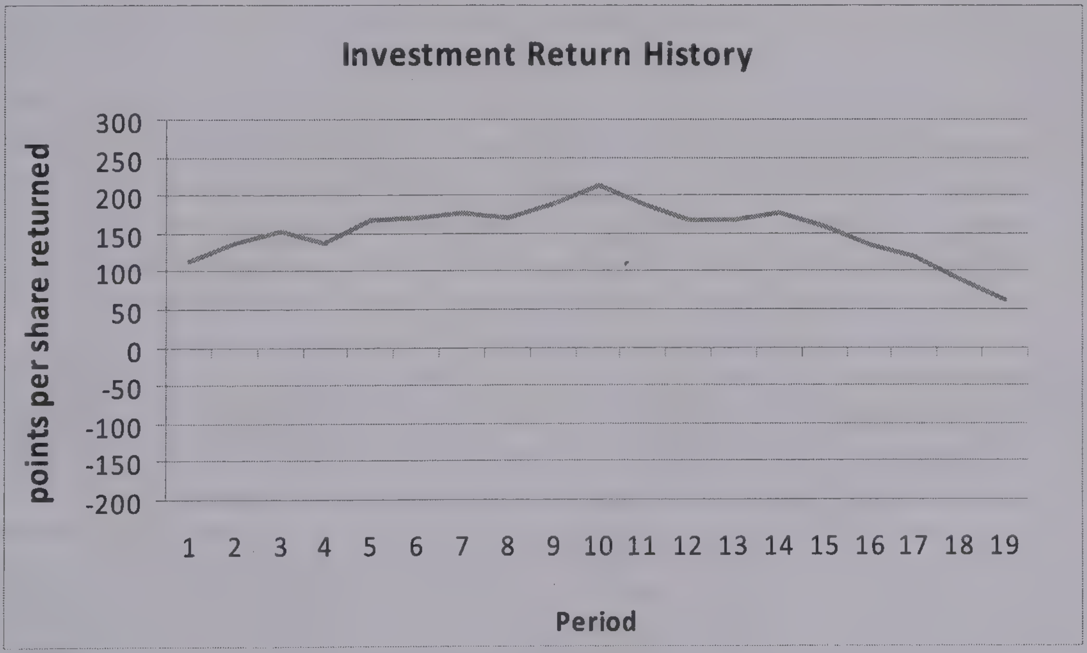
[11] We constructed each investment's return history using either a long-term trend or a cyclical pattern, plus a noise parameter. Thus, the expected return for the next period was calculated as the expected value for the next point in the series given the underlying historical pattern.

FIGURE 2
Sample Investment Graphs

Panel A: An Example of the Investment History for One Bell Investment



Panel B: An Example of the Investment History for One Whistle Investment



select the cheaper Bell shares. The expected return for the next period for Bell shares was, on average, 20 points per share across all Bell investments, whereas the expected return for the next period for Whistle shares was, on average, 50 points per share across all Whistle investments. Importantly, any individual investment could earn considerably more or less than the average.

Specifically, the expected return for the 15 individual Bell investments ranged from −8 to 48, and the expected return for the 15 individual Whistle investments ranged from −6 to 106.

In the two Contract Frame conditions, principals chose whether or not to implement an incentive contract prior to the agents' investment decision. Implementation of the incentive contract cost the principal 50 points. The incentive contract, if implemented, either established a bonus for the agent if total returns met or exceeded a minimum return (*bonus contract* condition), or established a penalty for the agent if total returns failed to meet a minimum return (*negative contract* condition). In the *fixed wage* condition, the principal was not given the opportunity to implement an incentive contract; that is, neither a bonus nor a penalty was mentioned to the participants.

In the *bonus contract* condition, if the principal elected to implement the incentive contract, the agent received an extra 150 points if the total return of all ten shares selected met or exceeded 500 points. No bonus points were paid to the agent if the total return was less than 500 points. The bonus was paid from the administrator's fund, not deducted from the principal's endowment.[12] Likewise, for the *penalty contract* condition, the agent was required to pay 150 points if the total return of all ten shares selected was less than 500. No penalty points were paid if the total return equaled or exceeded 500 points. The penalty was paid to the administrator's fund, not added to the principal's earnings. The *bonus* and *penalty contracts* provided monetarily equivalent incentives for the agents, so that any differences in reported trust perceptions following Task 1 or observed effort on Task 2 were due to the signaling effects of the contracts.[13] The summary of points that could be earned by each participant for Task 1 is presented in the experimental materials provided in Appendix A.

After the principal decided whether or not to implement an incentive contract, the agent was notified of this decision. At this point, the agent made the investment choices, and the investment payoff was determined by the administrator. Note, however, that actual returns were not revealed to participants until the end of the second task of the experiment. We withheld this information from all participants so that the agent's behavior on Task 2 would be influenced only by the framing of the contract imposed by the principal, and would not be affected by his or her earnings from Task 1.

Following Task 1, we solicited information using a post-experimental questionnaire. Participants responded to several questions intended to capture additional variables included in our formal model. Specifically, each agent indicated, on a 100-point Likert scale ranging from "not at all" to "a great deal," the extent to which he or she (1) felt that the principal questioned his or her integrity (Signal of Questioning Integrity), (2) felt that the principal questioned his or her competence (Signal of Questioning Competence), (3) felt that the principal trusted him or her (Perceived Trust),[14] (4) trusted the principal (Reciprocal Trust), (5) felt the principal intruded upon his or her decisions (Perceived Intrusion), and (6) felt he or she had decision-making autonomy (Perceived Autonomy).[15]

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[12] This design choice ensured that the principal's cost of implementing the contract was held constant (at 50 points) across all conditions. An alternative approach would have been to have the principal pay from his or her account all costs, including the agent's total compensation (including wages, bonuses, and penalties) and to increase the principal's pay level to cover these costs. We chose not to take this approach because it would have complicated the experimental task considerably, and as long as the included costs were held constant across conditions, we are aware of no theories to suggest that this approach would affect our results.

[13] Note that the incentive contracts were designed to be fairly weak controls, in the sense that they do not provide the agent clear incentives to choose the optimal investment portfolio. This design choice, along with the absence of mutually observable actions, allows us to isolate the signaling effect of the contracts as distinct from the economic benefit documented in Coletti et al. (2005) and, thus, to investigate the difference between the signals engendered by the penalty contract as compared to the bonus contract.

[14] We follow prior literature and measure perceived and reciprocal trust using single-item Likert scale questions. However, we acknowledge that trust is a complex and difficult-to-measure construct and, therefore, this variable may capture other related factors, such as the perceived fairness of the principal, general effect, perceived justice, etc. To partially capture these constructs, we also include in our empirical model a direct path from Contract Frame to Perceived Trust (Figure 1, Link 11) that does not go through our theoretically motivated paths related to trust signaling (i.e., perceptions of intrusion, autonomy, questioning competence, and questioning integrity).

Task 2 of the Experiment

While the focus of Task 1 was on the effects of contract framing on perceived and reciprocal trust (Figure 1, Links 1–9, 11), Task 2 provided the primary dependent variable, Effort on Tasks Not Governed by the Incentive Contract. The principal's discretion over pay for Task 2 made the agent's trust relevant (Figure 1, Link 10). In Task 2, participants retained the same partners and roles, and continued with the same contextual setting in which agents made investments on behalf of principals. Each participant began Task 2 with the total number of points he or she had at the end of Task 1 following the investment decisions made by the agent. Although the participants did not know the exact number of points they had earned up to this point because they had not yet been told the returns from the shares selected by the agent, they were told that each agent had a minimum of 600 points with which to make an additional investment, as guaranteed to be the case by design.

Task 2 was a contextualized version of the standard "trust game," as defined by McCabe et al. (2003), with the agent moving first and the principal moving second. In Task 2, the agent made an additional investment for the principal. This investment decision was different from the prior investment decisions in several important ways. First, there was only one type of investment, a "Horn" investment, which cost the agent 10 points per share, whereas in Task 1, some investments cost 10 points and some cost 25 points per share. Second, the return per Horn share investment was known to be 30 points, whereas in Task 1, the return per share was uncertain, with expected returns based on the history of past returns. Third, the agent could purchase from zero to 50 shares of the Horn investment, whereas in Task 1, the agent could only select one share each of ten different investments. Note that the guaranteed bank of 600 points for each agent is enough to invest in the full 50 Horn shares at 10 points per share.

The agent's choice of the number of Horn investments to make is our measure of costly agent effort. We follow a large literature that operationalizes agent effort using a costly choice (e.g., Fehr et al. 1993), based on the simplifying assumptions of analytical models that individuals have utility for wealth and leisure (see Baiman 1982).[16] Importantly, investing in Horn shares is an effort choice that is controlled by the agent, results in disutility (because the cost of the shares is borne by the agent), and is correlated positively with output in the form of the principal's return. Thus, the investment in Horn shares meets the definition of effort used in the agency theory literature, as outlined by Baiman (1982).

As in Task 1, the principal earned the returns from the portfolio, equal to 30 points × the number of shares of the Horn investment purchased by the agent. As in the standard trust game, after learning how many shares the agent purchased, the principal could then pay any amount of the

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[15] Several recent studies have examined whether eliciting participants' beliefs about trust prior to a traditional trust game affect subsequent trusting behavior (Guerra and Zizzo 2004; Zizzo 2008). Results of these studies indicate that participants' trusting behavior exhibited in the trust game does not differ significantly when beliefs are elicited as compared to conditions when there is no belief elicitation.

[16] The first mover investment in a trust game is a widely used measure of behavior within a trust environment (Camerer 2003, 85–100). We employ the first mover investment as a simple and straightforward measure of effort. Alternative design choices for this experiment would require less reliable measures of effort (e.g., time spent on the task) that are likely to be affected by other factors outside of the experiment (e.g., a preference for completing the experiment quickly). Recent research in accounting has compared the operationalizations of effort as cost with real-effort tasks in an experiment on reciprocity, and finds that people react similarly to both operationalizations of effort (Bruggen and Strobel 2007). Although prior literature consistently operationalizes effort as a linearly increasing cost to the agent, we acknowledge that the cost of effort is not necessarily linear and that this is a limitation of this research design.

TABLE 1

Descriptive Statistics

Means (standard deviations in italics)	Fixed Wage n = 30[a,b]	Bonus Contract n = 25[a,c]	Penalty Contract n = 24[a,c]
Effort on Tasks Not Governed by Incentive Contract[d]	24.80	33.92	21.44
	17.98	*17.22*	*15.18*
H1: Test of Mean Difference in Effort between Bonus and Penalty Conditions ($t = 12.48$, $p < 0.01$)			
Perceived Intrusion[e]	32.50	31.04	60.00
	26.71	*32.64*	*31.98*
Perceived Autonomy[e]	74.83	74.58	57.20
	25.31	*23.95*	*31.46*
Signal of Questioning Competence[e]	52.50	46.46	58.80
	23.22	*30.20*	*25.51*
Signal of Questioning Integrity[e]	54.67	49.79	56.40
	29.39	*32.01*	*27.41*
Perceived Trust[e]	52.83	68.96	24.48
	24.41	*24.09*	*23.52*
Reciprocal Trust[e]	56.67	65.42	32.00
	28.39	*21.41*	*26.34*

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[a] The number of participants is twice the number of observations reported here because one observation requires two participants.

[b] This column includes observations from the *fixed wage* condition, but not those from the *penalty* and *bonus contract* conditions when the principal chose not to implement the incentive contract.

[c] We report the descriptive statistics only from those dyads in which the principals chose to impose the incentive contract because these are the observations used for our hypothesis test.

[d] Effort is measured using the number of Horn shares A Participants purchased during Task 2 of the experiment.

[e] Participants responded to the following questions using a 100-point Likert scale:

- To what extent do you feel that Participant B has intruded on your decisions? (Perceived Intrusion)
- To what extent do you feel that you have the autonomy to make decisions? (Perceived Autonomy)
- To what extent do you feel that Participant B questions your competence? (Signal of Questioning Competence)
- To what extent do you feel that Participant B questions your integrity? (Signal of Questioning Integrity)
- To what extent do you feel that Participant B trusts you? (Perceived Trust)
- To what extent do you trust Participant B? (Reciprocal Trust)

return back to the agent, such that monetary payouts for agent effort are at the complete discretion of the principal. Importantly, the principal could choose to keep all of the returns and pay nothing to the agent. Thus, the principal had the opportunity to behave opportunistically, making the agent's trust in the principal an important determinant of the agent's chosen effort.

Without obligation to return anything, standard economic theory predicts that the principal would pay none of the Horn investment returns to the agent. Anticipating this, the agent would not invest in any Horn shares; i.e., exert no effort. Thus, any positive level of effort in the form of investment by the agent in costly Horn shares critically depends on the level of the agent's trust in the principal to return at least the cost of the Horn shares (McCabe et al. 2003).

Following completion of Task 2, participants were given a questionnaire that included demographic information and manipulation checks regarding the extent to which they understood the tasks completed. Participants received their respective payoffs resulting from decisions made in both parts of the experiment and were dismissed. Descriptive statistics for variables defined above are provided in Table 1.

IV. RESULTS

Hypothesis Test

Our primary interest is in examining the effect of contract frame on effort for a task not governed by the incentive contract. Our primary test of H1 compares effort on Task 2 (i.e., the number of Horn shares purchased) when the principal chose to implement the bonus contract, to effort when the principal chose to implement the penalty contract. Thus, for this analysis, we ignore the *fixed wage* condition, as well as those observations in the *bonus contract* and *penalty contract* conditions for which the principal chose not to implement the incentive contract. Consistent with H1, agents exert greater effort in the *bonus contract* condition than in the *penalty contract* condition (see Table 1, means = 33.92 and 21.44, respectively). This difference is statistically significant ($t = 12.48$, $p < 0.01$). Thus, when a task is not governed by the incentive contract, we find that the contract frame affects agent effort in the opposite direction to that found in prior literature (e.g., Hannan et al. 2005). To further assess whether the effort difference between the bonus and penalty contract conditions can be attributed to the hypothesized effect of contract frame on the trust environment, we test the complete theoretical model depicted in Figure 1 in the following section.

Test of Complete Theoretical Model

We use structural equations-based path analysis to estimate our theoretical model depicted in Figure 1. Note that in addition to the ten model paths described in the theoretical development of H1, we also include in the model an additional path by which bonus and penalty contracts differentially affect the trust environment, separate from the posited signaling path that is our primary focus (Figure 1, Path 11). This additional general path captures other affect-based constructs by which contract framing may affect the trust environment.[17] We acknowledge, however, that this path will likely only partially capture these constructs, as their influence might also be embedded in the signaling path.

To assess the model, we conduct a test of goodness of fit. The Comparative Fit Index (CFI), a measure of the proportion of improvement of the fit of our model to the null model, is 0.965, which is above the generally accepted minimum value of 0.95 (Byrne 2001). We further confirm the model's goodness of fit with a traditional Chi-square test ($\chi^2 = 10.76$, $p = 0.71$), and an Incremental Fit Index (1.02) that is above the recommended minimum of 0.95 (Byrne 2001). Thus, the model provides a good fit for describing the relations in the data.

The standardized path coefficients and statistical significance are presented for the estimated model in Figure 3. As expected, results indicate that participants perceive a greater sense of intrusion (30.12, $p < 0.01$) and less autonomy (−15.06, $p < 0.05$) under a penalty contract than under a bonus contract. Greater Perceived Intrusion is positively associated with the Signal of Questioning Competence (0.42, $p < 0.01$) and the Signal of Questioning Integrity (0.37, $p < 0.01$). In addition, when the agent feels that his or her integrity is being questioned, Perceived Trust is reduced (−0.52, $p < 0.01$).[18]

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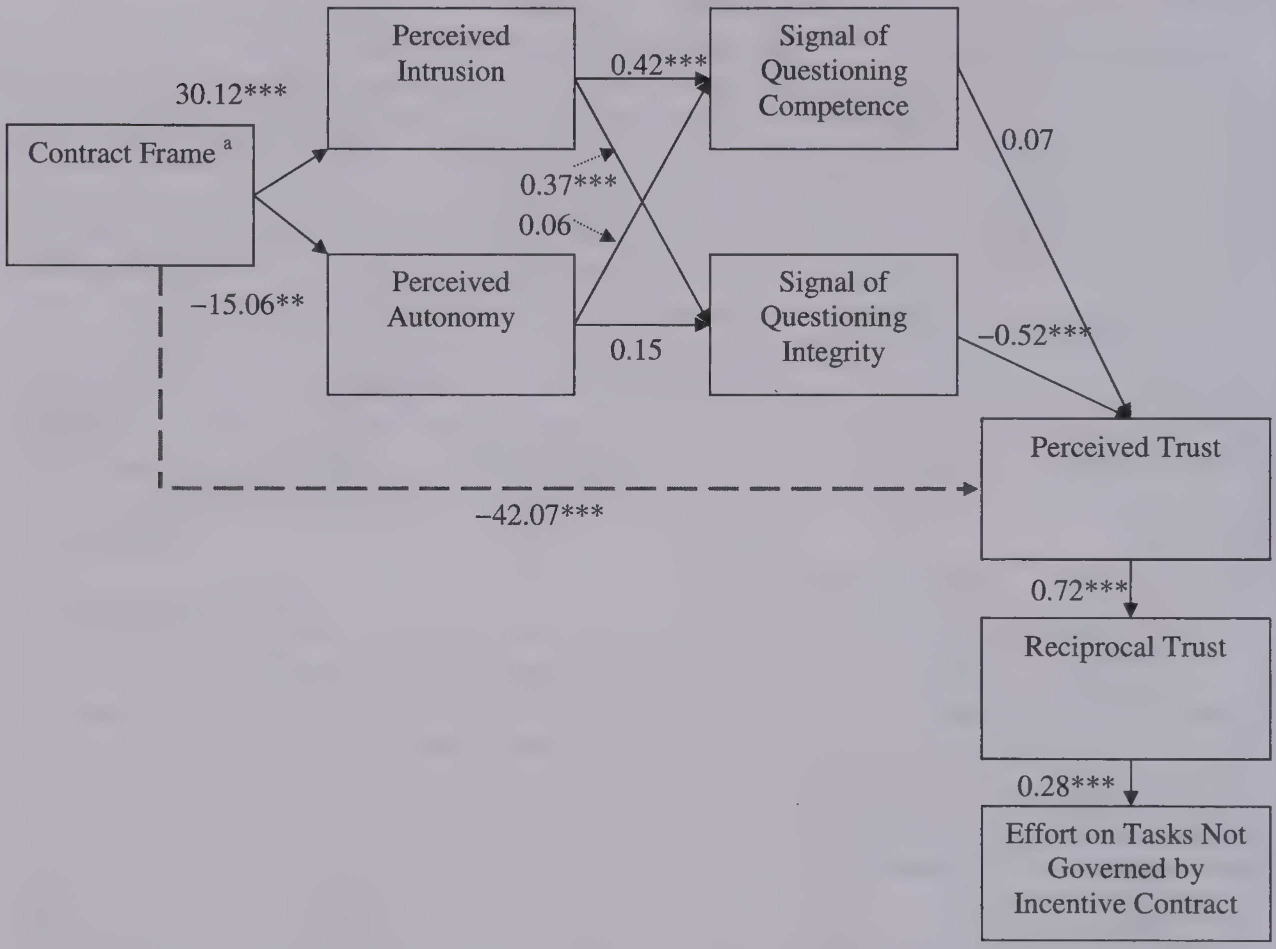
[17] Affect is a response to a stimulus that presents as either a positive or negative mood or emotional reaction (Fiske and Taylor 1991). While, to our knowledge, prior research does not examine an affective reaction to control system implementation, research in accounting does document that affective reactions can have significant effects on capital budgeting (Kida et al. 2001; Moreno et al. 2002; Sawers 2005) and inventory valuation decisions (Chung et al. 2008).

[18] The effect of contract frame on perceived trust flows through the signal that the principal is questioning the agent's integrity rather than the agent's competence. This is likely because this study examines incentive contracts, which are a form of outcome control. Formal controls can affect behavior by providing both motivation and direction (Merchant and Van der Stede 2007). Outcome controls, while being an effective motivational tool, are less useful for providing direction than are behavior controls. Therefore, outcome controls, regardless of whether they are positively or negatively framed, are less likely than behavior controls to be interpreted as a signal that the principal is questioning the agent's competence.

FIGURE 3
Test of Full Model: Comparison of Penalty and Bonus Contract Conditions



,* Indicate significance at the 0.05 and 0.01 levels, respectively (one-tailed).

This figure shows the results of the path analysis. The standardized path coefficient and corresponding one-tailed significance are shown next to each path. Goodness of fit is measured through the Comparative Fit Index (0.965), which is above the generally accepted minimum value of 0.95 (Byrne 2001) and confirmed with a traditional Chi-square test ($\chi^2 = 10.76$, $p = 0.71$), an Incremental Fit Index (1.02), and a Root Mean Square Error of Approximation (0.00).

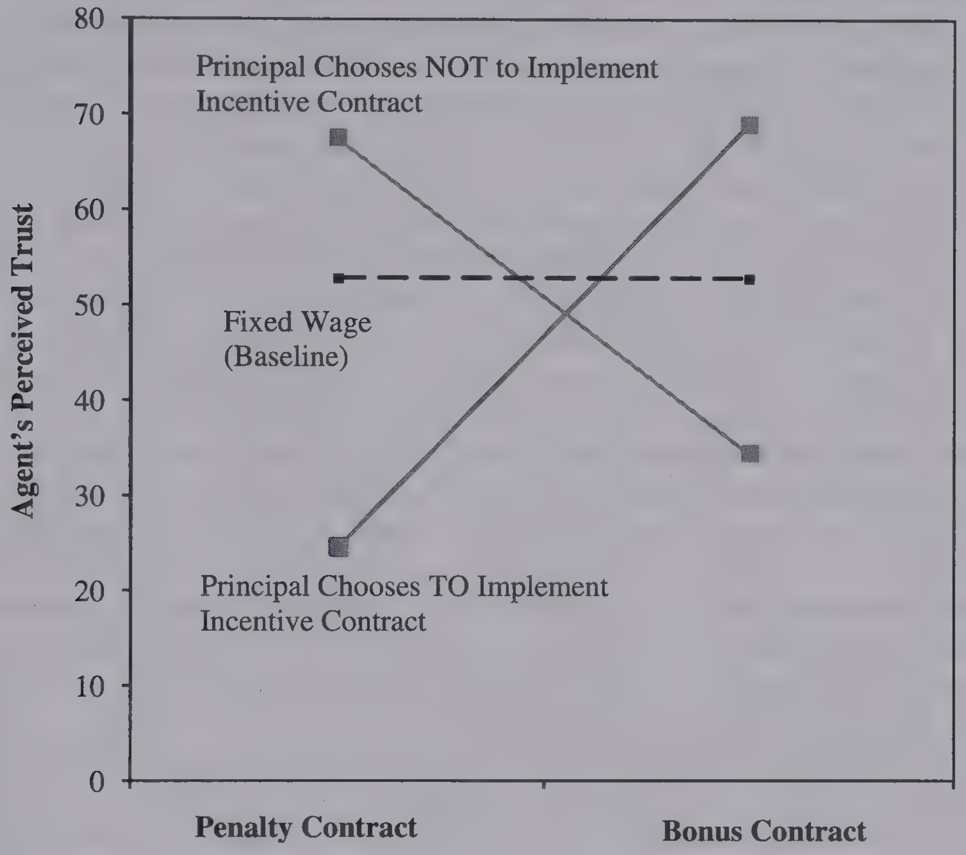
[a] The data used in this analysis include observations only from the *penalty* and *bonus contract* conditions when the principal chose to implement the incentive contract. Contract Frame assumes a value of 0 for the *bonus contract* condition, and 1 for the *penalty contract* condition.

The results also show that trust is reciprocal. As the agent's Perceived Trust increases, his or her Reciprocal Trust for the principal also increases (0.72, $p < 0.01$). Finally, when we test the link between the agent's Reciprocal Trust and the agent's Effort on Task 2, we find the expected positive relation between the agent's Reciprocal Trust and the agent's Effort (0.28, $p < 0.01$). The general path from Contract Frame to Perceived Trust is negative and significant (−42.07, $p < 0.01$), documenting a negative effect on trust of a penalty contract as compared to a bonus contract, incremental to the signaling effect we hypothesize. While not the primary focus of our study, we expect that this path may reflect a more negative affective reaction to the implementation of the penalty contract as compared to the bonus contract.

In sum, consistent with our expectations, the penalty contract has a negative effect on the trust environment, and this effect spills over to tasks not governed by the incentive contract, such that

FIGURE 4

Supplemental Test of Agent's Perceived Trust under the Bonus and Penalty Contract Conditions versus the Fixed Wage (Baseline) Condition



This figure shows the average agents' Perceived Trust across conditions and across the decisions of principals to implement or not implement the incentive contract. The average Perceived Trust in the *fixed wage condition* (in which the principal was not given the opportunity to implement an incentive contract) is represented by the horizontal line labeled "Fixed Wage (Baseline) Condition." The other two lines on the graph represent the Perceived Trust reported by agent participants in the *penalty* and *bonus contracts* conditions. Specifically, the positively sloped line represents agents' Perceived Trust when principals chose to implement the contract, and the negatively sloped line represents agents' Perceived Trust when principals chose not to do so.

bonus contracts elicit greater effort on these tasks as compared to penalty contracts. This effect occurs both through the signaling effect we posit, as well as through an additional link that captures other mechanisms, such as perceived unfairness or negative affect. Thus, contrary to prior research on contract framing, we document that in an incomplete contract setting when the agent is vulnerable to the principal's decisions and, therefore, must exhibit trust when determining his or her effort level, penalty contracts lead to diminished effort.

Supplementary Tests

Comparison to Fixed Wage (Baseline) Condition

In our *fixed wage* condition, where no incentive contract is available for implementation, the average Perceived Trust is 52.83 on a 100-point Likert scale, which we represent in Figure 4 by the horizontal dashed line. The other two lines on the graph represent the Perceived Trust reported by agent participants in the *penalty contract* and *bonus contract* conditions. The positively sloped line represents agents' Perceived Trust when principals chose to implement an incentive contract, and the negatively sloped line represents agents' Perceived Trust when principals chose not to do so.

Because our primary interest is in examining the effects of contract frame on trust and effort, our main analysis compares the two points on the upward-sloping line. That is, we compare the situation in which the principal has chosen to implement a bonus contract to the situation in which he or she has chosen to implement a penalty contract. We include the *fixed wage* condition for benchmarking purposes. Our analyses ignore the points on the downward-sloping line, which reflects the principal's choice to not implement an incentive contract, for two reasons. First, in typical business settings, employees would not be aware of an explicit choice to not implement an incentive contract. Second, a practical reason for ignoring the downward-sloping line is that the number of observations is quite low. Recall that these observations are the byproduct of the principal's endogenous choice of whether to implement an incentive contract; a choice crucial to the experimental design (cf. Christ 2012). However, because only seven principals in the *bonus contract* condition and 24 principals in the *penalty contract* condition chose to not implement the contract, any conclusions drawn from these observations would be unreliable.

By including the *fixed wage* condition, we are able to compare our results to prior studies examining the effects of formal control on trust (e.g., Das and Teng 1998, 2001; Enzle and Anderson 1993; Malhotra and Murnighan 2002). As previously described, this literature generally finds a negative relation between formal control and trust, suggesting that there are unintended consequences to implementing a formal control; namely, the degradation of the trust environment, which ultimately diminishes firm performance. However, this literature focuses on negatively framed formal controls, such as a penalty contract or audits of performance, and has not considered the effects of positively framed formal controls, such as a bonus contract.

Consistent with the prior literature, we find that relative to the *fixed wage* condition, the *penalty contract* condition results in lower levels of both Perceived Trust and Reciprocal Trust (see Table 1), and all contrasts are statistically significant at $p < 0.01$ (untabulated). Thus, we confirm that penalty contracts, which are negatively framed formal controls, do have unintended consequences of diminished trust. However, Perceived Trust is significantly higher in the *bonus contract* condition than in the *fixed wage* condition, at $p < 0.05$ (untabulated). There is no statistically significant difference between the *bonus contract* and *fixed wage* conditions for Reciprocal Trust. The results of these comparisons indicate that contrary to prior research, which did not differentiate between control frames, when *positively* framed formal controls are imposed, there is no degradation (and in the case of Perceived Trust, there is actual enhancement) of the trust environment.

Principal Implementation Decisions

While our main goal is to examine the effects of contract frame on the trust environment and agent effort, our data also provide insight into the contract implementation decisions of the principals. Principals appear to strategically anticipate the effect of their contract choice on agent behavior. Specifically, 78 percent (25 of 32) of principals in the *bonus contract* condition chose to implement the contract, compared to 49 percent (24 of 49) in the *penalty contract* condition. Thus, principals are more likely to choose to implement the Task 1 incentive contract when doing so is more likely to lead to higher effort in Task 2 (untabulated, $F = 6.34$, $p < 0.05$).

V. DISCUSSION AND SUGGESTIONS FOR FUTURE RESEARCH

Organizations implement incentive contracts to induce desirable employee effort. While incentive contracts framed as bonuses and penalties are found in practice, bonus contracts are much more prevalent. Accounting research examining the effects of contract frame finds that despite employees' stated preferences for bonus contracts, employees exert greater effort under penalty contracts, resulting in greater firm profitability when penalty contracts are employed (Hannan et al. 2005; Hossain and List 2012). Thus, there has been a disconnect between the dominance of bonus contracts in practice with these prior research findings. Importantly, we note that prior literature on contract framing assumes a complete contract setting. In practice, employment contracts are often incomplete, leaving some components of employee pay to the discretion of the principal. In such incomplete contract settings, an agent's trust in his or her principal is an important determinant of effort.

This study examines the effect of contract framing on effort in an incomplete contract setting and, thus, considers trust as a mediating variable. We find evidence that for a task not governed by the incentive contract, agents exert greater effort under bonus contracts relative to penalty contracts. Analysis of a comprehensive theoretical model we developed based on the formal control/trust literature reveals that the relation between contract frame and effort depends on the level of trust induced by the implementation of a bonus or penalty contract.

The results of this study contribute to the incentive contract framing literature and help explain why bonus contracts are more common in practice than penalty contracts, despite evidence that penalty contracts elicit greater effort. Our findings suggest that it is important to consider both the bonus versus penalty contract frame and the completeness of the contract when designing incentive compensation plans.

The results of this study also contribute to a second stream of literature examining the effects of formal controls, such as incentive contracts, on trust. Studies in this stream of literature often conclude that formal controls have unanticipated consequences of deteriorating the trust environment, resulting in lower effort. However, this literature has focused primarily on negatively framed formal controls, such as penalty contracts, but has not considered the effects of positively framed formal controls, such as bonus contracts. Our results reveal that the frame of the formal control influences whether the control is perceived by the agent to be a signal of distrust and affects agent behavior.

The results of this study suggest several avenues for future research. First, this study and several prior studies on the effects of incentive contract framing (e.g., Luft 1994; Hannan et al. 2005) analyze the various mechanisms through which framing may influence agent behavior. However, because each study focuses on a different mechanism, it is unclear when the effect of one mechanism might outweigh the others and when managers should implement a bonus versus a penalty contract. Future research should explore these mechanisms further to determine in which settings the effect of each mechanism is likely to dominate.

Second, although the principals in our study choose whether or not to impose an incentive contract, the framing of the contract available to the principal in each condition is determined exogenously by the experimenters. In practice, principals choose whether to frame an incentive contract as a bonus or penalty. It is possible that the endogeneity of the framing decision might moderate agents' response to it, resulting in more positive or more negative reactions to an endogenously chosen bonus or penalty contract. This should not pose a threat to the validity of our study, because it works against our finding results. However, future research should consider both the agent's response to the contract frame chosen by management, as well as the factors that the principal considers when choosing the contract frame. Similarly, research examining the characteristics of principals who choose bonus versus penalty contracts may be fruitful. For example, are certain types of principals more likely to be trusting, and do they, therefore, exhibit a consistent preference for bonuses or penalties?

Third, we document a somewhat surprising result that the implementation of a bonus contract positively influences perceptions of being trusted, separate and distinct from the signaling effect we hypothesize. While not the focus of this study, the reasons for this result are worthy of additional study. One possibility is that agents' preference for fairness results in a favorable reaction to the bonus contract. A second possibility is that agents have a general affective reaction to this type of formal control. Managers often rely on affective evaluations as a basis for making judgments (Schwartz 2002). This "affect heuristic" may moderate the effect of contract framing on perceived trust. Our results may suggest that, incremental to the negative signaling effect we posit, negatively framed formal controls create negative affect, deteriorating the trust environment. Positively framed controls, on the other hand, appear to induce positive affect, leading to greater trust. The investigation of this result is a promising area for future research.

Fourth, our experiment constitutes a one-shot trust game. Although the results show that effort is highest when a bonus contract is implemented, it is unclear whether this benefit would remain if the principal/agent pairs engaged in repeated trust games. Future research could investigate whether contract frame continues to influence effort even after the principal has had the opportunity to take advantage of the agent's trust.

Finally, our model is based on agents' beliefs regarding principals' motivation for implementing the incentive contract. However, prior research suggests that if agents do not interpret the implementation of the formal control as a signal of distrust, they will not respond negatively (Christ 2012). Therefore, future research should consider whether the effects of the bonus and penalty contracts found in the current paper hold if the principal is able to credibly signal that he or she trusts the agent.

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APPENDIX A
EXPERIMENTAL INSTRUMENT

General Instructions for All Conditions

INSTRUCTIONS

Participant #: ___________

GENERAL

You are about to participate in a study on decision making. Please read these instructions carefully, because the amount of money you earn will depend in part on your decisions. Also, there will be several short quizzes on these instructions to ensure your understanding, and you will not be able to continue until you accurately complete the quizzes, so please pay close attention.

If you have any questions, you should first read back through the instructions. If you continue to have questions after all the instructions are read, please raise your hand and the Administrator will answer you in private.

Unless you are asking a question to the Administrator in private, **please do not talk** at all, during this experiment.

OVERVIEW OF THE STUDY

This is a computerized decision making study. We expect the entire session to last approximately 60 minutes, during which time you will be required to answer questions and make decisions.

In this study, you will assume the role of either Participant A or Participant B. You will remain in the same role throughout the entire study. We will tell you which role you will assume after you read the first set of instructions.

Before you begin, the computer will randomly match participants into pairs (of one Participant A and one Participant B). The pairings are anonymous. You will not be told who you are paired with either during or after the study.

You will earn points, which can be affected by decisions made by you and/or the person with whom you are paired. These points will be converted to cash and you will be paid the cash amount before you leave today. Points will be converted to cash using the following formula:

US \$ Payment = (Points earned X .025)

Task 1 Overview for [Bonus Contract] [Penalty Contract] [Fixed Wage] Conditions

Initial Decisions

Overview

As a first step, Participant A will have the task of selecting a portfolio of investments for Participant B. That is, Participant B will earn the returns from the portfolio, but Participant A will choose the specific investments to be included in the portfolio. Participant A will also pay for the investments, but will be given a fund from which to pay. More specifically, at the start of the study,

- **Participant A** will be given [850] [1,000] [1,000] points, and
- **Participant B** will be given 250 points.

Participant A's initial fund is larger, because Participant A must pay for the investments and Participant B will receive the returns from the investments.

The following instructions will describe in detail the decisions made by Participant A and Participant B. Because you do not yet know to which role you will be assigned, it is important for you to pay close attention to both sets of instructions.

Task 1 Agent Investment Decision Description for All Conditions

Participant A

Participant A has the task of selecting the portfolio of investments. Participant A will be presented with 30 possible investments. From these 30 investment choices, Participant A ***must select and pay for 1 share each of 10*** of these investments, which will earn points for Participant B. Participant B will hold the shares for one period only, and so Participant B's returns will be maximized if Participant A selects the 10 shares that are expected to make the highest returns in the next period.

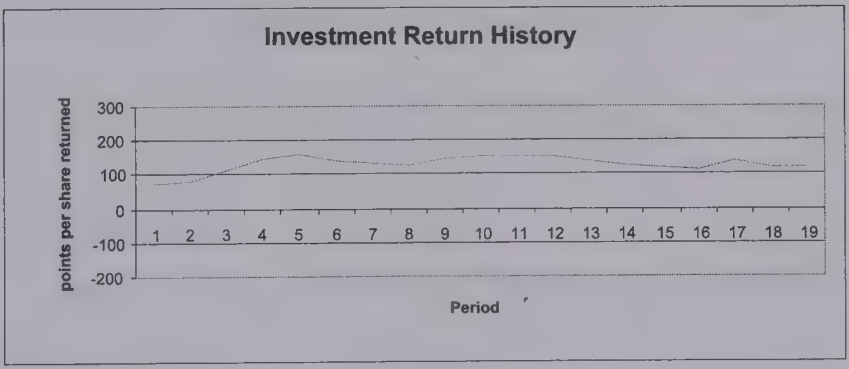
NOTE: Participant A must select 10 *different* investments, and buy exactly *one share* of each of these investments.

There are two types of investments, 🔔 (bell) investments and 📯 (whistle) investments. The cost per share depends on the investment type:

- Each 🔔 (bell) share selected will cost Participant A 10 points.
- Each 📯 (whistle) share selected will cost Participant A 25 points.

📯 (whistle) shares are more expensive, because on average they are expected to return more per share in the next period. Specifically, 🔔 (bell) shares will earn an average return of 20 points per share, whereas 📯 (whistle) shares will earn an average return of 50 points per share. Importantly, this is only an average, and any individual investment can earn considerably more or less than the average (as you can see by looking at the graphical representations of the investment return history, which are provided to you on the large, folded documents enclosed, and described below).

Both participants will be provided with a graphical representation of the previous 19 periods of returns (points per share) for each of the 30 possible investments. For example, the history of points per share returned for one investment available for selection might look like this:



[Below for Bonus Contract and Penalty Contract Conditions Only]
Participant B will have the opportunity to implement a control system prior to Participant A's investment decision. The control system, if implemented, establishes a [bonus] [penalty] system for Participant A in which Participant A will [receive an extra 150 points if the total return of all shares chosen by Participant A meets or exceeds 500 points] [be required to pay 150 points if the total return of all shares chosen by Participant A is less than 500]. [No bonus points would be paid to Participant A if total return is less than 500 points.] [No penalty points would be paid if total return equals or exceeds 500 points.] The [bonus] [penalty] will be paid [from] [to] the administrator's fund. That is, the [bonus] [penalty] will not be [deducted from] [added to] Participant B's fund.

After Participant B decides whether or not to implement a control system, Participant A will be notified of this decision.

Task 1 Summary of Participant A Points Earned for [Bonus Contract] [Penalty Contract] Conditions

Summary of Points – Participant A

Participant A will receive an initial fund of [850] [1,000] points. From this fund, Participant A will pay for the 10 shares selected (10 points for each 🔔 (bell) share and 25 points for each 📯 (whistle) share). Further, the points will depend on whether or not Participant B chose to implement a control system and, if a control system has been implemented, the total return of the shares chosen by Participant A. Thus, the payoffs to Participant A for this initial decision are as follows:

	No control system	Control system
Total Share Returns < 500	[850 points - Cost of Shares] [1,000 points - Cost of Shares]	[850 points - Cost of Shares] [1,000 points - Cost of Shares - 150 penalty points]
Total Share Returns >= 500	[850 points - Cost of Shares] [1,000 points - Cost of Shares]	[850 points - Cost of Shares + 150 bonus points] [1,000 points - Cost of Shares]

For example, suppose Participant B implements the control system and Participant A chooses 6 🔔 (bell) shares and 4 📯 (whistle) shares. Also assume that the returns for these investments [exceed] [are less than] 500 points. Participant A's payoff will equal [850 – (6 X10) – (4 X 25) + 150 = 840 points] [1000 – (6 X10) – (4 X 25) - 150 = 690 points].

If Participant B does not implement the control system, or the returns for the investments [are less than] [exceed] 500, Participant A's payoff will equal [850-(6X10) – (4 X 25) = 690] [1000-(6X10) – (4 X 25) = 840].

Task 1 Summary of Participant A Points Earned for Fixed Wage Condition

Summary of Points – Participant A

Participant A will receive an initial fund of 1000 points. From this fund, Participant A will pay for the 10 shares selected (10 points for each 🔔 (bell) share and 25 points for each 📯 (whistle) share).

For example, suppose Participant A chooses 6 🔔 (bell) shares and 4 📯 (whistle) shares. Participant A's payoffs will equal: 1000 – (6 X10) – (4 X 25) = 840 points.

Task 1 Principal Contract Implementation Decision Description for [Bonus Contract] [Penalty Contract] Conditions (omitted for Fixed Wage condition)

Participant B

Participant B will receive the total return from the 10 (ten) shares chosen by Participant A.

As described in the instructions for Participant A, Participant B will have the opportunity to implement a control system prior to Participant A's investment decision. The control system, if implemented, establishes a [bonus] [penalty] system for Participant A.

If Participant B chooses to implement a control system, the cost to Participant B is 50 points.

Task 1 Summary of Participant B Points Earned for [Bonus Contract] [Penalty Contract] Conditions

Participant B

Participant B will receive the total return from the 10 (ten) shares chosen by Participant A.

As described in the instructions for Participant A, Participant B will have the opportunity to implement a control system prior to Participant A's investment decision. The control system, if implemented, establishes a [bonus] [penalty] system for Participant A.

If Participant B chooses to implement a control system, the cost to Participant B is 50 points.

Summary of Points – Participant B

The total number of points that **Participant B** earns will depend on the total return of the shares chosen by Participant A, and whether or not Participant B chose to implement the control system. Thus, the payoffs to Participant B are as follows:

	No control system	Control system
Total Share Returns< 500	250 points + total share returns	250 points + total share returns - 50 points for cost of control system
Total Share Returns >= 500	250 points + total share returns	250 points + total share returns - 50 points for cost of control system

Task 1 Summary of Participant B Points Earned for Fixed Wage Condition

Summary of Points – Participant B

The total number of points that **Participant B** earns will depend on the total return of the shares chosen by Participant A. Specifically, Participant B will earn 250 points, plus the total returns from the shares chosen by Participant A. That is:

Participant B points = 250 points + total return on shares chosen by Participant A

Task 2 Overview for All Conditions

CONTINUING DECISIONS

Overview

You are still in the same role as before – either Participant A or Participant B.

In addition, you are paired with the same person. The identification of participants will continue to be anonymous throughout the study.

You will start this part with whatever points you have in your fund following the prior set of decisions. (Note that you don't know the amount exactly, because we have not told you the returns from the shares selected by Participant A. However, each Participant A still has a minimum of 600 points with which to make investments.)

You will now have the opportunity to earn more points based on the choices made by you and the person with whom you are paired.

Participant A

Participant A now has the task of deciding on one additional investment for Participant B. This investment decision is different from the prior investment decisions in several important ways:

1. There is only one type of investment: a 📯 (horn) investment.
2. Each 📯 (horn) share costs 10 points (whereas in prior decisions, some investments cost 10 points and some cost 25 points).
3. The return per share in the next period is known to be 30 points (whereas in the prior decisions, the return per share in the next period was unknown, and had to be predicted based on the history of past returns).
4. Participant A can purchase anywhere from 0 to 50 shares of the 📯 (horn) investment (whereas in the prior decisions, Participant A could only select one share each of 10 different investments).
5. Participant B will earn the returns from the portfolio. ***However, Participant B will have the opportunity to share the return from the investment with Participant A (whereas in the prior decisions, Participant B kept the total return).***

Participant B

After learning how many shares Participant A purchased, **Participant B** will be given the opportunity to pay any amount of the return to Participant A. The return will equal 30 points X the number of shares of the 📯 (horn) investment purchased by Participant A, and Participant B can share (i.e., give back) anything from 0 to the total return with Participant A.

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Analysts' Motives for Rounding EPS Forecasts

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ABSTRACT: We investigate analysts' motives for rounding annual EPS forecasts (placing a zero or five in the penny location of the forecast). We first show that an intuitive reason for analysts to engage in rounding is in circumstances where the penny digit of the forecast is of less economic significance. By rounding, analysts reveal that their forecasts are not intended to be precise to the penny. We also show that analyst incentives impact the likelihood of rounding. Specifically, we predict that analysts will exert less effort forecasting earnings for firms that generate less brokerage or investment banking business since such firms create less value for the analysts' employers. As a consequence of this reduced effort and attention, the analyst will be more uncertain about the penny digit of the forecast and so will round. Our results are consistent with this prediction. One implication of our findings is that a rounded forecast is a simple and easily observable proxy for a more noisy measure of the market's expectation of earnings. Consistent with this implication, we show that rounded forecasts bias down earnings response coefficients at earnings announcements.

Keywords: *rounding; analyst effort; brokerage; investment banking; earnings forecasts; earning response coefficient.*

I. INTRODUCTION

Rounding a number is replacing it with another value that is approximately equal but is a shorter or a simpler representation. Rounded numbers require less cognitive effort to remember and communicate. As a result, humans exhibit a strong psychological preference for rounded numbers. This phenomenon, known as "heaping," has been documented in the psychology, finance, and statistics literatures (e.g., Huttenlocher et al. 1990; Harris 1991; Brown et al. 1998; Roberts and Brewer 2001; Pope and Simonsohn 2011).

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Financial analysts appear to exhibit a similar preference for rounded numbers when they forecast earnings. Herrmann and Thomas (2005) document that approximately 55 percent of annual EPS forecasts on I/B/E/S from 1985 to 2001 end with a zero or five in the penny location (the unconditional expected percentage should be around 20).[1] Given that earnings per share is a widely used input in equity valuation and investment decision making, it would seem that the economic importance of a precise forecast would dwarf the benefits of providing a rounded number that is easy to remember. Therefore, cognitive ease is unlikely to be the primary reason for rounding by analysts. An alternative explanation is that humans will round a digit when they are uncertain or unconfident about the exact numerical value of that digit. In such cases rounding implicitly signals the lack of precision or accuracy. For example, in the handbook for real estate appraisers, *The Appraisal of Real Estate*, the Appraisal Institute (2008) states that:

> Customarily appraisal conclusions are rounded to reflect the lack of precision associated with value opinions. The more confidence the appraiser has in the opinion, the less need there is for rounding.

Analysts' earnings forecasts are similar to real estate appraisals in that the analyst faces uncertainty when forecasting future earnings. If an analyst lacks confidence that the precision of the forecast holds to the penny digit, then his or her choice is either to: (1) round the forecast to the level of precision with which the analyst feels comfortable (e.g., the nearest dime or quarter) or (2) arbitrarily assign a number to the penny location. Note that decision (1) will automatically lead to a rounded forecast. Decision (2) could also lead to a rounded forecast if the analyst is subject to the behavioral bias of "heaping" when arbitrarily assigning a number.

Herrmann and Thomas (2005) provide evidence that rounded forecasts are less accurate than other forecasts. They suggest that the reason for the lower accuracy is that rounding analysts are less informed/have less information than other analysts. Their tests focus on *analyst characteristics* and they investigate whether rounding analysts work for smaller brokerage houses, have poorer past accuracy, are following more firms from more industries, forecast less frequently, have longer forecast horizons, and have been following the firm for a shorter period of time.

In this paper we adopt a different but complementary approach to Herrmann and Thomas (2005). Specifically, we argue that analysts can *choose* to be less informed about a particular stock and this choice affects how certain they feel about their forecasts, and as a consequence, their decision to round. In other words, analysts can rationally decide not to provide a forecast precise to the penny even when the information to help them do so is readily available.[2] This is because exerting effort to provide more precise forecasts is costly to analysts.[3] Analysts have limited time and so will rationally focus their forecasting efforts on firms for which the benefits of doing so are the highest. Our tests therefore focus on whether the *followed firm attributes* impact analysts' decision to round.

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[1] In the U.S., the convention is to forecast EPS in dollars and cents. Therefore, we assume that an analyst is more likely to have engaged in rounding when the forecast has a 0 or 5 in the penny location.

[2] For example, an analyst following IBM may know that reading the 10K and learning more about the industry will improve his confidence and his ability to provide a precise EPS forecast. However, even with this information readily available, due to the opportunity cost of the time and effort required to read and understand the information, the analyst may choose not to become more informed (e.g., because he would like to become the lead analyst for Google and so is focusing his time and effort on Google).

[3] Placing a number other than 0 or 5 in the penny location of a forecast does not physically require more effort on the part of the analyst. The point is that when an analyst exerts less effort in forecasting, he will be less confident and be more uncertain about his forecast. This lack of confidence and greater uncertainty results in an increased likelihood of rounding.

Our first test examines an intuitive reason for analysts to round. As earnings per share increases from one digit (e.g., 0.05) to two (0.55), three (1.55), or four digits (15.55), the economic importance of the penny digit to investors in valuing the firm declines. Hence, an analyst is less likely to exert effort to obtain "to-the-penny" precise information to forecast larger EPS with four digits than smaller EPS with fewer digits. Furthermore, other things being equal, it is also more difficult and requires more effort for an analyst to forecast the penny digit accurately for four-digit EPS than others with fewer digits.[4] As a result, we would expect the uncertainty that an analyst faces in determining the penny digit of his forecast to increase with the magnitude of the forecast. This will also lead an analyst to round more for EPS forecasts with larger magnitude.

We show that about 34 percent of forecasts are rounded for stocks where forecast EPS is less than \$1, and this increases to 49 percent for stocks where forecast EPS is between \$1 and \$10 and is over 60 percent for stocks with EPS forecasts over \$10.00. In addition, we find that the incidence of the number 5 increases to around 20 percent for EPS forecasts up to \$1.00 and then remains steadily at 20 percent for higher levels of EPS. These findings suggest that the high prevalence of rounding observed by Herrmann and Thomas (2005) is at least partly due to analysts rationally choosing to round as the economic significance of the penny location declines.

Our second and third tests examine whether analysts' compensation incentives impact the decision to round. We expect analysts to put more effort into providing an accurate and informative forecast when there is more monetary benefit for them from doing so. Part of an analyst's job is to provide research to clients that will generate interest in the stock and encourage trading through the brokerage division (Irvine 2000; and Jackson 2005). More trading activity results in greater brokerage fees for the brokerage division and more bonus compensation for the analysts (Groysberg et al. 2008). Prior research suggests that brokerage fees are positively correlated with trading volume, stock price volatility, institutional holdings, and growth opportunities (e.g., Frankel et al. 2006). We predict that analysts will round more in firms that generate less brokerage fees since there is less benefit to exerting effort to obtain precise information. Our results are consistent with this prediction.

Our third test examines whether there is a relation between rounding and external financing. Corporate financing is an important source of revenues for many investment banks (Lin and McNichols 1998; Michaely and Womack 1999; Bradshaw et al. 2006). A firm that is followed by analysts and is seeking to raise more external financing is likely to be viewed by the investment bank as having greater revenue-generating potential. In helping their employers attract and compete for underwriting business, analysts are likely to exert more effort in forecasting earnings for potential investment banking clients. We therefore predict that analysts will round less when forecasting earnings for firms raising more external financing. Our results are consistent with this prediction.

Our fourth test examines the impact of firm complexity on analysts' tendency to round. Large companies and companies with more complicated business models require the analyst to acquire more information and exert more effort in understanding the business. All else equal, this would result in a greater propensity to round. However, the demand for analysts' services by investors is likely to be higher for larger, more complex firms. This could result in greater analyst effort, and so, in less rounding. Our results indicate that rounding increases both with size and the number of

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[4] For example, when earnings equal \$1,543 and the number of shares is 100, then EPS is four digits (15.43). If the number of shares outstanding increases to 10,000, then EPS is two digits (0.15). The penny digit of the forecast is accurate to the nearest dollar of total earnings for the four-digit forecast but to the nearest hundred dollars for the two-digit forecast. It is likely more difficult and therefore requires more effort for an analyst to forecast the value of earnings to be \$1,543 than to be around \$1,500.

business segments. These results suggest that, on balance, analysts are more likely to round when following larger, more complex firms.

Our findings complement and build on Herrmann and Thomas (2005). In their research they determine whether rounding analysts are less informed than other analysts following the same firm. Thus, their approach controls for firm characteristics. In contrast, we argue that *firm characteristics* affect analyst incentives and their tendency to round. We find that: (1) the explanatory power of firm characteristics is about three times greater than that of analyst characteristics; and (2) firm characteristics have significant incremental explanatory power over analyst characteristics in explaining rounding.

Our final test examines one implication of rounding. Prior research shows that earnings response coefficients are biased down when there is more noise in proxies for the market's expectation of earnings due to a classic "errors in variables" problem (Imhoff and Lobo 1992; Burgstahler and Chuck 2010; Kim 2011). If investors understand that rounded forecasts contain more noise, then they should place a lower weight on these forecasts in forming their expectations. As a result, rounded forecasts will be a noisier proxy of the market's expectation and will have lower earnings response coefficients than other forecasts. Our results are consistent with this prediction.

Our results also show that rounded forecasts are not only less accurate, but also more optimistically biased than other forecasts. Hence, we also examine whether investors adjust for this optimistic bias in their expectations of earnings. We find that, on average, rounded forecasts have a small but significantly lower announcement return than other forecasts, suggesting that investors may not fully adjust for the optimistic bias associated with rounded forecasts.

The next section provides a discussion of prior research. Section III develops our predictions. In Section IV we provide our sample selection. Section V investigates the determinants of rounding and investor response to rounded forecasts. Section VI concludes.

II. PRIOR RESEARCH

Several avenues of research in accounting and finance have examined whether investors, managers, or analysts focus or herd to specific numbers. For example, Carslaw (1988) finds that there are more zeros and fewer nines than would be expected by chance in the second-from-left-most digit in reported earnings for New Zealand firms. Thomas (1989) shows similar unusual patterns for reported earnings for U.S. firms covered in the Compustat database. Furthermore, he documents a greater propensity of zeros and fives in the third digit of EPS numbers for the sample. Das and Zhang (2003) confirm Thomas's (1989) findings and extend the research by showing that the unusual frequency of rounded EPS reflects managers' incentive to round up EPS to meet analysts' forecasts, report positive profits, and sustain recent performance. A more recent study by Bamber et al. (2010) identifies rounding patterns in managers' EPS forecasts. Their findings suggest that nearly half of managers' annual EPS forecasts end in nickel intervals. They argue that managers' nickel forecasts appear to reflect managers' efforts to protect proprietary information and are due to self-serving opportunism.

The finance literature presents similar rounding phenomena in the distribution of stock prices on the U.S. equity markets (e.g., Osborne 1962; Niederhoffer 1965, 1966; Harris 1991; Christie and Schultz 1994; Godek 1996; Grossman et al. 1997; Cooney et al. 2003; Johnson et al. 2008). Harris (1991) shows price clustering increases with price level and volatility and decreases with capitalization and transaction frequency. These results are subsequently confirmed and other implications are investigated concerning the desirability of certain numbers in international stock markets (e.g., Aitken et al. 1996; Brown et al. 2002; Ahn et al. 2005; Sonnemans 2006; Brown and Mitchell 2008).

The paper closest to ours is Herrmann and Thomas (2005), which documents a high prevalence of rounding in analyst annual EPS forecasts. They argue that rounding will occur when an analyst is less informed and so rounded forecasts will be less accurate. They use prior research examining analyst characteristics that are correlated with accuracy to predict which analysts are more likely to round.[5] They compare rounding analysts to non-rounding analysts following the same firm and, consistent with their predictions, document that rounding analysts: work for smaller brokerage firms; have poorer past accuracy; update their forecasts less frequently; have longer forecast horizon; and follow more firms from more industries. However, their research also raises questions as to whether analysts' characteristics are a complete explanation for rounding. First, they note in the text that rounding is more common in transportation, public utilities, and energy (Herrmann and Thomas 2005, 820). It is unclear why less-informed analysts would gravitate to these industries. Second, Herrmann and Thomas (2005) document that rounding analysts have been following firms for a longer period of time. Again, it is unclear why an analyst would become less informed, the longer he/she follows a firm. Herrmann and Thomas (2005) recognize these concerns and suggest that future research probe these issues in more detail.

In our paper we conjecture that analysts rationally trade off the benefits and costs of providing informative and precise forecasts, and that these trade-offs depend on *firm characteristics*. In taking this perspective we are able to generate predictions that are not implications of an *analyst characteristics* perspective. For example, we predict that rounding will increase with the level of EPS. We can see no reason why analyst characteristics would vary with the level of EPS. In addition, if lack of expertise and information were the main drivers of rounding, then one might expect rounding to occur more frequently in difficult-to-forecast stocks such as those with high-growth expectations and those that have high earnings and stock price volatility. In contrast, we argue that although these stocks are difficult to forecast, the demand from investors for more information is greater. Thus, we predict less rounding in these stocks.

Our paper also contributes to prior research that analyzes the informativeness of analyst research reports. Prior research has used the stock market reaction to analyst reports as a proxy for the informativeness of the report (see Frankel et al. 2006). However, the use of this proxy is complicated by a number of factors.[6] In contrast, we conjecture that rounded forecasts can be used as an easy-to-observe proxy for a "noisier" forecast. We therefore do not rely on the stock market response to determine the "informativeness" of the EPS forecast. Instead, we can examine directly how investors respond to a less informative report, as proxied by a rounded forecast, as an implication of our findings.

III. PREDICTIONS

We predict that rounding will be influenced by firm characteristics. These characteristics include the level of EPS and the value of the firm for creating brokerage and investment banking business, as well as the complexity of the business. Herrman and Thomas (2005) suggest that analyst characteristics influence rounding and so we include predictions for these variables as well. Finally, we provide our predictions relating to earnings response coefficients.

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[5] The link between forecast accuracy and analyst characteristics has been investigated by Mikhail et al. (1999), Clement (1999), Jacob et al. (1999), Brown (2001), and Clement and Tse (2003).

[6] First, the researcher must assume that markets are efficient and that investors do not ignore or under- or overreact to analyst reports. Second, there are endogeneity issues that need to be addressed since some of the determinants of the stock market reaction to the report are also likely to be affected by whether analyst reports are informative (O'Brien and Bhushan 1990; Alford and Berger 1999; Frankel et al. 2006).

The Level of EPS (Predicted Sign: +)

The convention in the U.S. is for analysts to forecast EPS to the nearest penny. This contrasts with forecasts in the U.K. and Australia that regularly report fraction of a penny forecasts. Thus, in the U.S., an analyst forecasting EPS that is less than $1 need only forecast two digits (i.e., 0.xx), while a U.S. analyst forecasting EPS over $1 will forecasts three (x.xx) or even four digits (xx.xx). *Ceteris paribus*, forecasting EPS to four digits requires more effort than forecasting EPS to two digits. In addition, as the level of EPS increases, the economic importance of the penny digit declines, and so the demand for "to the penny precision" by investors is likely to decline. Analysts are likely to round more as the magnitude of EPS increases either because they round to the digit with which they are most confident, or because, when arbitrarily assigning a value to the digit they engage in the behavioral bias of heaping. A similar logic applies to firms with negative earnings. As the level of loss increases (i.e., earnings becomes more negative), analysts are likely engage in more rounding. Therefore we predict that rounding will increase with the absolute value of EPS.[7]

Note that although it is intuitive to expect rounding to increase with the absolute value of EPS, such a finding is not mechanical or predetermined. Cheong and Thomas (2011) show that unscaled forecast errors do not vary with the level of stock price. Given that the level of stock price and EPS are highly correlated, the results suggest that analysts may care about the accuracy of the penny position irrespective of the level of EPS. In addition, we show that actual reported EPS is not rounded as the level of EPS increases. Thus, it is an empirical question as to whether and to what extent rounding varies with the level of EPS.

Investor Recognition and Trading Commissions

We expect analysts to exert more forecasting effort when doing so potentially increases their compensation. If analyst compensation is positively related to trading commissions, then we expect analysts to exert more forecasting effort in firms that generate greater brokerage fees. We contend that an analyst is more likely to round when he or she is exerting less effort and so is less confident about the forecast. Therefore, we expect more rounding in firms that generate less brokerage business. Below we provide our proxies for brokerage fees.

Trading Volume (Predicted Sign: –)

A stock that has a high trading volume offers greater opportunities for trades to pass through the brokerage division (Jackson 2005). Analysts who provide more informative research are likely to generate more trading business, and this can benefit the analyst. In addition, trading volume can also reflect greater disagreement among investors about the value of the company and, hence, encourage information search (e.g., Beaver 1968; Karpoff 1986; Harris and Raviv 1993; Kandel and Pearson 1995; Kim and Verrecchia 1997). In contrast, low trading volume offers fewer brokerage fee opportunities, and so analysts are likely to exert less effort in forecasting. This lack of effort will result in greater uncertainty concerning the penny digit and will increase the likelihood that the analyst will round.[8]

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[7] Note that our prediction is similar to Harris (1991) who predicts that stock prices will exhibit more clustering around zero as the price level increases. However, our prediction relates to analyst incentives while Harris's (1991) prediction relates to incentives of traders and exchange regulations.

[8] Herrmann and Thomas (2005) find that more regulated, slower growth industries have more rounded forecasts. They suggest that analysts might provide less effort and commit fewer resources toward these firms because these industries likely generate less trading revenue for brokerage firms. The trading revenue argument is proposed as an untested conjecture in Herrmann and Thomas (2005). We test the hypothesis formally in this paper.

Return Variance (Predicted Sign: –)

If there is more uncertainty about a firm's future cash flows, then the stock is likely to have greater return volatility. Higher volatility presents more opportunities for traders to gain from information acquisition and so increases the demand for informative analyst research (e.g., Frankel et al. 2006). In contrast, a stock that has low return volatility and stable earnings offers fewer opportunities to gain from trade and so decreases the demand for informative forecasts. We therefore expect rounding to occur more frequently in firms with low return variance. Our prediction contrasts from what one would expect under a purely "analysts' lack of information" perspective on rounding. Holding everything else constant, one would expect less volatile stock to be easier to predict and, hence, would require less rounding.

Institutional Ownership (Predicted Sign: –)

The usefulness of analyst research for institutional investors is an important factor in determining whether analysts are nominated for *Institutional Investors*' "All American Research Team." Being viewed by institutions as the lead analyst in a particular industry can have significant compensation benefits (Groysberg et al. 2008). This suggests that an analyst is likely to exert more effort in valuing companies and forecasting earnings in firms with greater institutional holdings. In addition, since many institutions trade frequently, institutions can reward analysts for informative reports by trading through the brokerage division. In contrast, a stock with little institutional representation is likely to have fewer such side-benefits, and so an analyst is likely to put in less effort to produce an informative report and forecast, and more likely to round.

Growth Opportunities: Book-to-Market and Earnings-to-Price (Predicted Sign: +)

Frankel et al. (2006) point out that the effect of growth opportunities on informative reports depends on several factors. The value of growth firms is determined to a greater extent by future profitability relative to current profitability. Thus, providing more informative reports requires the analyst to exert effort and incur higher information-gathering costs to determine potential future opportunities. However, investors are also more interested in growth companies because of the greater potential gains, and this creates an increased demand for analyst guidance. In contrast, the earnings of low-growth firms are likely to be easier to forecast and to require less information acquisition on the part of the analyst, but the demand for informative reports is also likely to be lower. On balance, low-growth stocks are more likely to have lower investor interest and are less likely to raise financing. Therefore, low-growth firms are likely to be less lucrative for the employers of analysts, and so we expect less forecasting effort. As a consequence, we predict analysts are more likely to round in these firms. Our proxies for growth opportunities are *book-to-market* and *earnings-to-price*. We expect firms with fewer growth opportunities to have high values of both ratios.

Investment Banking Business Opportunities

External Financing (Predicted Sign: –)

Firms that are raising financing frequently select the investment bank that has a favorable analyst following the firm to underwrite the deal (Lin and McNichols 1998; Michaely and Womack 1999; Dechow et al. 2000; Bradshaw et al. 2006). Understanding this relation, an analyst who is covering a firm that is likely to need financing is likely to exert more effort in forecasting. By having greater knowledge of the firm, the analyst will increase the corporate finance division's ability to attract the firm as a client. We therefore predict that analysts are less likely to round their forecasts for firms that are raising financing. Following Bradshaw et al. (2006), we use the

Statement of Cash Flows to determine the amount of cash raised from debt and equity financing. We calculate *EXFIN* as the net amount of cash flow received from financing activities scaled by the market value of common equity of the current fiscal year.

Business Complexity

Firm Size (Predicted Sign: ?)

Larger firms are more complex than small firms, making them more difficult to analyze and value. In addition, if an analyst does provide an informative report, then this information is likely to be quickly dispelled among market participants and not necessarily captured by the analysts' brokerage division. The impact of these factors will increase the likelihood of observing rounding in forecasts for large firms. However, larger firms are also likely to have other sources of information, such as news reports and management voluntary disclosures, that potentially reduce the effort analysts must incur to forecast earnings. These factors would reduce the likelihood of observing rounding in large firms. Note also that size can proxy for many different things (e.g., it could also be correlated with brokerage fees). Therefore, we include *SIZE* in our tests, but we do not sign our prediction for this variable.

Number of Business Segments (Predicted Sign: +)

Companies with complicated business models require more effort to understand and therefore impose more costs on analysts. Effort-averse analysts, when facing these higher costs, could refrain from collecting information. This could lead to a positive association between business complexity and rounding. However, multiple-segment firms tend to be large firms. Investors could demand more informative forecasts for these firms. This could give analysts more incentive to acquire information and therefore lead to less rounding. We therefore predict that controlling for firm size, firms with more business segments will have more rounded forecasts.

Analyst Characteristics

Herrmann and Thomas (2005) examine the influence of analyst characteristics on analyst decisions to round EPS forecasts. Specifically, they investigate whether analysts are more likely to round when they have lower past accuracy, work for smaller brokerage firms, forecast less frequently, have less firm experience, have more stale forecasts, or when they follow more firms or more industries. For completeness and comparability, we include these analyst characteristics variables in our tests. Our definitions of variables are the same as in Herrmann and Thomas (2005). The detailed variable definitions are provided in Appendix A.

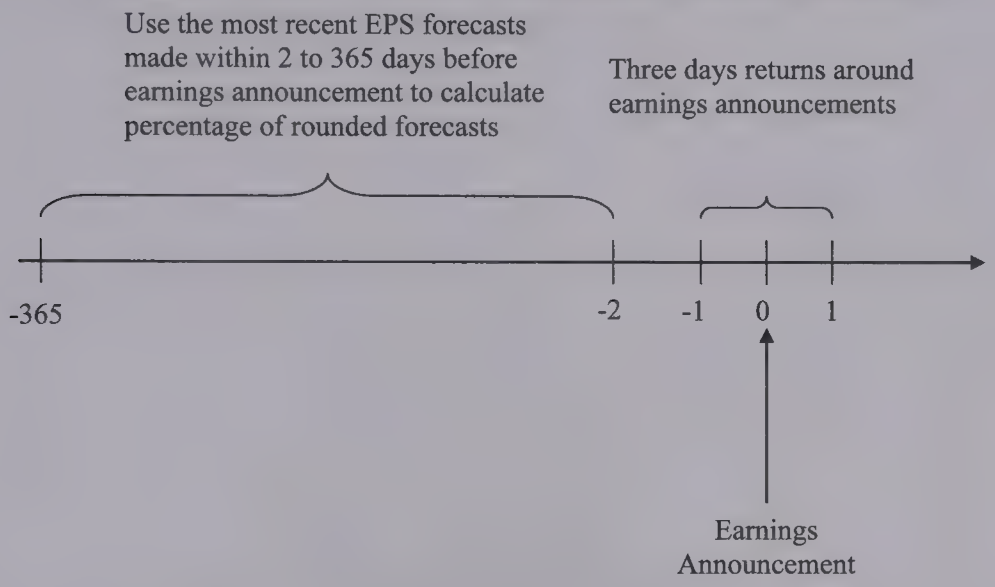
Rounding and Earnings Response Coefficients

Our final prediction tests one implication of rounding. If rounding proxies for analysts exerting less effort, and in turn, having less accurate forecasts, then rounded EPS forecasts will be a poorer proxy of the market's expectation of earnings than non-rounded forecasts. If this is the case, then earnings surprises (actual EPS – forecast EPS) based on rounded forecasts will contain more error than non-rounded forecasts of EPS. We therefore predict that earnings response coefficients will be lower for rounded forecasts due to the greater error in variables problem with these forecasts.

IV. SAMPLE

We obtain financial statement data from Compustat and stock returns from CRSP. We obtain analyst forecasts from I/B/E/S for the period 1984 to 2009. We use the *unadjusted* detail database to

EXHIBIT 1
Timeline of Tests of Investor Response to Analyst Rounding


Use the most recent EPS forecasts made within 2 to 365 days before earnings announcement to calculate percentage of rounded forecasts
Three days returns around earnings announcements
-365
-2
-1
0
1
Earnings Announcement

avoid the retroactive stock split rounding effect as described in Baber and Kang (2002) and Payne and Thomas (2003). For our empirical tests, we obtain the most recent *annual* EPS forecasts. This is the forecast for each available analyst, made closest to the corresponding annual earnings announcement date. We allow the "most recent" forecast for a specific analyst to be made up to one year prior to the earnings release date. See Exhibit 1 for a timeline.

We collect the actual EPS from I/B/E/S to ensure consistency with the forecasts in the treatment of nonrecurring items. *Accuracy* is calculated as the absolute value of *Forecast – Actual EPS*. When we calculate *Accuracy* we adjust the forecast for splits between the time that the forecast was made and the release of actual EPS. We do this so that both actual EPS and forecast EPS are measured on the same basis. We scale the forecast error in two ways. We first scale the forecast error by price to help make it comparable across firms. We also scale the forecast error by the absolute value of actual EPS. We scale observations wherein the absolute value of actual EPS is less than 3 cents by 3 to control for the effect of extreme outliers.[9] We also examine forecast *bias* since analysts tend to be optimistic and, therefore, accuracy is likely to be correlated with bias. In addition, it is interesting from a descriptive perspective to determine the extent to which rounded forecasts are biased.

V. RESULTS

Our results are presented in four sections. We present descriptive evidence relating to rounding in the earnings forecast distribution and details on how rounding changes with the level of the EPS forecast. We next provide our formal tests examining the relation between firm characteristics and rounding using the analyst forecast as the variable of analysis. We then present a similar analysis of the data at the firm level. The final section provides our tests of the relation between earnings response coefficients and rounding.

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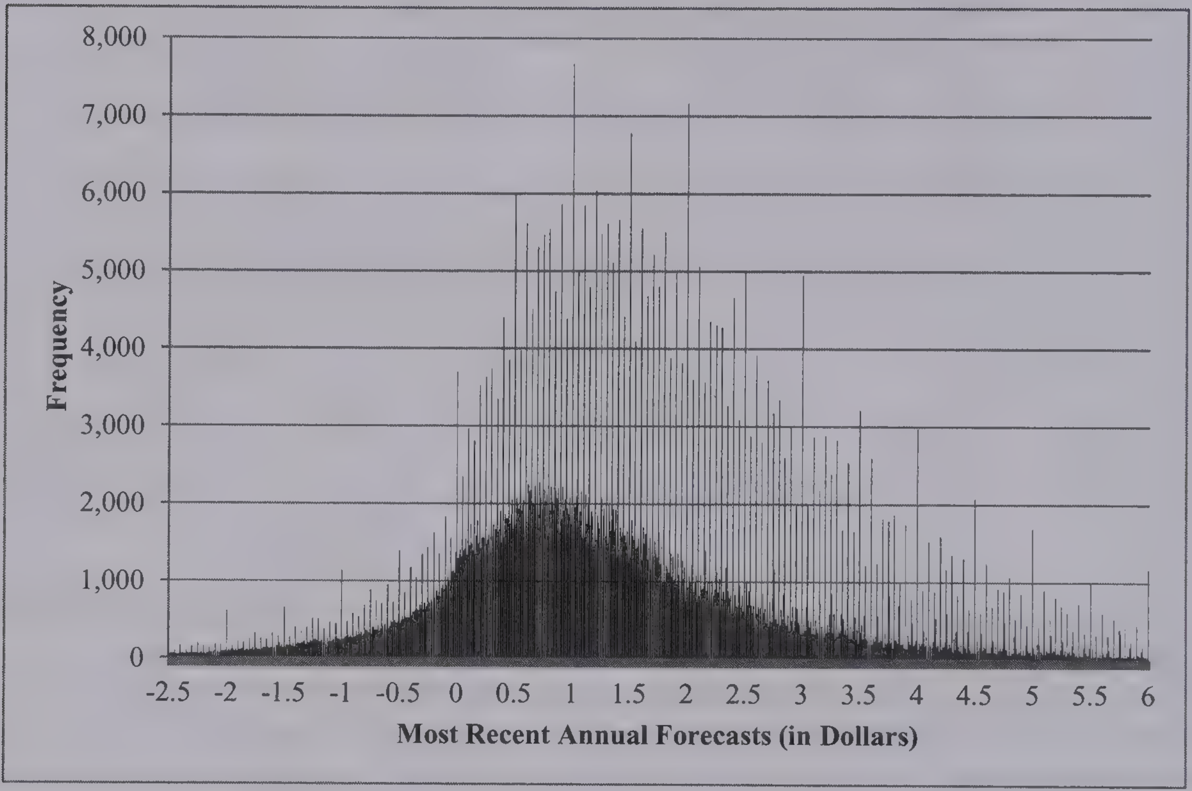
[9] Excluding all observations with actual EPS less than 3 cents instead of winsorizing them does not change the conclusions.

FIGURE 1
Distributions of Analyst EPS Forecasts



This figure plots the distribution of the most recent EPS forecasts for each firm-analyst-year observation. The sample includes 809,129 observations from 1984 to 2009, among which 46.30 percent are rounded to the nearest 5 cents.

Descriptive Evidence on Rounding and Its Relation to the Level of EPS

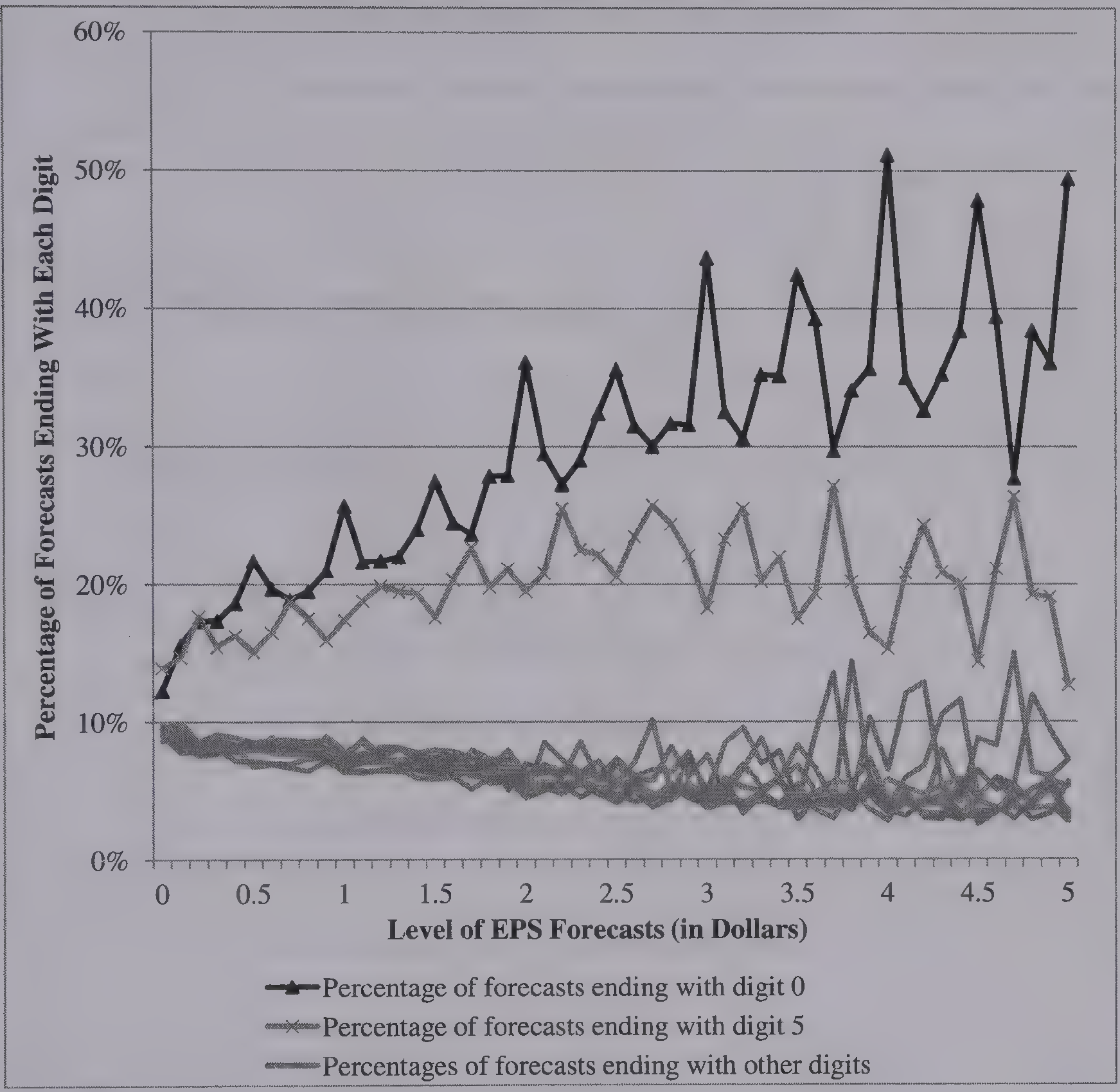
Figure 1 provides the distribution of analyst EPS forecasts. There is clearly a greater frequency of forecasts that are rounded to the nearest 50 cents ($1, $1.50, $2, etc.), then to the nearest 10 cents ($1.10, $1.20, $1.30, etc.), and finally to the nearest five cents (e.g., .65, .75 cents). Among the 809,129 most recent annual EPS forecasts, 46.30 percent are rounded to 0 or 5.

Figure 2 examines the proportion of forecasts ending with each digit as the level of EPS increases. The horizontal axis reports the level of EPS in 10-cent intervals (0.10 to 0.20, etc.) and the vertical axis reports the proportion of forecasts that ends with each specific digit. Figure 2 shows that as the level of EPS increases to $5.00, the proportion of forecasts that end with zero increases from just above 10 percent to around 40 percent. The figure also indicates that the proportion of EPS forecasts ending with a five increases from around 15 percent but then steadies out at about 20 percent after forecast EPS is over $1.00. Thus analysts appear to round more to 0 than to 5 as the level of EPS increases. The figure shows that the proportions of forecasts ending with digits 1 through 4 or 6 through 9 tend to decline below 10 percent as the level of EPS increases.

Figure 3, Panel A is similar to Figure 2 except that it combines forecasts that end with a zero or a five and plots the combined proportion as the level of forecast EPS increases. We also plot the proportion of actual EPS that have a 0 or 5 in the penny digit to compare the relative proportions. As expected, the percentage of rounded forecasts increases systematically with the magnitude of EPS forecasts. In contrast, the percentage of actual EPS that ends with a 0 or 5 in the penny digit stays at about 20 percent across the whole range that we examine. Panel B of Figure 3 reports the extent of rounding for negative EPS stocks.

FIGURE 2

Percentage of Forecasts Ending with Zero, Five, versus Other Digits



This figure plots the percentage of forecasts ending with zero, five, and other digits for forecasts of different magnitudes. We place analysts' forecasts into bins with width of 10 cents. For example, bin $0.0 includes forecasts in the range of [0,0.10), bin $0.10 includes forecasts in the range of [0.10,0.20) etc.

The results show a similar, although somewhat weaker, increasing trend of rounding in EPS forecasts as the absolute value of the forecasts increases.

Table 1 provides a more detailed analysis of analyst EPS forecasts ending with each digit. The digits are ranked from the highest to the lowest percentage of the distribution. The results indicate that zeros are most common (25.95 percent), followed by 5 (18.66 percent). For other digits, consistent with analysts applying the "round half to even rule," there are a greater frequency of forecasts that end with an even number (29.99 percent) than forecasts that end with an odd number (25.40 percent).[10] The next most

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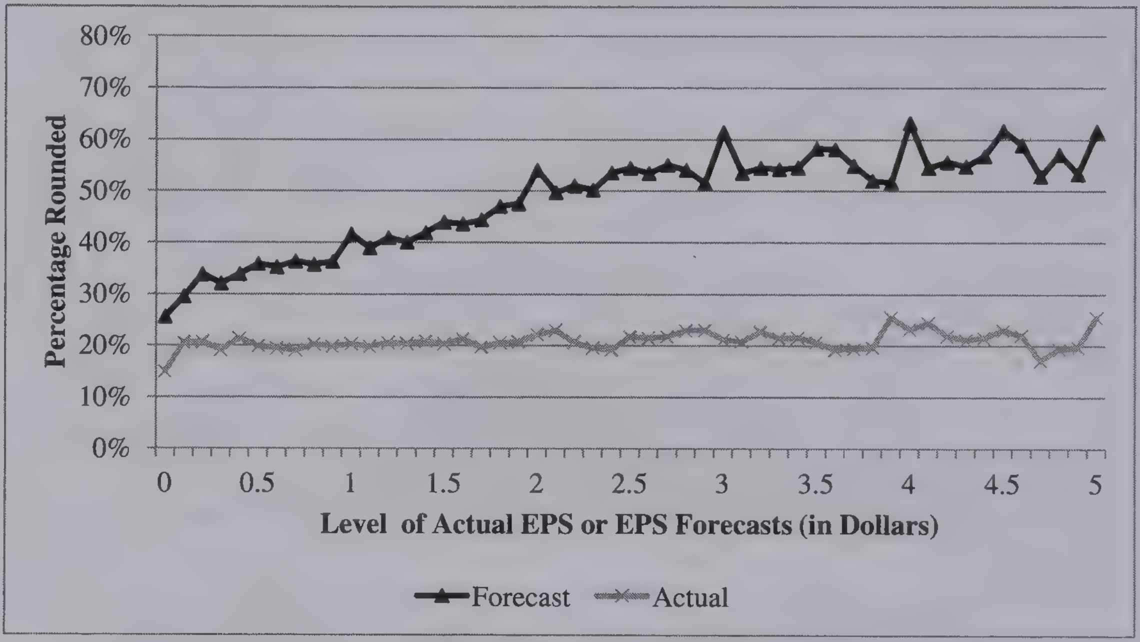
[10] The "round half to even" rule is a tie-breaking rule when the digit to be rounded off is 5. This method is commonly used in bookkeeping and involves analyzing the digit that is affected by the rounding. If the digit is odd, then the number is rounded up to an even number; if the digit is even, then the number is rounded down to the even number. For example, 3.5 is rounded up to 4, 2.5 is rounded down to 2.

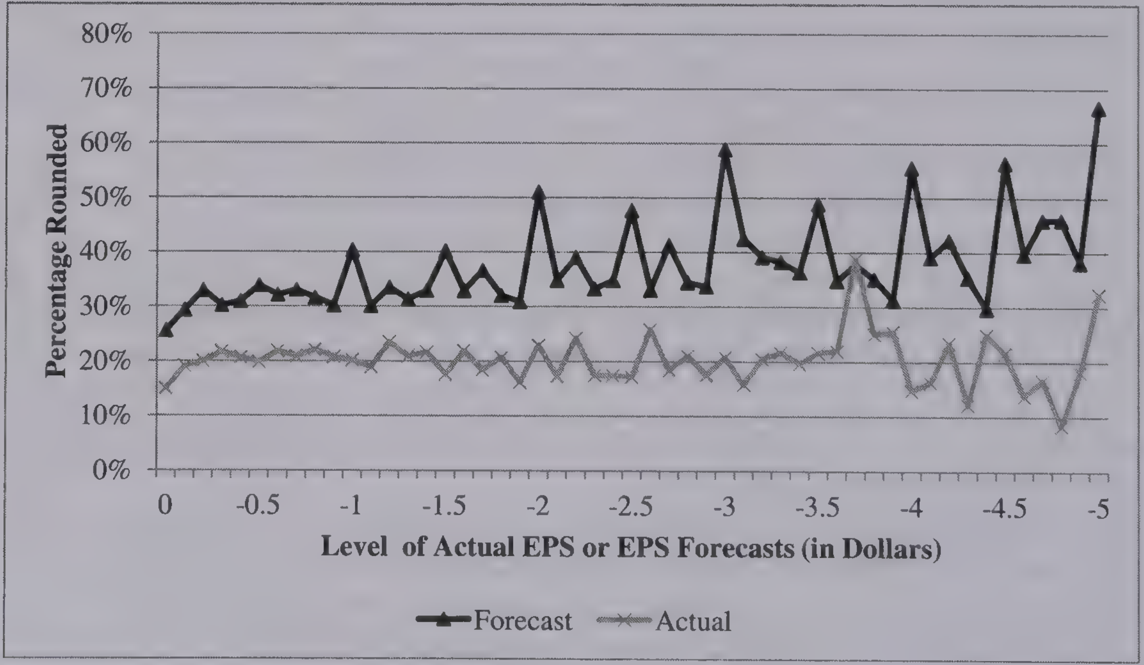
FIGURE 3

The Relation between Rounding (the Penny Digit Being a Zero or Five) and the Level of Actual Reported EPS and Forecast EPS

Panel A: Percentage of Rounded Forecasts for Positive Forecasts



Panel B: Percentage of Rounded Forecasts for Negative Forecasts



This table plots the percentage of rounded (the penny digit being a zero or five) actual EPS or forecast EPS for different levels of EPS. We put observations between −\$5 and \$5 into 101 bins each with an interval of \$0.1, for example, bin labeled \$0.0 includes those forecasts in the range of [\$0, \$0.01), bin labeled −\$0.1 includes those in [−\$0.1, 0), and bin labeled \$0.1 contains those in [0, \$0.1).

TABLE 1

The Frequency, Accuracy, and Bias of Each Cent Digit of Analyst EPS Forecasts

Panel A: Frequency, Forecast Bias, and Forecast Accuracy for Analyst EPS Forecasts Ending with Each Digit (Forecast Error is defined as 100 × (Forecast − Actual)/Price; Forecast Accuracy is defined as 100 × |Forecast − Actual|/Price)

Cent Digit	n	Percentage of Distribution	Mean Forecast Error (Bias)	Mean Absolute Forecast Error (Accuracy)	Bias/Accuracy
0	198,385	25.95%	1.439	2.246	0.64
5	142,652	18.66%	1.189	1.945	0.61
Other even digits					
6	58,918	7.71%	0.873	1.633	0.53
4	57,803	7.56%	0.883	1.662	0.53
2	57,460	7.52%	0.971	1.748	0.56
8	55,050	7.20%	0.901	1.679	0.54
Combined	229,231	29.99%	0.907	1.680	0.54
Other odd digits					
3	50,757	6.64%	0.963	1.764	0.55
7	49,949	6.53%	0.932	1.755	0.53
1	47,432	6.20%	1.186	2.002	0.59
9	46,103	6.03%	0.988	1.843	0.54
Combined	194,241	25.40%	1.015	1.839	0.55

Panel B: Frequency, Forecast Bias, and Forecast Accuracy for Analyst EPS Forecasts Ending with Each Digit (Forecast Error is defined as (Forecast − Actual)/max(0.03,|Actual|); Forecast Accuracy is defined as |Forecast − Actual|/max(0.03,|Actual|)

Cent Digit	n	Percentage of Distribution	Mean Forecast Error (Bias)	Mean Absolute Forecast Error (Accuracy)	Bias/Accuracy
0	198,385	25.95%	0.173	0.287	0.60
5	142,652	18.66%	0.146	0.259	0.56
Other even digits:					
6	58,918	7.71%	0.092	0.217	0.42
4	57,803	7.56%	0.088	0.222	0.40
2	57,460	7.52%	0.086	0.222	0.38
8	55,050	7.20%	0.084	0.213	0.40
Combined	229,231	29.99%	0.088	0.218	0.40
Other odd digits:					
3	50,757	6.64%	0.081	0.225	0.36
7	49,949	6.53%	0.080	0.217	0.37
1	47,432	6.20%	0.085	0.235	0.36
9	46,103	6.03%	0.088	0.223	0.40
Combined	194,241	25.40%	0.083	0.225	0.37

common numbers are the even numbers around 5, that is, 6 and 4. The least-reported digits are 1 and 9 that are on either side of 0. The low frequency of 1 and 9 is consistent with these being the first numbers to get rounded to 0 as the level of EPS increases.

Consistent with Herrmann and Thomas (2005), the results in Table 1, Panel A indicate that forecasts ending with 0 and 5 are less accurate than other forecasts. Herrmann and Thomas (2005) do not investigate the bias in forecasts, but our results suggest that bias plays a role in explaining the relative inaccuracy of rounded forecasts.[11] We find that 0 and 5 forecasts are more optimistically biased than other forecasts. In addition, the ratio of bias to accuracy is approximately 61–64 percent for rounded forecasts. In contrast, this ratio is around 55 percent for other numbers. Therefore, when analysts face uncertainty and decide to round, they tend to round *up*. Why they should round up is not immediately obvious. One possible explanation is linked to our tests reported later in the paper. It is a well-known empirical regularity that analyst forecasts tend to be "walked down" as the earnings announcement approaches (e.g., Richardson et al. 2004). If analysts round more in stocks that are of little investor interest, then they may also be less likely to update their forecasts as new information becomes available. Consistent with this explanation, we find that rounded forecasts tend to be staler than non-rounded forecasts.[12] Panel B of Table 1 presents similar results for forecast errors scaled by actual EPS.

Table 2 provides details concerning the extent of rounding for different parts of the forecast EPS distribution. Panel A reveals that about 32 percent of negative forecasts are rounded. For positive EPS forecasts under $1.00, approximately 34 percent of the forecasts are rounded. For stocks between $1.00 and $10.00 approximately 49 percent are rounded, and for the few stocks with EPS forecasts greater than $10 but less than $100, approximately 60 percent are rounded. Rounding increases to 95 percent for the very few stocks with forecast EPS over $1,000. Panel A also shows that rounded forecasts are less accurate in categories where EPS forecasts are less than $10.00. For high EPS stock (over $10.00), rounding no longer appears to reflect a lack of uncertainty since rounded forecasts are close to or somewhat more accurate than non-rounded forecasts. This could be because, for high EPS stock, rounding to the nearest penny has very little impact on overall precision, or because rounding is done purely to provide a simpler number that is easier to communicate. Panel B provides results concerning bias and mirrors the accuracy results reported in Panel A.

In summary, Herrmann and Thomas (2005) hypothesize that rounding is due to analysts lacking information to accurately forecast EPS. The results in this section provide an additional and complementary explanation. Our results indicate that rounding increases with the level of EPS. As EPS increases, the penny digit of the forecast is of less economic significance and, by rounding, analysts indicate (either intentionally or subliminally) that their forecasts are not precise "to the penny." In addition, as EPS increases, analysts could round to produce a number that is easier or simpler to communicate. Finally, our results suggest that rounded forecasts are more optimistically biased than other forecasts. When analysts round, they tend to round up.

Analyst-Level Analysis

We next investigate the relation between rounded forecasts and our firm-specific determinants. We first estimate the following logistic model:

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[11] In a related study, Zhou (2010) documents an optimistic bias associated with "nickel forecasts." Zhou (2010) does not investigate firm-specific determinants of rounding.

[12] In unreported tests we find that rounded forecasts are more biased than non-rounded forecasts even after controlling for forecast horizon. Therefore, the walk-down effect is a partial, but not complete, explanation for the bias.

TABLE 2

Mean Forecast Accuracy and Bias for Rounded versus Other Forecasts

Panel A: Forecast Accuracy = 100 × |Forecast − Actual|/Price

Group	n	Percent Rounded	Rounded	Other	Difference
(−∞,0)	88,755	32.26%	8.266	5.663	2.603***
[0, 1)	199,918	34.15%	2.832	1.654	1.178***
[1, 10)	469,899	49.36%	1.163	0.825	0.338***
[10, 100)	5,750	60.44%	1.529	2.429	−0.900***
[100, 1,000)	149	90.60%	1.598	5.767	−4.169
[1,000, ∞)	38	94.74%	0.555	1.739	−1.184***

Panel B: Forecast Bias = 100 × (Forecast − Actual)/Price

Group	n	Percent Rounded	Rounded	Other	Difference
(−∞,0)	88,755	32.26%	5.814	3.428	2.386***
[0, 1)	199,918	34.15%	2.052	0.920	1.132***
[1, 10)	469,899	49.36%	0.588	0.350	0.239***
[10, 100)	5,750	60.44%	0.810	1.639	−0.830***
[100, 1,000)	149	90.60%	1.456	5.676	−4.220
[1,000, ∞)	38	94.74%	0.017	−1.739	1.756***

*** Indicates significance at the 1 percent level.

This table compares the forecast accuracy and forecast bias for rounded versus other forecasts at different levels. Rounded forecasts are forecasts that end with a 0 or 5 in the penny digit.

$$\begin{aligned} Prob(ROUND_{i,j,t} = 1) = F(&\beta_0 + \beta_1 ALAGEPS_{i,t} + \beta_2 VOLATILITY_{i,j,t} + \beta_3 VOLUME_{i,j,t} \\ &+ \beta_4 INST\_HOLD_{i,j,t} + \beta_5 BM_{i,j,t} + \beta_6 EP_{i,j,t} + \beta_7 EXFIN_{i,t} \\ &+ \beta_8 SIZE_{i,j,t} + \beta_9 NSEG_{j,t} + \varepsilon_{i,j,t}), \end{aligned} \tag{1}$$

where $F(\beta'X) = e^{\beta'X}/(1 + e^{\beta'X})$.

$ROUND_{i,j,t}$ = 1 if analyst *i*'s most recent forecast of EPS for firm *j*'s year *t* is a rounded forecast, and 0 otherwise;

$ALAGEPS_{i,t}$ = absolute value of year *t*–1 actual EPS of firm *i*;

$VOLATILITY_{i,j,t}$ = standard deviation of monthly stock returns over the 12 months prior to the forecast date;

$VOLUME_{i,j,t}$ = standardized trading volume over the 12 months prior to the forecast date;[13]

$INST\_HOLD_{i,j,t}$ = percentage of shares outstanding owned by institutional investors in the quarter before the forecast date;

$BM_{i,j,t}$ = ratio of book value of equity to market value at the end of the month before the forecast date;

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[13] In light of the difference in the market microstructure between NASDAQ and NYSE/AMEX stocks (Atkins and Dyl 1997), we standardize the variable by sorting stocks into 100 portfolios separately for NASDAQ and NYSE/AMEX stocks, and then divide the rank by 100 to convert the value to the range between 0 and 1.

$EP_{i,j,t}$ = ratio of prior year's net income to market value at the end of the month before the forecast date;
$EXFIN_{i,t}$ = computed as in Bradshaw et al. (2006) and is the net amount of cash flow received from debt and equity financing activities as indicated in the Statement of Cash Flows for the current fiscal year, scaled by the market value of common equity;
$SIZE_{i,j,t}$ = logarithm of market value at the end of the month before the forecast date; and
$NSEG_{i,t}$ = logarithm of (1 + number of business segments of firm *j*).

Herrmann and Thomas (2005) examine whether analyst characteristics explain rounding. To investigate the relative importance of analyst characteristics in determining rounding, we estimate the following logistic model:

$$\begin{aligned} Prob(ROUND_{i,j,t} = 1) = F(&\beta_0 + \beta_{10}LAGACCU_{i,j,t} + \beta_{11}BROKERSIZE_{i,j,t} \\ &+ \beta_{12}FOR\_FREQ_{i,j,t} + \beta_{13}EXPERIENCE_{i,j,t} + \beta_{14}HORIZON_{i,j,t} \\ &+ \beta_{15}FIRMS\_FOL_{i,j,t} + \varepsilon_{i,j,t}). \end{aligned} \tag{2}$$

We construct the analyst characteristic variables in the same manner as Herrmann and Thomas (2005) where rounding analysts are compared to other analysts following the same firm:

LAGACCU = accuracy of the analyst's prior year's EPS forecast for the firm;
BROKERSIZE = based on the number of analysts employed by the brokerage house;
FOR_FREQ = number of forecasts the analyst made for the firm in a given year;
EXPERIENCE = number of years the analyst has followed the firm;
HORIZON = number of calendar days from the date of the forecast to the date of the earnings announcement; and
FIRMS_FOL = number of firms followed by the analyst.

The detailed definitions of these variables are provided in Appendix A. We also examine the number of industries followed (*IND_FOL*). Industry following is highly correlated with *FIRM_FOL* so we do not include both in our regressions. Results are similar when we substitute *IND_FOL* for *FIRM_FOL* in our analysis.

To investigate the incremental importance of firm-level variables versus analyst characteristics in determining rounding, we estimate Model (3) that combines Models (1) and (2) and examine Pseudo R^2 and significance levels of the variables:

$$\begin{aligned} Prob(ROUND_{i,j,t} = 1) = F(&\beta_0 + \beta_1 ALAGEPS_{i,t} + \beta_2 VOLATILIITY_{i,j,t} + \beta_3 VOLUME_{i,j,t} \\ &+ \beta_4 INST\_HOLD_{i,j,t} + \beta_5 BM_{i,j,t} + \beta_6 EP_{i,j,t} + \beta_7 EXFIN_{i,t} \\ &+ \beta_8 SIZE_{i,j,t} + \beta_9 NSEG_{j,t} + \beta_{10}LAGACCU_{i,j,t} \\ &+ \beta_{11}BROKERSIZE_{i,j,t} + \beta_{12}FOR\_FREQ_{i,j,t} \\ &+ \beta_{13}EXPERIENCE_{i,j,t} + \beta_{14}HORIZON_{i,j,t} \\ &+ \beta_{15}FIRMS\_FOL_{i,j,t} + \varepsilon_{i,j,t}). \end{aligned} \tag{3}$$

To mitigate the problem that cross-sectional dependence inflates test statistics, we estimate all regression models by year and report the mean coefficient. To mitigate any time-series dependence problems we further correct the Fama-MacBeth t-statistics for serial correlations of one lag.

Table 3 provides the means for each variable and a correlation matrix. Panel A reveals that the differences in means between rounded and non-rounded forecasts for firm-level variables are all significant in the predicted direction. Rounded forecasts have higher levels of lagged absolute value of EPS; the followed firms have less volatile stock returns, have lower stock volume, have lower institutional holdings, have high book-to-market and EP ratios (i.e., are "low-growth" stock), and

TABLE 3

Descriptive Statistics and Correlations of Determinants of Rounding

Panel A: Mean for Firm-Specific Characteristics

	n	*ROUND*	*ZERO*	*ALAG-EPS*	*VOLA-TILITY*	*VOLUME*	*INST_HOLD*	*BM*	*EP*	*EXFIN*	*SIZE*	*NSEG*
Predicted Sign of Difference:				+	–	–	–	+	+	–	?	+
All	242,010	45.09%	26.14%	1.933	0.115	3.119	0.595	0.538	0.053	0.010	7.402	1.092
Rounded	109,118		57.96%	2.218	0.102	2.935	0.563	0.571	0.059	0.004	7.464	1.132
Other	132,892		0.00%	1.700	0.125	3.271	0.623	0.510	0.048	0.015	7.351	1.059
Difference			57.96%	0.518	–0.023	–0.336	–0.060	0.061	0.011	–0.012	0.113	0.073
p-value				< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001

Panel B: Mean for Analyst Characteristics

	n	*ROUND*	*LAGACCU*	*BROKERSIZE*	*FOR_FREQ*	*EXPERIENCE*	*HORIZON*	*FIRMS_FOL*	*IND_FOL*
Predicted Sign of Difference:			–	–	–	–	+	+	+
All	242,010	45.09%	0.635	0.393	0.488	0.511	0.392	0.321	0.284
Rounded	109,118		0.632	0.395	0.444	0.515	0.420	0.295	0.263
Other	132,892		0.638	0.391	0.525	0.507	0.368	0.342	0.301
Difference			–0.006	0.004	–0.082	0.009	0.052	–0.047	–0.038
p-value			0.0088	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001

(continued on next page)

TABLE 3 (continued)

Panel C: Correlation Matrix with Pearson (Spearman) Correlations on the Upper (Lower) Diagonal

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
(1) *ZERO*		0.66	0.14	−0.12	−0.07	−0.10	0.05	0.10	−0.03	0.02	0.06	−0.01	0.00	−0.09	0.01	0.06	−0.06	−0.05
(2) *ZEROFIVE*	0.66		0.16	−0.16	−0.09	−0.13	0.06	0.12	−0.04	0.03	0.08	−0.01	0.01	−0.12	0.01	0.08	−0.07	−0.06
Firm Characteristics																		
(3) *ALAGEPS*	0.15	0.18		−0.32	0.27	0.05	0.01	0.40	−0.03	0.41	0.24	0.04	−0.01	−0.06	−0.05	−0.06	−0.13	−0.12
(4) *VOLATILITY*	−0.13	−0.18	−0.45		−0.03	−0.01	0.12	−0.24	0.07	−0.34	−0.19	−0.04	0.00	0.07	0.07	0.05	0.07	0.08
(5) *VOLUME*	−0.07	−0.09	0.28	−0.07		0.44	−0.26	−0.05	−0.06	0.86	0.23	0.12	−0.06	−0.05	−0.18	−0.10	−0.09	−0.12
(6)*INST_HOLD*	−0.11	−0.14	0.05	0.05	0.44		−0.11	−0.07	−0.02	0.24	0.08	0.04	−0.03	0.03	−0.11	−0.04	0.08	0.05
(7) *BM*	0.10	0.12	0.03	−0.01	−0.31	−0.14		0.26	−0.07	−0.33	0.11	−0.02	0.02	0.03	0.03	−0.01	0.03	0.02
(8) *EP*	0.11	0.15	0.50	−0.33	−0.05	−0.09	0.29		−0.14	0.01	0.12	0.02	0.01	−0.02	0.00	−0.03	−0.04	−0.04
(9) *EXFIN*	−0.05	−0.07	−0.14	0.19	−0.08	−0.02	−0.02	−0.03		−0.10	−0.07	−0.01	0.00	0.02	0.04	0.00	0.02	0.03
(10) *SIZE*	0.03	0.04	0.46	−0.40	0.85	0.23	−0.32	0.07	−0.16		0.33	0.12	−0.05	−0.10	−0.18	−0.10	−0.17	−0.19
(11) *NSEG*	0.06	0.08	0.26	−0.23	0.22	0.07	0.06	0.10	−0.13	0.34		0.02	0.00	−0.03	−0.08	−0.02	−0.05	−0.04
Analyst Characteristics																		
(12) *LAGACCU*	−0.01	−0.02	0.01	−0.01	0.06	0.02	−0.02	0.01	0.00	0.05	0.00		0.00	0.02	−0.03	−0.07	−0.06	−0.06
(13) *BROKERSIZE*	0.00	0.00	0.02	−0.01	−0.02	−0.02	0.02	0.01	−0.01	0.00	0.00	0.02		0.04	0.04	0.00	0.02	−0.12
(14) *FOR_FREQ*	−0.10	−0.12	−0.07	0.09	−0.04	0.04	0.01	−0.04	0.03	−0.10	−0.03	0.03	0.04		0.04	−0.38	0.09	0.05
(15) *EXPERIENCE*	0.01	0.01	−0.06	0.07	−0.18	−0.11	0.02	−0.01	0.05	−0.18	−0.08	0.00	0.03	0.04		−0.06	0.12	0.07
(16) *HORIZON*	0.06	0.08	−0.05	0.03	−0.06	−0.02	−0.03	−0.04	0.00	−0.06	−0.02	−0.05	−0.01	−0.36	−0.08		0.00	0.04
(17) *FIRMS_FOL*	−0.06	−0.07	−0.11	0.07	−0.03	0.12	0.00	−0.06	0.02	−0.11	−0.04	−0.03	0.00	0.10	0.11	−0.02		0.62
(18) *IND_FOL*	−0.05	−0.06	−0.11	0.09	−0.05	0.09	−0.02	−0.06	0.03	−0.13	−0.03	−0.03	−0.15	0.05	0.06	0.02	0.60	

(continued on next page)

TABLE 3 (continued)

Detailed definitions of analyst-characteristic variables are provided in Appendix A.

Variable Definitions:
Firm-Characteristic Variables:
$ALAGEPS_{i,t}$ = absolute value of year t–1 actual EPS of firm i;
$VOLATILITY_{i,j,t}$ = standard deviation of monthly stock returns over the 12 months prior to the forecast date;
$VOLUME_{i,j,t}$ = standardized trading volume over the 12 months prior to the forecast date;
$INST\_HOLD_{i,j,t}$ = percentage of shares outstanding owned by institutional investors in the quarter before the forecast date;
$BM_{i,j,t}$ = ratio of book value of equity to market value at the end of the month before the forecast date;
$EP_{i,j,t}$ = ratio of prior year's net income to market value at the end of the month before the forecast date;
$EXFIN_{i,t}$ = net amount of cash flow received from external (debt and equity) financing activities for the current fiscal year as determined from the Statement of Cash Flows scaled by the market value of common equity;
$SIZE_{i,j,t}$ = logarithm of market value at the end of the month before the forecast date; and
$NSEG_{i,t}$ = logarithm of (1 + number of business segments of firm i).
Analyst-Characteristic Variables:
$LAGACCU$ = accuracy of the analyst's prior year's EPS forecast for the firm;
$BROKERSIZE$ = based on number of analysts employed by the brokerage house;
FOR_FREQ = number of forecasts the analyst made for the firm in a given year;
$EXPERIENCE$ = number of years the analyst has followed the firm;
$HORIZON$ = number of calendar days from the date of the forecast to the date of the earnings announcement;
$FIRMS\_FOL_i$ = number of firms followed by the analyst; and
IND_FOL_i = number of firms followed by the analyst.

receive less external financing from the capital market. Analysts also tend to round more in firms with more business segments and in larger firms.

The results of analyst-specific characteristics are reported in Table 3, Panel B and are less consistent with predictions. The differences in *LAGACCU*, *FOR_FREQ*, and *HORIZON* are statistically significant in the predicted direction. However, the differences in the other analyst characteristics (*BROKERSIZE*, *EXPERIENCE*, *FIRMS_FOL*, and *IND_FOL*) are statistically significant in the wrong direction. Similar to Herrmann and Thomas (2005), we find that rounding analysts have more *EXPERIENCE* than non-rounding analysts. As Herrmann and Thomas (2005) acknowledge in their paper, these results are inconsistent with analyst rounding when they lack information because experienced analysts should be more informed than less experienced analysts. One possible explanation for this result is that analysts become less concerned with forecast accuracy as their experience increases. The analyst career-concern literature suggests that analysts are rewarded for accuracy in their early years in order to build a reputation (e.g., Hong and Kubik 2003; Groysberg et al. 2008), but that they are compensated less for accuracy as their tenure with the firm increases. This suggests that the benefit of exerting forecasting effort is greater for analysts with less experience and could explain why rounding increases with analyst experience. The correlations presented in Panel C of Table 3 convey a message similar to the results provided in Panel A and B.

Table 4 provides the results of our multivariate logistic regressions. We first report the results for Model (1) that includes only the firm-level variables. Consistent with our predictions, we find that the coefficients on all firm characteristics have the predicted signs. The results indicate that analysts are more likely to round their forecasts for firms with a larger magnitude of the prior year's EPS. Consistent with brokerage and investment banking fees influencing analysts' incentives, we find that analysts are more likely to round their forecasts in firms with low trading volume, high book-to-market and earning-to-price ratios, and low external financing. Consistent with complexity increasing the likelihood of rounding, we find that large firms with more business segments have more rounded forecasts. The coefficients on volatility and institutional holdings also bear the correct signs but are insignificant.

Model (2) replicates Herrmann and Thomas's (2005) model. The results show that all coefficients on analyst characteristics have the correct signs but are statistically significant for only *FOR_FREQ* and *HORIZON*. The average Model (2) pseudo R^2 (2.35 percent) is about one-third of the average Model (1) pseudo R^2 (6.71 percent). This simple comparison suggests that firm-level variables collectively are more important in determining rounding than analyst characteristics. In Model (3) we include both sets of variables. The coefficients as well as the t-statistics on firm-characteristics are similar to those of Model (1). Including firm characteristics increases the statistical significance of the coefficients on analyst variables considerably. Now all analyst characteristics are significant except the size of the brokerage firm. The pseudo R^2 increases to 9.02 percent for Model (3), suggesting that both firm characteristics and analyst characteristics are important for explaining rounding behavior. Again, Model (3) indicates that more experienced (as opposed to less experienced) analysts are more likely to round their EPS forecasts.

In untabulated results we find that the results are robust to the exclusion of negative forecasts and are generally stronger for forecasts greater than $1 where rounding is more prevalent. We also find qualitatively similar results for subsample periods before and after the year 1996, with the results being stronger for the recent period.

Firm-Level Analysis

One potential issue with the logistic regression reported above is that it gives a greater weight to firms followed by more analysts. To determine whether this weighting impacts our conclusions

TABLE 4

Logistic Regression of Rounding on Firm Characteristics and Analyst Characteristics

	Pred. Sign	Model (1) Estimate	Model (1) t-statistic	Model (2) Estimate	Model (2) t-statistic	Model (3) Estimate	Model (3) t-statistic
Intercept		−1.515***	−13.85	−0.018	−0.09	−1.771***	−10.81
Firm Characteristics							
ALAGEPS	+	0.106***	3.56			0.109***	3.62
VOLATILITY	−	−0.384	−0.67			−0.280	−0.44
VOLUME	−	−0.530***	−5.55			−0.488***	−5.05
INST_HOLD	−	−0.107	−0.78			−0.043	−0.35
BM	+	0.559***	4.56			0.603***	4.72
EP	+	0.670***	3.41			0.674***	3.13
EXFIN	−	−0.295***	−3.57			−0.295***	−3.77
SIZE	?	0.134***	6.41			0.146***	5.77
NSEG	+	0.261***	4.45			0.263***	4.57
Analyst Characteristics							
LAGACCU	−			−0.030	−1.64	−0.052***	−3.18
BROKERSIZE	−			−0.036	−0.74	−0.067	−1.39
FOR_FREQ	−			−0.444***	−7.10	−0.338***	−4.86
EXPERIENCE	−			−0.027	−0.94	0.082***	3.35
HORIZON	+			0.366***	8.08	0.529***	8.57
FIRMS_FOL	+			0.044	0.81	0.158***	3.54
Average n per year		9,337		9,337		9,337	
Pseudo R^2 (%)		6.71		2.35		9.02	

*** Indicates significance at the 1 percent level.

This table provides the regression results of the following logistic models:

$$\begin{aligned} \mathit{Prob}(\mathit{ROUND}_{i,j,t} = 1) = F(&\beta_0 + \beta_1 \mathit{FEPS}_{i,j,t} + \beta_2 \mathit{VOLATILITY}_{i,j,t} + \beta_3 \mathit{VOLUME}_{i,j,t} + \beta_4 \mathit{INST\_HOLD}_{i,j,t} \\ &+ \beta_5 \mathit{BM}_{i,j,t} + \beta_6 \mathit{EP}_{i,j,t} + \beta_7 \mathit{SIZE}_{i,j,t} + \beta_8 \mathit{FOLLOW}_{i,j,t} + \beta_9 \mathit{DISPERSION}_{i,j,t} \\ &+ \beta_{10} \mathit{LOGAGE}_{i,j,t} + \varepsilon_{i,j,t}). \end{aligned}$$

$$\begin{aligned} \mathit{Prob}(\mathit{ROUND}_{i,j,t} = 1) = F(&\beta_0 + \beta_1 \mathit{BROKERSIZE}_{i,j,t} + \beta_2 \mathit{FOR\_FREQ}_{i,j,t} + \beta_3 \mathit{EXPERIENCE}_{i,j,t} \\ &+ \beta_4 \mathit{HORIZON}_{i,j,t} + \beta_5 \mathit{FIRMS\_FOL}_{i,j,t} + \varepsilon_{i,j,t}). \end{aligned}$$

$$\begin{aligned} \mathit{Prob}(\mathit{ROUND}_{i,j,t} = 1) = F(&\beta_0 + \beta_1 \mathit{FEPS}_{i,j,t} + \beta_2 \mathit{VOLATILITY}_{i,j,t} + \beta_3 \mathit{VOLUME}_{i,j,t} + \beta_4 \mathit{INST\_HOLD}_{i,j,t} \\ &+ \beta_5 \mathit{BM}_{i,j,t} + \beta_6 \mathit{EP}_{i,j,t} + \beta_7 \mathit{SIZE}_{i,j,t} + \beta_8 \mathit{FOLLOW}_{i,j,t} + \beta_9 \mathit{DISPERSION}_{i,j,t} \\ &+ \beta_{10} \mathit{LOGAGE}_{i,j,t} + \beta_{11} \mathit{BROKERSIZE}_{i,j,t} + \beta_{12} \mathit{FOR\_FREQ}_{i,j,t} \\ &+ \beta_{13} \mathit{EXPERIENCE}_{i,j,t} + \beta_{14} \mathit{HORIZON}_{i,j,t} + \beta_{15} \mathit{FIRMS\_FOL}_{i,j,t} + \varepsilon_{i,j,t}). \end{aligned}$$

We estimate the regressions cross-sectionally each year and then calculate t-statistics using the Fama-MacBeth approach with Newey-West correction for autocorrelations for 1 year lag.

Variable definitions are provided in Table 3.

drawn from Model (1), we next conduct our analysis at the firm level. We estimate the following model:

$$\begin{aligned} \mathit{PCT\_ROUND}_{i,t} = &\beta_0 + \beta_1 \mathit{ALAGEPS}_{i,t} + \beta_2 \mathit{VOLATILITY}_{i,t} + \beta_3 \mathit{VOLUME}_{i,t} \\ &+ \beta_4 \mathit{INST\_HOLD}_{i,t} + \beta_5 \mathit{BM}_{i,t} + \beta_6 \mathit{EP}_{i,t} + \beta_7 \mathit{EXFIN}_{i,t} + \beta_8 \mathit{SIZE}_{i,t} \\ &+ \beta_9 \mathit{NSEG}_{i,t} + \varepsilon_{i,t}, \end{aligned} \tag{4}$$

TABLE 5

Firm-Level Regression of Percentage of Rounded Forecasts on Firm Characteristics

Model (4)	Predicted Sign	Estimate	t-statistic
Intercept		0.193***	8.13
ALAGEPS	+	0.034***	6.17
VOLATILITY	−	0.029	0.36
VOLUME	−	−0.083***	−3.44
INST_HOLD	−	−0.044**	−2.38
BM	+	0.091***	4.55
EP	+	0.049	0.70
EXFIN	−	−0.042***	−3.55
SIZE	?	0.025***	6.24
NSEG	+	0.061***	8.55
Average n per year		1,823	
R^2		8.95	

, * Indicate significance at the 5 percent and 1 percent levels, respectively.
This table provides the regression results of the following model:

$$PCT\_ROUND_{i,t} = \beta_0 + \beta_1 ALAGEPS_{i,t} + \beta_2 VOLATILITY_{i,t} + \beta_3 VOLUME_{i,,t} + \beta_4 INST\_HOLD_{i,t} + \beta_5 BM_{i,t} + \beta_6 EP_{i,t} + \beta_7 EXFIN_{i,t} + \beta_8 SIZE_{i,t} + \beta_9 NSEG_{i,t} + \varepsilon_{i,t}.$$

We estimate the regressions using the Fama-MacBeth approach with Newey-West correction for autocorrelations for 1 year lag.
Variable definitions are provided in Table 3.

where $PCT\_ROUND_{i,t}$ is the percentage of analysts whose most recent forecasts for firm *i*'s year *t* EPS end with a 0 or 5 in the penny digit. All other variables are defined similarly to that of Model (1) and are calculated as of the month end before the earnings announcement.

Table 5 provides the regression results. The table presents slightly stronger results than those for the analyst-level analysis in Table 4. The coefficients on all independent variables have the predicted sign except *VOLATILITY*, for which the coefficient is insignificant. The results confirm that analysts tend to round their EPS forecasts more for firms with a larger magnitude of EPS, low trading volume, low institutional ownership, low external financing, high book-to-market, large size, and more business segments. These findings indicate that economic considerations such as brokerage commissions, underwriting fees, and business complexity have a significant impact on analysts' effort to improve forecast precision.

Analyst-Level Analysis: Earnings Response Coefficients for Rounded Forecasts

We next examine whether a rounded forecast can be used as a convenient proxy for the greater likelihood that the forecast is a noisier predictor of future earnings. We provide the following regression:

$$RET_{j,t} = \beta_0 + \beta_1 SURP_{i,j,t} + \beta_2 \mathbf{ROUND}_{i,j,t} + \beta_3 \mathbf{ROUND}_{i,j,t} \times \mathbf{SURP}_{i,j,t}, \qquad (5)$$

where, $RET_{j,t}$ is the size-adjusted stock returns for firm *j* over the three-day window around earnings announcement of year *t*. $SURP_{i,j,t}$ is the difference between actual EPS reported by the firm *j* and forecast EPS reported by analyst *i*, scaled by price for earnings announcement *t*. Our objective is to determine whether the coefficient β_3, on the interaction term between *ROUND* and *SURP*, is negative. This would indicate that the earnings response coefficient is biased down due to investors placing less weight on rounded forecasts in forming their expectations. Model (5) allows us to examine unconditionally whether rounded forecasts have a lower response coefficient than non-rounded forecasts.

Model (6) controls for other factors identified in the literature as influencing the magnitude of earnings response coefficients:

$$\begin{aligned} RET_{j,t} = {} & \beta_0 + \beta_1 SURP_{i,j,t} + \beta_2 \boldsymbol{ROUND}_{i,j,t} + \beta_3 \boldsymbol{ROUND}_{i,j,t} \times \boldsymbol{SURP}_{i,j,t} \\ & + \beta_4 VOLATILITY_{i,j,t} + \beta_5 SIZE_{i,j,t} + \beta_6 BM_{i,j,t} + \beta_7 DISPERSION_{i,j,t} \\ & + \beta_8 VOLATILITY_{i,j,t} \times SURP_{i,j,t} + \beta_9 SIZE_{i,j,t} \times SURP_{i,j,t} + \beta_{10} BM_{i,j,t} \times SURP_{i,j,t} \\ & + \beta_{11} DISPERSION_{i,j,t} \times SURP_{i,j,t} + \beta_{12} |SURP_{i,j,t}| \times SURP_{i,j,t} + e_{i,j,t}. \end{aligned} \tag{6}$$

More volatile firms (*VOLATILITY*) are likely to have noisier earnings estimates; larger firms (*SIZE*) have lower earnings response coefficients; and book-to-market (*BM*) is a measure of growth. We include *DISPERSION* because prior research (e.g., Burgstahler and Chuk 2010; Kim 2010) find that analyst forecast dispersion affects earnings response coefficients and include |*SURP*| to control for the nonlinearity in ERC as documented in Freeman and Tse (1992) and Kinney et al. (2002). We calculate *DISPERSION* as the standard deviation of individual forecasts scaled by the absolute value of the mean of the forecasts. For completeness we interact all control variables with *SURP*.

Table 6 provides the results of the two regressions. The results for Model (5) indicate that the earnings response coefficient (ERC) is 0.1687 for non-rounded forecasts and 0.1275 (0.1687 – 0.0412) for rounded forecasts. The ERC on rounded forecasts is approximately 24 percent lower than that on non-rounded forecasts (–0.0412/0.1687) and the difference is statistically significant. The final two columns of the table present the results for Model (6). Consistent with prior research, the results show that the earnings response coefficient is lower for large firms, firms with more dispersed forecasts, and for larger earnings surprises. More importantly, the coefficient β_3 remains negative and statistically significant. These results suggests that a rounded forecast is a more noisy proxy of the investors' expectations since investors respond less to forecast errors made by rounding analysts.

Note that in Table 1 we find that rounded forecasts are more optimistically biased than other forecasts. This indicates that, on average, actual earnings are lower than forecast earnings for rounding analysts. Since this optimistic bias is predictable *ex ante*, investors should be aware of this bias and not respond to the predictably negative earnings surprise based on rounded forecasts. Thus, assuming market efficiency, the coefficient on *ROUND* should be 0. However, the results in Table 6 indicate that the coefficient on *ROUND* is negative and significant ($\beta_2 = -0.0011$ or -0.0014).[14] This is consistent with investors not fully adjusting their expectations for the optimistic bias in rounded forecasts and being surprised by the negative earnings surprise. Thus, overall our results suggest that investors place less weight on forecasts made by rounding analysts than those made by other analysts, but that they do not fully adjust their expectations for the predictable optimism in rounded forecasts.

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[14] In untabulated results we find that the mean earnings announcement return is 0.14 percent for rounded forecasts and 0.30 percent for others. This suggests that the earnings announcement return is about 0.16 percent lower for rounded forecasts than other forecasts, which translates to annualized return difference of about 13.33 percent. However, given the short holding period and high portfolio turnover, this *ex ante* predictable return difference could be difficult to exploit after taking into consideration trading costs.

TABLE 6

An Examination of Investor Response to Unexpected Earnings (Actual EPS Less Forecast EPS Scaled by Price) at Earnings Announcements and the Impact of Rounding

	Model (5)		Model (6)	
	Coeff.	t-statistic	Coeff.	t-statistic
Intercept	0.0029***	3.97	0.0070***	2.87
ROUND	−0.0011**	−2.04	−0.0014***	−3.60
SURP	0.1687***	4.12	0.7102***	3.62
ROUND × *SURP*	−0.0412**	−2.74	−0.0386**	−2.54
Control Variables				
VOLATILITY			−0.0062	−0.41
SIZE			−0.0004**	−2.20
BM			0.0009	0.64
DISPERSION			−0.0015	−0.96
VOLATILITY × *SURP*			−0.0166	−0.08
SIZE × *SURP*			−0.0351***	−3.07
BM × *SURP*			0.0098	0.33
DISPERSION × *SURP*			−0.0913*	−1.65
|*SURP*| × *SURP*			−2.9515**	−2.55
Average n per year	27,920		17,456	
Average R^2 (%)	0.60		1.80	

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*, **, *** Indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

This table reports the regression results of the following model:

$$\begin{aligned} RET_{j,t} = {} & \beta_0 + \beta_1 SURP_{i,j,t} + \beta_2 ROUND_{i,j,t} + \beta_3 ROUND_{i,j,t} \times SURP_{i,j,t} + \beta_4 VOLATILITY_{i,j,t} + \beta_5 SIZE_{i,j,t} \\ & + \beta_6 BM_{i,j,t} + \beta_7 DISPERSION_{i,j,t} + \beta_8 VOLATILITY_{i,j,t} \times SURP_{i,j,t} + \beta_9 SIZE_{i,j,t} \times SURP_{i,j,t} \\ & + \beta_{10} BM_{i,j,t} \times SURP_{i,j,t} + \beta_{11} DISPERSION_{i,j,t} \times SURP_{i,j,t} + \beta_{12} |SURP_{i,j,t}| \times SURP_{i,j,t} + e_{i,j,t}. \end{aligned}$$

We estimate the regressions using the Fama-MacBeth approach with Newey-West correction for autocorrelations for 1 year lag.

Variable Definitions:
$RET_{j,t}$ = size-adjusted stock returns over the three days surrounding earnings announcement for stock *j* for year *t*;
$SURP_{i,j,t}$ = actual earnings less the most recent forecast made by analyst *i* for firm *j* scaled by price; and
$ROUND_{i,j,t}$ = an indicator variable that takes the value of 1 if a forecast ends with a 0 or 5, and 0 otherwise.
Control Variables Included in the Regression:
$VOLATILITY_{i,j,t}$ = standard deviation of monthly returns over the 12 months prior to forecast date;
$SIZE_{i,j,t}$ = logarithm of market cap;
$BM_{i,j,t}$ = ratio of book value of equity to market cap;
DISPERSION = forecast dispersion as of the month before a forecast was issued;
$|SURP_{i,j,t}|$ = absolute value of $SURP_{i,j,t}$.

VI. CONCLUSION

We examine analysts' tendency to round forecasts to the nearest 0 or 5 cents. We conjecture that analysts *choose* the level of effort that they put into forecasting and that this choice influences their decision to round. The less effort analysts exert in forecasting earnings, the more uncertain they are about the penny digit of the forecast and the more likely they are to round.

We suggest that an intuitive reason that an analyst may choose not to exert effort in forecasting with "to the penny" precision exists in cases where firms have larger EPS numbers. As the level of EPS increases: (1) the economic importance of the penny digit declines and so is less relevant to investors; and (2) the precision of the analyst's information is not as likely to hold to the penny. We show that rounding is an increasing function of the level of EPS. Actual EPS displays no such rounding with the proportion of 0s and 5s remaining at 20 percent.

Our next tests examine whether compensation incentives influence analysts' incentives to round. We suggest that analysts are likely to exert less effort in forecasting earnings for firms that are of less interest to investors and so generate less brokerage business. We also suggest that analysts will exert less effort in firms that are unlikely to be raising new financing since these firms are unlikely to become investment-banking clients. Our results are consistent with these predictions. We show that rounding occurs more frequently in low-growth firms, in firms with low trading volume and low institutional holdings, and in firms that are not accessing capital markets for new financing. We also examine whether rounding is more likely to occur when the firm being followed has a more complex business. This could occur because, relative to a less complex firm, the analyst has to exert more effort to understand the business. Our results suggest that rounding is more likely in larger firms and in firms with more business segments.

We investigate the stock price response to rounded forecasts to determine whether researchers could use "rounding" as a simple proxy for lower information content of the forecast. Our results indicate that investors respond less to "unexpected earnings" (actual minus forecast earnings) when the forecast is rounded. This is consistent with a rounded forecast being a noisier proxy of the market's expectation of earnings. We also find that, on average, rounded forecasts have a small but significantly lower announcement return than other forecasts. This suggests that the optimistic bias in rounded forecasts may not be always fully anticipated by investors.

Our paper offers several avenues for future research. Do management disclosures affect the extent of rounding since this could reduce the cost to the analyst of providing a more accurate forecast? As analysts' incentives change, do we observe changes in rounding behavior? How have changes in the information environment (e.g., the ability of analysts to observe other analysts' forecasts) affected incentives to round? Are there ways to better separate true 0s and 5s from rounded 0s and 5s so that we could better predict when and where analysts are exerting more or less effort? This, in turn, could help researchers and investors better forecast future earnings and lead to better predictions on how investors will respond to earnings.

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APPENDIX A

DEFINITIONS OF ANALYST CHARACTERISTIC VARIABLES

$LAGACCU_{i,j,t}$ = accuracy of analyst *i*'s most recent forecast of firm *j*'s earnings in year *t*–1. It is calculated as the maximum absolute forecast error for analysts following firm *j* in year *t*–1 minus the absolute forecast error of analyst *i* following firm *j* in year *t*–1, scaled by the range of absolute forecast errors for analysts following firm *j* in year *t*–1;

$BROKERSIZE_{i,j,t}$ = broker size, calculated as the number of analysts employed by the broker employing analyst *i* following firm *j* in year *t* minus the minimum number of analysts employed by brokers for analysts following firm *j* in year *t*, scaled by the range of broker size for analysts following firm *j* in year *t*;

FOR_FREQ$_{i,j,t}$ = forecast frequency, calculated as the number of firm *j* forecasts made by analyst *i* in year *t* minus the minimum number of firm *j* forecasts for analysts following firm *j* in year *t*, scaled by the range of the number of firm *j* forecasts issued by analysts following firm *j* in year *t*;

EXPERIENCE$_{i,j,t}$ = analyst's firm experience, calculated as the number of years of firm-specific experience for analyst *i* following firm *j* in year *t* minus the minimum number of years of firm-specific experience for analysts following firm *j* in year *t*, scaled by the range of years of firm-specific experience of analysts following firm *j* in year *t*;

HORIZON$_{i,j,t}$ = forecast horizon, calculated as the number of calendar days from forecast date to the earnings announcement date for analyst *i* following firm *j* in year *t* minus the minimum forecast horizon for analysts following firm *j* in year *t*, scaled by the range of forecast horizon for analysts following firm *j* in year *t*;

FIRM_FOL = number of firms followed, calculated as the number of firms followed by analyst *i* following firm *j* in year *t* minus the minimum number of firms followed by analysts following firm *j* in year *t*, scaled by the range of the numbers of firms followed by analysts following firm *j* in year *t*; and

IND_FOL$_{i,j,t}$ = number of industries followed, calculated as the number of industries followed by analyst *i* following firm *j* in year *t* minus the minimum number of industries followed by analysts following firm *j* in year *t*, scaled by the range of the numbers of industries followed by analysts following firm *j* in year *t*.

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The Timeliness of Bad Earnings News and Litigation Risk

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ABSTRACT: This study investigates whether the timely revelation of bad earnings news is associated with a lower incidence of litigation. The timeliness of earnings news is captured by a new measure based on the evolution of the consensus analyst earnings forecast. Holding total bad earnings news and other determinants of litigation constant, we find that earlier revelation of bad earnings news lowers the likelihood of litigation. This result holds for both settled and dismissed lawsuits. Further, we reconcile our findings with prior work that measures timeliness using managerial warnings via press releases. These tests suggest our findings are attributable to the ability of our timeliness measure to capture bad earning news revealed through disclosure channels beyond press releases.

Keywords: *securities litigation; disclosure; analyst forecast; earnings news.*

Data Availability: *Data are available from public sources identified in the paper.*

JEL Classifications: *K22; K41; M41.*

I. INTRODUCTION

This study examines the relation between the timeliness of bad earnings news and the incidence of securities litigation. Skinner (1994) proposes that the earlier revelation of bad earnings news reduces the expected costs of litigation because earlier revelation diminishes

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the perception that management "hid the truth" and reduces damages by shortening the class period. This litigation reduction hypothesis predicts that timelier revelation of bad earnings news should reduce the likelihood of being sued and/or the costs of resolving lawsuits that do occur.

Prior work investigates the litigation reduction hypothesis using measures of the timeliness of bad earnings news based on management earnings "warnings" or "pre-announcements" via a press release. Early studies, such as Francis et al. (1994) and Skinner (1997), find that quarters triggering litigation are actually more likely to contain earnings warnings. Skinner (1997) suggests this result may be driven by endogeneity, as firms with worse earnings news have higher incentives to warn because of higher litigation risk. Skinner (1997) finds that earlier disclosure is associated with lower settlements after controlling for estimated shareholder damages. Field et al. (2005) explicitly control for endogeneity and find no relation between earnings warnings and the incidence of litigation in general, but document a negative relation between earnings warnings and the incidence of litigation after dismissed suits are excluded from their analysis.

We construct a new measure of the timeliness of *total* bad earnings news, which captures how quickly earnings news is revealed to the market using the evolution of analysts' consensus earnings forecasts. This new measure allows for a stronger test of the litigation reduction hypothesis for two reasons. First, our measure captures *all* bad earnings news revealed to the market, including instances where there are multiple news events. Earnings warnings delivered in companies' press releases are only one way in which earnings news can be revealed to the market. Alternative methods include analyst conference calls, presentations, webcasts, private communications (at least before Regulation FD), and analyst research. Liability under U.S. securities laws does not depend on any particular form or source for revelations that correct an inflated stock price. Thus, the litigation reduction hypothesis holds as long as total bad earnings news is revealed on a timelier basis, regardless of the method of revelation. Second, our measure is constructed using machine-readable data sources, allowing for larger sample tests and eliminating the need for costly and time-consuming hand collection of disclosure data (e.g., press release warnings).

Analysts' consensus forecasts are arguably the best available proxy for the market earnings expectation. We measure earnings news timeliness for the quarter triggering lawsuits by computing the proportion of total earnings news revealed up to each day in the quarter and then taking the average of the daily proportions. For example, assume the ultimate earnings revelation in a quarter is \$0.10 per share, the beginning forecast is \$0.50 per share, and the forecast on a given day is \$0.30. The proportion of the total news revealed up to that day is 50 percent (i.e., (0.50 – 0.30)/(0.50 – 0.10)). We calculate this proportion for each day and then compute the average across all days in the quarter. Thus, our timeliness measure captures the average daily proportion of total earnings news revealed to the market during the quarter triggering litigation.

Our sued sample consists of 423 securities class action lawsuits from 1996–2005. Consistent with prior research (e.g., Field et al. 2005; Johnson et al. 2007), we use a matched sample research design. Our first matched sample contains non-sued firms matched with sued firms based on total earnings news and disclosure window length. This ensures that sued and non-sued firms have similar total earnings news over the same time period, which helps control for the endogeneity noted by Skinner (1997). Specifically, for each sued firm-quarter (i.e., the quarter triggering the lawsuit), we select a non-sued firm from the same quarter with the closest total earnings news (i.e., the ultimate earnings revelation less the beginning-of-quarter consensus forecast, scaled by beginning-of-quarter stock price). Our second matched sample utilizes the predicted probability of litigation. Specifically, we estimate a litigation prediction model that expresses the probability of being sued as a function of economic determinants. Then, for each sued firm-quarter, we select the non-sued firm from the same quarter with bad earnings news and the closest predicted litigation probability.

Using each of the two matched samples, we find that timelier revelation of bad earnings news is associated with a lower threat of litigation. This negative relation applies to both settled and dismissed suits. In addition, our results are robust to controlling for numerous determinants of litigation risk, such as firm size, stock returns over the litigation quarter, the largest negative one-day return during the litigation quarter, and insider selling. Furthermore, our results are similar if we use alternative matching methods, such as matching on market capitalization and stock returns over the litigation quarter or matching on industry and the largest negative one-day stock return over the litigation quarter. These alternative matching methods provide evidence that our results are not driven by sued firms having larger stock price drops over the litigation quarter or over a short window. In addition, our results are not driven by innate differences between sued and non-sued firms. We find similar patterns when we benchmark sued firms against themselves by examining the speed of news revelation in non-litigation quarters.

We also reconcile our findings with those of prior studies by repeating our test with a timeliness measure based on managerial earnings warnings. We hand-collect data on management earnings warnings via press releases from Factiva and Lexis-Nexis for a random subsample of firms with large negative total earnings news. Following Field et al. (2005), we measure disclosure timeliness using an indicator variable for the presence of an earnings warning prior to the earnings announcement. Consistent with prior studies (e.g., Francis et al. 1994; Field et al. 2005), we find that sued firms are more likely to issue earnings warnings than non-sued firms. However, we find that bad earnings news is revealed earlier for non-sued firms than for sued firms when our new measure of the timeliness of total earnings news is used. This result suggests that the difference between our findings and those of prior studies is attributable to the ability of our timeliness measure to capture news revelation beyond management press releases.

This study makes three primary contributions. First, we provide additional evidence on Skinner's (1994) litigation reduction hypothesis. Our findings complement and extend those of Field et al. (2005) by providing new evidence that timelier revelation of bad earnings news deters not only suits that are eventually settled, but also those that are dismissed by the court. The finding regarding dismissed suits is particularly important because these are cases where the courts find that the factual allegations do not convincingly lead to an inference of fraud. Exposure to such litigation is arguably the very type of litigation that, from a policy perspective, we may want timely revelation of bad earnings news to preempt. Second, our results demonstrate the importance of considering multiple revelation channels when studying managerial disclosure. Our findings suggest that examining only press releases yields an incomplete picture of managerial disclosure and can lead to erroneous inferences.

Third, we construct a new measure of timeliness that captures how quickly total bad earnings news is revealed to the market. This measure is available on a firm-year or firm-quarter basis, based on machine-readable data, and explicitly focused on earnings news. This measure can be used in future research to investigate the impact of timely earnings news revelations. For example, this measure could be used to examine whether the market reacts differently to the timely revelation of earnings news (holding the magnitude of news constant), or whether the timeliness of earnings news affects firm/managerial reputation, the cost of capital, or information asymmetries between managers and outsiders. In addition, the measure could be used to test changes in the information environment due to regulations, such as Regulation FD, that might impact disclosure practices.

Section II discusses the institutional background and related literature. Section III describes the research design and reports results. Section IV concludes.

II. INSTITUTIONAL BACKGROUND AND RELATED LITERATURE

Securities litigation involving bad earnings news generally alleges violations of Rule 10b-5. The elements of a Rule 10b-5 violation are "(1) a misstatement or omission of (2) a material fact (3) made with intent (4) that the plaintiff justifiably relied on (5) causing injury in connection with the purchase or sale of securities" (Skinner 1994). Suits involving bad earnings news, therefore, allege either an *omission* (i.e., the failure to disclose earnings news on a timely basis) and/or a *misstatement* (i.e., an incorrect earlier forecast or statement of fact), either of which may satisfy element (1). A significant negative stock price reaction to the news demonstrates both materiality (element (2)) and causation (element (5)). Element (4) is generally presumed under the fraud on the market theory (see Francis et al. 1994).

In order to survive a motion to dismiss under the Private Securities Litigation Reform Act of 1995 (PSLRA), the plaintiffs' complaint must allege facts that lead to a strong inference of fraudulent intent. Thus, earnings disclosure cases often turn on whether the alleged omission or misstatement was *intentional*. Under the high pleading standard of the PSLRA, a mere stock price drop accompanying the release of bad news is insufficient to withstand a motion to dismiss. Rather, the circumstances surrounding the alleged omission or misstatement are critical and the appearance that managers are trying to hide information is beneficial to plaintiffs. Likely because they must allege facts that give a strong inference of intent, plaintiffs generally present as many facts as possible to demonstrate that it is implausible that managers were ignorant of the bad news prior to its revelation and commonly allege both omissions and misstatements.

The first study to formally propose that the timelier revelation of bad earnings news could reduce the expected costs of litigation is Skinner (1994). Skinner (1994) and several later studies (e.g., Skinner 1997; Field et al. 2005; Healy and Palepu 2001) outline three factors that support the litigation reduction hypothesis. First, timely disclosure weakens the claim that managers (1) withheld bad earnings news they should have disclosed and/or (2) made false statements in light of this undisclosed news to keep the share price inflated. Although plaintiffs can rarely prove when management first learned of bad earnings news, the late revelation of bad news helps create the *ex post appearance* that managers had "something to hide." The late revelation of bad news enhances the credibility of plaintiffs' claims that managers kept the share price inflated until the fraud could no longer be maintained, which is a common claim in disclosure lawsuits. Second, timely disclosure shortens the nondisclosure period, reducing the size of the plaintiff class and potential damages in a lawsuit. Third, timely disclosure may mitigate large negative stock price reactions, which are a trigger for litigation. Disclosing part of the bad news earlier spreads the bad news out over time and helps avoid a large, one-time price drop. In addition, timely disclosure may enhance managers' perceived competence or credibility, engendering a less severe negative stock price reaction.[1]

Several studies (Francis et al. 1994; Skinner 1997; Field et al. 2005) examine the litigation reduction hypothesis using managerial earnings warnings or preannouncements via press releases to measure the timeliness of management disclosure of bad earnings news.[2] These studies use the presence of an earnings warning prior to the earnings announcement (Field et al. 2005), whether the earnings warning is issued before or after the quarter-end (Francis et al. 1994; Skinner 1997), or the number of days between the press release date and a benchmark point (i.e., quarter-end, quarter-beginning, or earnings announcements) (Francis et al. 1994; Skinner 1997) as their measures of the timeliness of bad earnings news.[3] The key common factor in these studies is that all of the measures are contingent on an explicit earnings warning.

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[1] In addition to Rule 10b-5, firms may also be liable under Section 11 of the Securities Act of 1933 in cases where they are issuing securities. While Section 11 has a lower intent standard, the second and third reasons for the litigation hypothesis still hold in a Section 11 context. As such, we include these cases in our primary analysis, but find in supplementary analysis that results are virtually identical when these cases are excluded.

[2] A related line of research examines how the risk of shareholder litigation affects managers' disclosure choices (e.g., Skinner 1994; Kasznik and Lev 1995; Baginski et al. 2002; Rogers and Van Buskirk 2009).

[3] One exception is Francis et al. (1994), who also examine earnings disclosures through analyst reports, and find that managers sometimes disclose news to analysts who then disseminate the information in the pre-Regulation FD environment. Francis et al. (1994) also examine analyst forecasts for a small sample of firms to test whether analysts anticipate negative earnings news to a larger extent for non-sued firms than for sued firms. They find no such evidence. However, they do not examine the timing of the forecast revisions during the litigation quarter.

Francis et al. (1994) find that sued firms issue voluntary earnings disclosures prior to the earnings announcement at a significantly higher rate than non-sued firms. This result appears inconsistent with the litigation reduction hypothesis. Skinner (1997) confirms the main finding of Francis et al. (1994) with a different design. He compares press release disclosure patterns of sued firms during quarters that triggered litigation to their disclosure practices in non-litigation quarters and finds that earnings warnings via press releases are much more common in litigation quarters than in non-litigation quarters. However, Skinner (1997) notes that endogeneity could drive the positive relation between disclosure timeliness and litigation risk. In other words, firms with worse bad earnings news have stronger incentives to issue earnings warnings *because* they face higher litigation risk. After controlling for estimated shareholder damages, Skinner (1997) finds some evidence that timelier disclosure is associated with lower settlement amounts, consistent with the litigation reduction hypothesis. Using a simultaneous equation approach to address the endogeneity problem noted by Skinner (1997), Field et al. (2005) find no significant relation between earnings warnings and litigation risk when examining all suits. After excluding dismissed suits, Field et al. (2005) find that earnings warnings are negatively related to the incidence of litigation, consistent with the litigation reduction hypothesis.

Overall, Skinner's (1997) findings with respect to settlements and the Field et al. (2005) findings with respect to the incidence of settled suits provide some support for the litigation reduction hypothesis. However, there is no evidence to date that early disclosure of bad earnings news helps to prevent non-meritorious litigation (i.e., litigation that would be dismissed if filed), which is an important type of litigation to deter.

In this study, we offer several reasons why it is important to consider total earnings news when testing the litigation reduction hypothesis. First, press releases are only one channel through which managers can reveal bad earnings news to the market. Managers can also reveal negative earnings news through public disclosure channels such as conference calls (Tasker 1998; Frankel et al. 1999), presentations at conferences (Bushee et al. 2009), and company websites (Deller et al. 1999). Further, anecdotal evidence and academic research indicate that, at least prior to Regulation FD, managers often engaged in private communication with institutional investors and financial analysts through closed conference calls, one-on-one meetings, or phone conversations.[4] Analysts also conduct research through industry analysis and the polling of customers and suppliers, which can lead to the revelation of earnings news. Finally, whistle-blowing by a former employee or a press article may reveal news to the market.

Second, the bad news revelation is not the source of legal liability under U.S. securities laws. Rather, the prior failure to disclose when a duty to disclose exists (an omission) or a prior misstatement (e.g., a misleading or knowingly false earnings projection) exposes firms to litigation.[5] The revelation leading to litigation does not need to come from management or take any particular form (e.g., an explicit warning). Rather, a wide array of disclosure sources and forms may reveal the alleged fraud and result in litigation. For example, Dyck et al. (2010) detail numerous cases in which external whistleblowers revealed the fraud.

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[4] Managers continue to conduct one-on-one meetings with analysts and institutional investors post-Regulation FD. For example, in a survey by Rivel Research (NIRI 2001), 79 percent of the 577 National Investor Relations Institute member companies claim that they hold one-on-one meetings to the same or greater extent post-Regulation FD.

[5] There is no overarching duty to disclose all material information immediately (e.g., *In re Parametric Technology Corp. Securities Litigation*, 300 F. Supp. 206 (D. Mass. 2001); Rogers and Van Buskirk 2009). However, if a firm decides to disclose information, it must disclose the whole truth such that the disclosure is not misleading (Rogers and Van Buskirk 2009). In addition, if a firm makes an initial disclosure, many courts have extended the "whole truth" doctrine to provide a "duty to update" if the information is still relevant to market participants (e.g., Langevoort and Gulati 2004). Finally, a duty to disclose arises when corporate insiders trade based on material, non-public information (e.g., *San Leandro Emergency Med. Plan v. Philip Morris*, 75 F. 3d 801 (2d Cir. 1996)).

Finally, the importance of taking total earnings news into account is also supported by the materiality requirement of Rule 10b-5, which considers the total "information mix" available to the market (see *Basic, Inc. v. Levinson*, 485 U.S. 224 1988; Francis et al. 1994). Whether any managerial statement or failure to disclose is misleading is determined in the context of the total information available to the market, including information from non-management sources. Non-timely bad news revelation of any form, such as a warning, a conference call, or a press article simply leads to the *appearance* that management had "something to hide," which may make plaintiffs' allegations of *intentional* omissions or prior misstatements by management seem more credible. Thus, the timeliness of total bad earnings news available to the market is likely of the highest importance with respect to the litigation reduction hypothesis.

Unfortunately, it is difficult to gather data regarding many of these revelation channels, and some are simply unobservable. For example, conference call and conference presentation transcripts became available only recently, and managers' private communication with financial analysts is largely unobservable. Fortunately, analysts' earnings forecasts are a widely used proxy for market expectations and provide a measure of all earnings news that reaches market participants (e.g., Bartov et al. 2002). Analysts' forecasts incorporate information developed through independent analyst research, as well as information released in all disclosures, including those otherwise unavailable to researchers, such as private communication with managers. Thus, this proxy is a broad measure that captures the timeliness of total earnings news during the litigation quarter and complements prior research that examines official press releases.

In our empirical analysis below, we examine two related issues. First, we examine whether the timelier revelation of total bad earnings news is linked to lower litigation risk. Second, we examine the source of the information that drives analyst forecast revisions, which generate our measure of the timeliness of total earnings news.

III. SAMPLE, RESEARCH DESIGN, AND EMPIRICAL RESULTS

Sued Sample

We use the Securities Class Action Services (SCAS) database from RiskMetrics to identify all class action securities lawsuits filed in Federal court from 1996–2005. We end our sample in 2005 because our later tests require data on settlement decisions, which generally take several years to resolve. Table 1 summarizes our sample selection process. We require lawsuits to allege losses in connection with common equity and defendant firms must have data available for our tests on CRSP and Compustat. Similar to Field et al. (2005), we exclude cases that involve restatements of prior financial statements (i.e., accounting irregularities) because it is less likely that timely disclosure of, say, fictitious revenue will deter litigation.

Since we measure earnings news and its timeliness using analyst forecasts, we require firms to have data available in I/B/E/S. We further require firms to have unfavorable (i.e., negative) earnings news in the fiscal quarter that triggered the lawsuit.[6] We define earnings news as the "earnings

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[6] We collected a random sample of 20 lawsuits for which there was no indication of bad news using the evolution of the analyst consensus forecast from I/B/E/S. Ninety percent of these cases involved issues other than the disclosure of bad earnings news, such as IPO "laddering" cases or results of FDA drug trials. The remaining two cases did appear to involve some disclosure of bad earnings news before the earnings announcement, but these firms had limited analyst coverage, so changes in the consensus analyst forecast per I/B/E/S did not reflect this news. Importantly, however, these disclosures occurred late in the quarter (87 percent of the quarter elapsed on average since the last announcement date), so our screen does not appear to exclude timely revelation lawsuits and does not bias our sample in favor of lawsuits with late revelation of news.

TABLE 1
Sample Selection

All lawsuits filed from 1996 to 2005	2,456
Less: Firms without data from CRSP and Compustat	(813)
	1,643
Less: Suits involving a restatement	(335)
	1,308
Less: Firms without necessary data from I/B/E/S	(463)
	845
Less: Suits without bad earnings news in litigation quarter	(422)
Final Sued Sample (firm-quarters)	423

revelation" at the end of the class period (the period over which fraud is alleged) minus the first consensus forecast of the quarter, scaled by stock price at the beginning of the quarter. We measure the first consensus forecast of the quarter at least one day after the announcement of prior quarter earnings. We do this to ensure forecasts have been updated for the prior quarter's earnings announcement, but results are similar if we measure the first consensus forecast at least two weeks after the prior quarter announcement date. For cases where the end of the class period coincides with the official earnings announcement date (about 17 percent of cases), we define the "earnings revelation" as actual earnings in I/B/E/S. For all other cases, the "earnings revelation" is defined as the consensus forecast at the end of the class period.[7]

We refer to the time from the beginning consensus forecast to the end of the class period as the "quarterly revelation window." For cases where the class period ends on the earnings announcement date, the median length of the quarterly revelation window is 91 days. For all other cases, the median length of this window is 61 days. Overall, our final sued sample contains 423 sued firm-quarters that had unfavorable earnings news in the litigation quarter. Since few firms in our sample (15 out of 408 unique firms) have more than one suit, we often refer to firm-quarters as "firms" throughout the remainder of the study for brevity.

Matched Samples

Following prior work on litigation risk (e.g., Field et al. 2005; Johnson et al. 2007), we employ a matched-pairs design using sued firms and similar non-sued firms. A matched-pairs design yields two benefits in our setting versus the alternative of using the universe of firms. First, in some of our tests to follow, we hand-collect data regarding earnings warnings and information from analyst research reports. Resource and time constraints prevent us from collecting these data for the universe of firms. Drawing upon a matched sample of similar sued and non-sued firms therefore facilitates our data-collection efforts.

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[7] When the class period does not end at the earnings announcement date, we measure the consensus up to five days after the end of the class period to allow for a lag in the incorporation of bad news into analysts' forecasts. The results hold separately for both the suits whose class periods end before the earnings announcement dates and those whose class periods end at the earnings announcement dates.

Second, using a matched design yields a comparable set of non-sued control firms that plausibly could have been sued, but were not. Obtaining a set of non-sued firms similar to our sued firms is critical because *ex ante* litigation risk may endogenously affect firms' decisions to release news on a timely basis. For example, firms with low *ex ante* litigation risk may have little incentive to release bad news in a timely fashion (Skinner 1997). Simply adding controls for litigation risk in a regression using the universe of firms is problematic because litigation risk is nonlinear and is concentrated in a fairly small subsample of firms (Nelson and Pritchard 2008). A matched pairs design allows us to overcome this issue by explicitly matching sued firms with similar non-sued firms. We use two matched control samples of non-sued firms, one matched on total earnings news and one matched on total litigation risk.

Earnings News-Matched Sample

Since our focus is on the timeliness of bad earnings news, our first control sample is matched on total bad earnings news. Prior studies that test the litigation reduction hypothesis often select matching control observations using bad earnings news (e.g., Francis et al. 1994; Field et al. 2005). Therefore, for each sued firm, we select a non-sued firm from the same quarter with the closest total bad earnings news. For lawsuits where the class period ends on the earnings announcement day, total earnings news for matched firms is measured as actual earnings minus the beginning consensus forecast, scaled by beginning stock price. For all other cases, total earnings news for the matched firm is measured as the "ending" consensus forecast less the beginning consensus forecast, scaled by beginning stock price. The ending consensus forecast for non-sued firms is measured by the same number of days after the prior quarter earnings announcement date as the ending consensus forecast for the corresponding sued firm. This procedure helps ensure that the length of quarterly revelation window is equal for sued and non-sued firms. Our final earnings news-matched control sample contains 423 matching non-sued firms with unfavorable earnings news of a similar magnitude to sued firms.

Probability-Matched Sample

Because factors other than bad earnings news also affect litigation risk, we use a second control sample of non-sued firms matched on overall litigation probability. Specifically, we estimate the following logistic regression:

$$\begin{aligned} PROB(SUED = 1) = F(\alpha &+ \beta_1 NEWS + \beta_2 SIZE + \beta_3 RET + \beta_4 BM + \beta_5 HIGHLIT \\ &+ \beta_6 VOLATILITY + \beta_7 SHARE\_TURN + \beta_8 FOLLOW + \beta_9 MIN\_RET \\ &+ \beta_{10} SKEWNESS + \beta_{11} INSIDER). \end{aligned} \tag{1}$$

SUED is an indicator variable equal to 1 for observations in our sued sample, and 0 for all non-sued firm-quarters with bad earnings news (the last consensus less than the first consensus) in the Compustat-CRSP-I/B/E/S universe. The covariates in Equation (1) are variables that affect litigation risk (see, e.g., Francis et al. 1994; Jones and Weingram 1996; Johnson et al. 2000; Palmrose and Scholz 2004; Field et al. 2005; Johnson et al. 2007; Nelson and Pritchard 2008). Detailed variable definitions are provided in Appendix A. In general, firms with higher market capitalization (*SIZE*), higher analyst following (*FOLLOW*), greater share turnover (*SHARE_TURN*), lower book-to-market (*BM*), and firms in high litigation risk industries (*HIGHLIT*) are more likely to be sued. In addition, firm-quarters with poor earnings news (*NEWS*), negative stock returns (*RET*), high return volatility (*VOLATILITY*), larger one-day negative returns (*MIN_RET*), more negative skewness (*SKEWNESS*), and higher abnormal insider selling

(*INSIDER*) are associated with higher litigation risk.[8] We find that all coefficients from Equation (1), except *HIGHLIT*, are significant at conventional levels in the predicted direction (untabulated).

For each sued firm, we select the non-sued firm from the same quarter that has negative earnings news and the closest predicted probability from Equation (1). Our final probability-matched control sample contains 423 matching non-sued firms with characteristics from Equation (1) similar to our sued sample.

Research Design

To test the litigation reduction hypothesis, we compare earnings news timeliness in our sample of sued firms to earnings news timeliness in our control samples on a univariate and multivariate basis. Our timeliness measure is defined as the average proportion of total news revealed up to a given day during the quarterly revelation window. To illustrate, assume that a hypothetical quarterly revelation window is only four days, the beginning consensus forecast is \$0.50 per share, and the ultimate earnings revelation is \$0.10 per share, implying total (unscaled) news of \$0.40 per share during the quarterly revelation window.

If the consensus forecast was revised to \$0.30 on the second day of the quarterly window, then 50 percent [(\$0.50 – \$0.30)/(\$0.50 – \$0.10)] of the total news had been revealed at that point. If the consensus forecast is revised to \$0.20 on day three of the quarterly window, then 75% [(\$0.50 – \$0.20)/(\$0.50 – \$0.10)] of the total news was revealed up to that day. Finally, if the consensus forecast is revised to \$0.10 on the final day of the revelation period, then 100 percent of the news was revealed at that point. Averaging these figures for each day yields our timeliness measure, which is 56 percent [(0% + 50% + 75% + 100%)/4] in this example. Hence, our measure captures the timeliness of the revelation of total news. We refer to this as *TIMELINESS* for each firm. Higher (lower) values of this variable imply more (less) timely revelation of bad earnings news.[9,10]

A potential, broader measure of total news is stock returns. Several prior studies utilize measures of timeliness based on stock returns over a given time period (e.g., Butler et al. 2007; Beekes and Brown 2006, 2007). However, utilizing returns has drawbacks that accompany the potential benefits. Most importantly, returns capture information beyond short-term earnings news. Utilizing a measure based on stock returns therefore makes it more difficult to determine the implications of our findings for managerial disclosure policy. This is particularly important given that prior accounting studies focus on litigation risk and bad *earnings* news disclosures. We therefore use earnings news revealed through analyst forecast revisions to provide evidence with respect to managerial disclosure choices. In untabulated tests, we find similar, but slightly weaker, results when we use stock returns to measure news timeliness.

Although we select matched control samples to try to neutralize key differences between sued and non-sued firms, we control for any remaining differences in a multivariate framework (Cram et al. 2009). Specifically, we estimate the following conditional logistic regression (variables are defined in Appendix A):

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[8] In addition to its relation to litigation, Billings (2008) finds insider trading is related to the timing of disclosures.

[9] We recognize that the analysts' consensus forecast may not always go down in a monotonic fashion. We find that 7 percent of sued firms and 14 percent of non-sued earnings-matched firms experience an "over-correction" at some point during the revelation window (i.e., analysts revise their forecast upward). However, the results are similar if we exclude these observations from the analyses.

[10] Our measure assumes that analysts revise their forecasts when new information is revealed to the market. This assumption is reasonable in instances where the firm has an active analyst following. However, we recognize that some firms have a fairly small, inactive analyst following (and in our setting, given their size, would be unlikely to face securities litigation). For these firms, our measure is less appropriate as it is unlikely to capture the timeliness of information revealed to the market.

$$PROB(SUED = 1) = F(\beta_1 TIMELINESS + \beta_2 NEWS + \beta_3 SIZE + \beta_4 RET + \beta_5 BM + B_6 HIGHLIT + \beta_7 VOLATILITY + \beta_8 SHARE\_TURN + \beta_9 FOLLOW + \beta_{10} MIN\_RET + \beta_{11} SKEWNESS + \beta_{12} INSIDER). \qquad (2)$$

We use conditional logistic regression because it is more appropriate for the matched-sample analysis that matches observations based on both outcome (i.e., the dependent variable) and selected control variables (see, e.g., Hosmer and Lemeshow 2000; Johnson et al. 2007; Cram et al. 2009; Armstrong et al. 2010). However, the results are similar if we use standard logistic regression (untabulated). In Equation (2), *SUED* is an indicator variable equal to 1 if an observation is in our sued sample. *SUED* is equal to 0 for observations in our matched samples. The litigation reduction hypothesis predicts that the coefficient on *TIMELINESS*, β_1, will be negative.

Main Results

Univariate Results

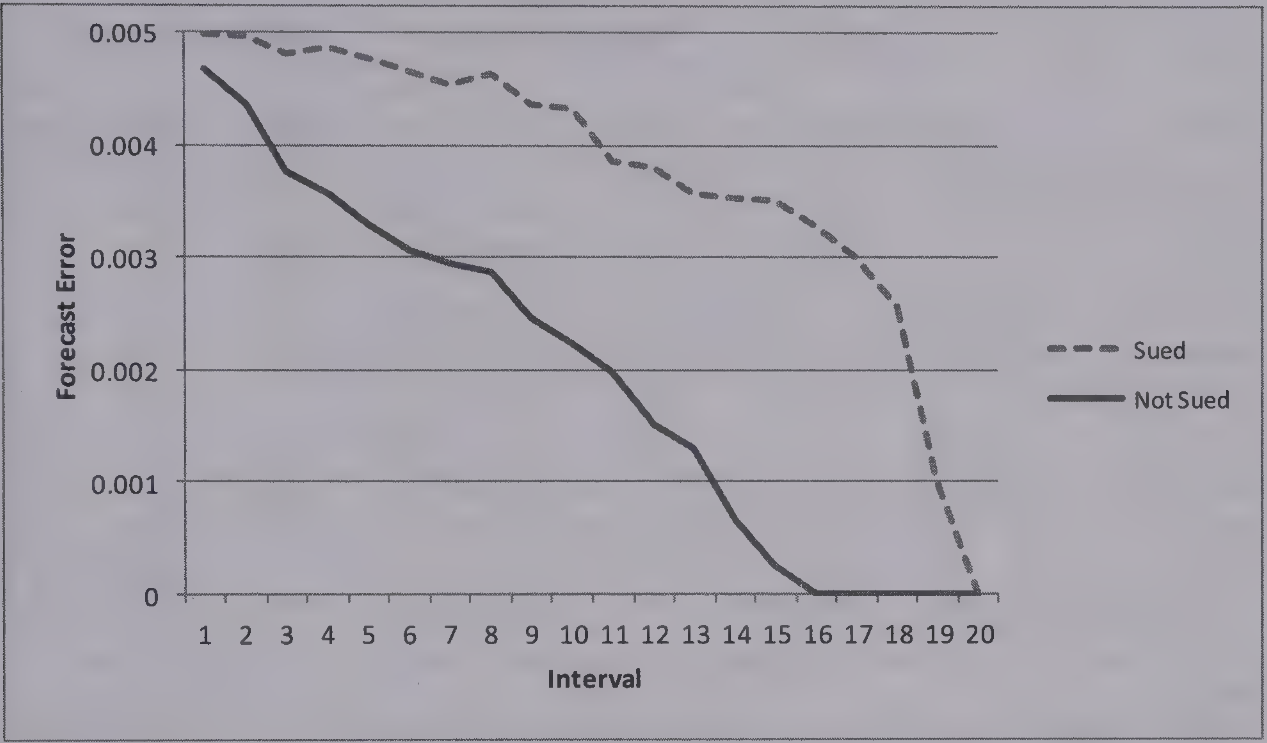
We begin with a graphical depiction of our main finding. For each sued and non-sued firm in our earnings news-matched sample, we divide the quarterly revelation window for each firm into 20 equal intervals. We then calculate the difference between the consensus forecast and the ultimate earnings revelation, scaled by stock price, at each interval. Figure 1 displays the evolution of the consensus for sued firms and non-sued firms matched on earnings news. By construction, total earnings news (as measured by the decline in the consensus) is nearly identical for sued and non-sued firms. However, Figure 1 reveals a stark difference in the news timing. Bad earnings news is revealed much earlier for firms that are not sued. For example, at the halfway point of the revelation window, over 55 percent of the bad news is revealed on average for non-sued firms, compared to less than 25 percent for sued firms (untabulated).

Table 2 contains descriptive statistics for our sued and non-sued samples. Consistent with the litigation reduction hypothesis, mean and median values of *TIMELINESS* are significantly higher for non-sued firms relative to sued firms. For the median earnings news- (probability)-matched control firm, a daily average of roughly 45 percent (35 percent) of total earnings news has been revealed throughout the quarter. For the median sued firm, the daily average of revealed news is about 12 percent. These differences are significant ($p < 0.01$).

As expected, there is no significant difference in total earnings news between sued firms and non-sued firm matched on earnings news. There is also no significant difference in earnings news between sued firms and non-sued firms matched on litigation probability. The mean level of bad earnings news for sued (non-sued) firms involves an earnings revelation that is 1.35 percent (1.13 percent to 1.17 percent) of market equity lower than the beginning consensus forecast. Other characteristics, such as size, returns, analyst following, etc. are significantly different between sued firms and control firms matched on earnings news. These differences, however, are generally not significant for control firms matched on litigation probability.

Although the probability-matched control sample is more effective in neutralizing differences in litigation determinants, we present findings using both control samples for the following reason. Some of our later tests involve hand-collection for sued and non-sued firms from managerial press releases and analyst research reports, and we want to ensure that the total magnitude of forecast revisions, and the window over which they are measured, is held constant across samples. Also, we want to ensure that the control sample used in these tests is fairly similar to those used in prior studies like Field et al. (2005). The control sample matched on earnings news is best suited for this purpose. Furthermore, as described in the subsection below entitled "Alternate Matched Control Samples," inferences regarding the effect of *TIMELINESS* on litigation risk are statistically and economically similar regardless of the control sample used.

FIGURE 1
Timeliness of Forecast Revisions in the Quarter Leading up to the Class Period End Date for Firms Sued and Firms Not Sued



We obtain all bad news class action securities lawsuits not involving a restatement filed in Federal court from 1996–2005 from the RiskMetrics Securities Class Action Services (SCAS) database. For each sued firm, we select a match firm from the same quarter with the closest total earnings news. Total earnings news is the bad earnings revelation at the end of the class period less first consensus forecast of the quarter, scaled by stock price at the beginning of the quarter. For each firm in our sample, we measure the difference between the consensus forecast each day and the ultimate earnings revelation, scaled by beginning stock price. This difference, or forecast error, measures the extent to which the consensus forecast at any given time reflects the total earnings news of the quarterly period. For each firm we then divide the quarterly revelation window into 20 equal intervals, and calculate the median forecast error across firms by interval.

Table 3 presents correlations among the variables in Equation (2). Panel A presents correlations for the earnings news-matched sample. *TIMELINESS* is strongly negatively correlated with *SUED* (corr. = –0.30, $p < 0.01$). Panel B presents correlation for the probability-matched sample. *TIMELINESS* is again strongly negatively correlated with *SUED* (corr. = –0.36, $p < 0.01$). Other correlations are consistent with the patterns in Table 2.

Multivariate Results

Table 4 reports the results of estimating Equation (2) using conditional logistic regression. Panel A reports the results using the control sample matched on earnings news. To facilitate interpretation, we utilize the quintile rank of the *TIMELINESS* variable to estimate Equation (2) instead of the raw *TIMELINESS* measure (results are similar if we use the raw measure). There is a negative relation between *TIMELINESS* and the probability of a lawsuit ($p < 0.01$), even after controlling for stock returns and other variables from Equation (2). To assess the economic

TABLE 2

Descriptive Statistics

		Sued			Not Sued Earnings News Match			Not Sued Probability Score Match		
Variable	**n**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**
TIMELINESS	423	0.23	0.12	0.32	0.45***	0.45***	0.38	0.37***	0.35***	0.32
NEWS	423	−1.35	−0.45	3.08	−1.13	−0.44	2.32	−1.17	−0.50	1.99
SIZE	423	7.07	6.82	1.77	6.27***	6.10***	1.69	7.07	6.89	1.63
BM	423	0.42	0.30	0.43	0.72***	0.57***	0.69	0.43	0.32	0.43
RET	423	−38.88	−38.81	24.70	−9.69***	−5.76***	24.45	−36.59	−40.78	24.03
HIGHLIT	423	0.51	1.00	0.50	0.29***	0.00***	0.45	0.49	0.00	0.50
SHARE_TURN	423	1.44	1.05	1.23	0.74***	0.51***	0.77	1.42	1.05	1.15
VOLATILITY	423	4.26	3.70	2.27	3.54***	2.98***	2.06	4.13	3.82	2.12
FOLLOW	423	9.25	7.00	6.90	6.09***	4.00***	4.93	8.66	7.00*	6.16
MIN_RET	423	−26.42	−23.98	15.32	−10.32***	−7.41***	9.40	−24.69*	−25.00	12.03
SKEWNESS	423	−1.73	−1.53	2.01	−0.05***	0.08***	1.20	−1.76	−1.79	1.61
INSIDER	423	0.55	0.00	3.77	−0.15***	0.00***	2.63	0.36	0.00**	2.57
PROB_LIT	423	0.08	0.06	0.07	—	—	—	0.08	0.06	0.06

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*, **, *** Indicate t-tests and median tests of differences across groups significant at $p < 0.10$, 0.05, and 0.01, respectively.

We obtain all class action securities lawsuits not involving a restatement filed in Federal court from 1996–2005 from the RiskMetrics Securities Class Action Services (SCAS) database. For each sued firm, we select two sets of match firms. The match firm in the earnings news-match sample is selected from the same quarter with the closest total earnings news. The match firm in the litigation probability-match sample is selected based on a probability match procedure where the match is the non-sued firm with the closest predicted probability of litigation (using determinants from prior literature) in the quarter triggering litigation for sued firms.

PROB_LIT is the predicted probability of litigation using the litigation probability-match sample. Descriptive statistics are tabulated for the sample of sued firms and the two respective control samples.

Variables are defined in Appendix A.

significance of our findings, we include marginal effects for each parameter estimate, which are akin to slope coefficients in an OLS regression. Marginal effects vary across observations, depending on the predicted probability of being sued. To gauge the "average" marginal effect, we first compute the marginal effect for each observation in the sample and then calculate the sample mean of individual marginal effects. Results are similar if we compute the sample median of individual marginal effects.

Table 4 indicates that a one unit increase in ranked *TIMELINESS* (i.e., moving up from one quintile to the next in our *TIMELINESS* measure) is associated with a decrease in the probability of a lawsuit of 2.7 percentage points. In interpreting the marginal effect, it is important to note that the unconditional probability of a lawsuit in our sample is 50 percent, rather than the substantially lower rate present in the full population, due to our matched-pair design.

Parameter estimates for the control variables are generally significant in the predicted direction. Exceptions include stock price volatility and return skewness, which are insignificant at conventional levels. The pseudo R^2 is 39.5 percent, indicating that the covariates help explain the incidence of lawsuits reasonably well.

Table 4, Panel A also provides the results from estimating Equation (2) separately for dismissed and settled lawsuits based on the final disposition of the case, each with their respective

TABLE 3

Pearson Correlations

Panel A: Pearson Correlations among the Lawsuit Indicator Variable (*SUED*), *TIMELINESS*, and Other Control Variables Using the Earnings News Match Sample as the Control Group (p-values are in parentheses)

	SUED	*TIMELINESS*	*NEWS*	*SIZE*	*BM*	*RET*	*HIGHLIT*	*SHARE_TURN*	*VOLATILITY*	*FOLLOW*	*MIN_RET*	*SKEWNESS*
TIMELINESS	−0.30											
	(<0.01)											
NEWS	−0.04	0.13										
	(0.24)	(<0.01)										
SIZE	0.23	−0.05	0.29									
	(<0.01)	(0.14)	(<0.01)									
BM	−0.26	0.11	−0.15	−0.25								
	(<0.01)	(<0.01)	(<0.01)	(<0.01)								
RET	−0.51	0.22	0.16	−0.01	0.21							
	(<0.01)	(<0.01)	(<0.01)	(0.80)	(<0.01)							
HIGHLIT	0.22	−0.09	−0.01	−0.02	−0.19	−0.23						
	(<0.01)	(0.01)	(0.87)	(0.56)	(<0.01)	(<0.01)						
SHARE_TURN	0.32	−0.12	−0.01	0.02	−0.14	−0.31	0.33					
	(<0.01)	(<0.01)	(0.80)	(0.58)	(<0.01)	(<0.01)	(<0.01)					
VOLATILITY	0.16	0.02	−0.18	−0.28	−0.03	−0.29	0.35	0.51				
	(<0.01)	(0.50)	(<0.01)	(<0.01)	(0.37)	(<0.01)	(<0.01)	(<0.01)				
FOLLOW	0.26	−0.10	0.15	0.71	−0.14	−0.10	0.11	0.21	−0.06			
	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(0.10)			
MIN_RET	−0.54	0.24	0.14	0.10	0.19	0.75	−0.25	−0.26	−0.27	0.00		
	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(0.97)		
SKEWNESS	−0.45	0.27	−0.03	−0.15	0.17	0.46	−0.05	−0.02	0.19	−0.09	0.68	
	(<0.01)	(<0.01)	(0.41)	(<0.01)	(<0.01)	(<0.01)	(0.18)	(0.59)	(<0.01)	(0.01)	(<0.01)	
INSIDER	0.11	−0.02	0.00	−0.06	−0.04	−0.04	−0.01	0.02	0.05	−0.09	−0.05	−0.05
	(<0.01)	(0.51)	(0.90)	(0.08)	(0.30)	(0.24)	(0.70)	(0.56)	(0.16)	(<0.01)	(0.18)	(0.14)

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TABLE 3 (continued)

Panel B: Pearson Correlations among the Lawsuit Indicator Variable (*SUED*), *TIMELINESS*, and Other Control Variables Using the Probability Match Sample as the Control Group (p-values are in parentheses)

	SUED	*TIMELINESS*	*NEWS*	*SIZE*	*BM*	*RET*	*HIGHLIT*	*SHARE_TURN*	*VOLATILITY*	*FOLLOW*	*MIN_RET*	*SKEWNESS*
TIMELINESS	−0.36											
	(<0.01)											
NEWS	0.00	0.19										
	(0.92)	(<0.01)										
SIZE	0.22	−0.06	0.35									
	(<0.01)	(0.09)	(<0.01)									
BM	−0.33	0.14	−0.18	−0.29								
	(<0.01)	(<0.01)	(<0.01)	(<0.01)								
RET	−0.54	0.27	0.22	−0.02	0.31							
	(<0.01)	(<0.01)	(<0.01)	(0.54)	(<0.01)							
HIGHLIT	0.22	−0.10	0.00	−0.02	−0.27	−0.25						
	(<0.01)	(<0.01)	(0.98)	(0.47)	(<0.01)	(<0.01)						
SHARE_TURN	0.39	−0.17	−0.03	0.04	−0.25	−0.40	0.36					
	(<0.01)	(<0.01)	(0.35)	(0.30)	(<0.01)	(<0.01)	(<0.01)					
VOLATILITY	0.18	−0.03	−0.19	−0.34	−0.16	−0.34	0.37	0.53				
	(<0.01)	(0.32)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)				
FOLLOW	0.26	−0.10	0.17	0.71	−0.18	−0.14	0.07	0.22	−0.12			
	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(0.05)	(<0.01)	(<0.01)			
MIN_RET	−0.59	0.31	0.27	0.11	0.30	0.79	−0.31	−0.43	−0.43	−0.05		
	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(0.11)		
SKEWNESS	−0.43	0.29	0.08	−0.19	0.19	0.46	−0.05	−0.10	0.17	−0.17	0.61	
	(<0.01)	(<0.01)	(0.01)	(<0.01)	(<0.01)	(<0.01)	(0.17)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	
INSIDER	0.17	−0.08	0.06	0.01	−0.08	−0.06	0.03	0.04	−0.01	−0.08	−0.07	−0.05
	(<0.01)	(0.02)	(0.10)	(0.88)	(0.02)	(0.10)	(0.38)	(0.28)	(0.84)	(0.02)	(0.03)	(0.12)

TABLE 4

Conditional Logit Regression Results Examining the Effect of the Timely Revelation of Earnings News on the Incidence of Litigation for Dismissed and Settled Suits

$$PROB(SUED = 1) = F\Big(\beta_1 TIMELINESS + \beta'(CONTROLS)\Big) \tag{2}$$

Panel A: Earnings News-Matched Control Sample

	(1) All Suits			(2) Dismissed Suits			(3) Settled Suits		
Variable	**Coeff.**	**t-statistic**	**Marg. Effect**	**Coeff.**	**t-statistic**	**Marg. Effect**	**Coeff.**	**t-statistic**	**Marg. Effect**
TIMELINESS	−0.48	(3.87)***	−0.027	−0.60	(2.18)**	−0.021	−0.38	(2.56)**	−0.022
NEWS	−4.19	(1.02)	−0.241	−0.40	(0.42)	−0.014	−5.98	(1.20)	−0.349
SIZE	0.34	(2.40)**	0.019	0.98	(2.04)**	0.034	0.29	(1.68)*	0.017
RET	−0.04	(4.03)***	−0.002	−0.06	(2.18)**	−0.002	−0.05	(3.64)***	−0.003
BM	−0.54	(2.00)**	−0.031	−1.34	(2.05)**	−0.046	−0.50	(1.37)	−0.029
HIGHLIT	0.10	(0.29)	0.006	−2.79	(2.38)**	−0.096	1.24	(2.45)**	0.072
VOLATILITY	0.12	(1.16)	0.007	0.34	(1.26)	0.012	0.12	(0.83)	0.007
SHARE_TURN	0.58	(2.98)***	0.033	1.36	(2.25)**	0.047	0.68	(2.53)**	0.040
FOLLOW	0.06	(1.43)	0.003	0.00	(0.00)	0.000	0.10	(1.86)*	0.006
MIN_RET	−5.68	(2.31)**	−0.003	−10.32	(1.98)**	−0.004	−1.95	(0.60)	−0.001
SKEWNESS	−0.14	(0.85)	−0.008	−0.23	(0.60)	−0.008	−0.24	(1.15)	−0.014
INSIDER	0.10	(2.62)***	0.006	0.15	(1.54)	0.005	0.10	(2.25)**	0.006
Pseudo R^2	39.5%			43.7%			39.5%		
n	846			360			486		

Panel B: Probability Score-Matched Control Sample

	(1) All Suits			(2) Dismissed Suits			(3) Settled Suits		
Variable	**Coeff.**	**t-statistic**	**Marg. Effect**	**Coeff.**	**t-statistic**	**Marg. Effect**	**Coeff.**	**t-statistic**	**Marg. Effect**
TIMELINESS	−0.66	(8.67)***	−0.056	−0.83	(5.96)***	−0.077	−0.62	(6.22)***	−0.049
NEWS	−0.22	(2.93)**	−0.018	−0.20	(2.42)**	−0.019	−0.38	(2.36)**	−0.029
SIZE	0.45	(3.30)***	0.038	0.29	(1.55)	0.027	0.70	(2.72)***	0.054
RET	−0.03	(3.56)***	−0.003	−0.02	(1.20)	−0.002	−0.05	(3.34)***	−0.004
BM	−0.72	(2.82)***	−0.061	−2.38	(3.96)***	−0.220	−0.48	(1.42)	−0.037
HIGHLIT	−0.15	(0.76)	−0.013	−0.28	(0.85)	−0.026	−0.11	(0.42)	−0.009
VOLATILITY	0.25	(3.11)***	0.021	0.09	(0.59)	0.008	0.32	(2.74)***	0.025
SHARE_TURN	0.16	(1.35)	0.014	0.21	(1.01)	0.020	0.36	(1.88)*	0.028
FOLLOW	0.06	(2.41)**	0.005	0.11	(2.80)***	0.010	0.06	(1.81)*	0.005
MIN_RET	−0.08	(4.26)***	−0.006	−0.09	(3.31)***	−0.008	−0.10	(2.96)***	−0.008
SKEWNESS	−0.33	(2.28)**	−0.028	−0.50	(2.15)**	−0.046	−0.46	(1.73)*	−0.035
INSIDER	0.12	(2.83)***	0.010	0.21	(2.99)***	0.019	0.14	(2.08)**	0.011
Pseudo R^2	17.7%			23.2%			18.5%		
n	846			360			486		

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TABLE 4 (continued)

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*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).
This table reports the results of a conditional logit regression examining the impact of timely news revelation on the incidence of litigation. We obtain all class action securities lawsuits not involving a restatement filed in Federal court from 1996–2005 from the RiskMetrics Securities Class Action Services (SCAS) database. In Panel A, for each sued firm, we select a match firm from the same quarter with the closest total earnings news. In Panel B, for each sued firm, the match is the non-sued firm with the closest predicted probability of litigation (using determinants from prior literature) in the quarter triggering litigation for sued firms. Column (1) contains the regression results for all firms, Column (2) contains the regression results for dismissed suits, and Column (3) contains the results for settled suits. Marginal effects are calculated as the mean of the individual marginal effects evaluated at each sample observation (Greene 2003).
All variables are defined in Appendix A.

matching non-sued firms.[11] This analysis addresses two issues. First, Field et al. (2005) find significant results only for settled suits and we want to assess the sensitivity of our findings to this distinction. Second, as discussed earlier, such non-meritorious litigation in dismissed suits is arguably the very type of litigation that capital market participants may want timely revelation of earnings news to preempt. The final columns of Panel A indicate that *TIMELINESS* is negatively related to litigation incidence for both dismissed and settled lawsuits. Thus, our main findings are not sensitive to the case outcome.[12]

Panel B of Table 4 reports the results of estimating Equation (2) using non-sued firms matched on litigation probability. Consistent with the results in Panel A, there is a significant negative relation between *TIMELINESS* and litigation risk. In fact, the results in Panel B are statistically and economically more significant than those in Panel A. For example, for all lawsuits, a one unit increase in ranked *TIMELINESS* is associated with a decrease in the probability of a lawsuit of 5.6 percentage points. In Panel B, we also find evidence that the deterrent effect of timely news revelation appears stronger for dismissed lawsuits (in terms of coefficient size and marginal effect). However, the negative relation between *TIMELINESS* and litigation risk holds for both dismissed and settled cases.[13,14]

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[11] We also examine an alternative definition of non-meritorious cases, where we move cases that settle for small amounts (less than 0.5 percent of equity value) to the dismissed category. We find that *TIMELINESS* is still significant in each subsample.

[12] We perform two additional subsample tests. First, we assess the sensitivity of our findings to the magnitude of total earnings news as large drops in earnings news likely yield the strongest inference of fraud. Our results strengthen when we focus on cases with a larger magnitude of bad earnings news. Second, we examine the periods before and after the implementation of Regulation FD separately due to changes in the analyst environment at that time (see Gintschel and Markov 2004). The coefficient on *TIMELINESS* in the post-Regulation FD period drops about one-half from its value in the pre-Regulation FD period, although it is still economically and statistically significant. This evidence that the difference in news timeliness between sued and non-sued firms shrinks in the post-Regulation FD period is consistent with non-sued firms engaging in more private communication with analysts in the pre-Regulation FD period. However, because other major economic and regulatory events occurred in the 2000s (e.g., Sarbanes-Oxley), we are leery of drawing any definitive causal inferences from this finding.

[13] The significant coefficients on the control variables in Panel B of Table 4 (i.e., using the probability-matched sample) are sensitive to the use of conditional logistic regression. The coefficients on those controls are insignificant when we use normal logistic regression.

[14] We also examine a measure of bad news "smoothness," the extent to which the consensus forecast drops in a smooth, linear fashion over the quarter. We do this to ensure that *TIMELINESS* is capturing the effect of interest rather than some other construct. When included in Equation (2) along with *TIMELINESS*, this variable is insignificant. Thus, the timeliness of bad earnings news, not its "smoothness," appears to influence litigation risk. The *SMOOTHNESS* variable is described in detail in Appendix A.

There are at least two potential reasons that the *TIMELINESS* measure produces different results from those of Field et al. (2005), who find evidence of a negative relation between litigation risk and press release warnings only among settled suits. First, unlike Field et al. (2005), we do not use an instrumental variable (IV) approach. Larcker and Rusticus (2010) note that IV estimations can lead to a lack of statistical power. Thus, *TIMELINESS* may simply have more power to detect the negative relation between timely news revelation and litigation risk. Second, *TIMELINESS* captures earnings news revelation from sources beyond managerial press releases. Thus, *TIMELINESS* provides more granular information compared to an indicator variable based on the presence of an earnings warning. We provide more detail with respect to reconciling our results to those of Field et al. (2005) below, under "Reconciliation to Prior Work."

Overall, the results from Figure 1 and Tables 2 through 4 indicate consistently that timelier revelation of bad earnings news is associated with a lower incidence of litigation. This evidence supports the litigation reduction hypothesis.

Additional Analyses

Alternate Matched Control Samples

In addition to the two control samples used in Table 4, we also estimated Equation (2) using two alternative matched control samples. The first involves matching sued firms and non-sued firms on market capitalization and stock returns over the litigation quarter. The second involves matching on industry and negative one-day stock returns over the litigation quarter, similar to the procedure in Johnson et al. (2007). We do not tabulate results using these alternate samples, but statistical and economic significance is similar to that in Table 4.

Economic Significance in the Broad Population

Although a matched pair design helps isolate non-sued firms with earnings news and other litigation determinants similar to sued firms, such a procedure involves non-random sampling. Since securities litigation is a fairly rare event in the broad population of firms, our matched design over-samples sued firms and under-samples non-sued firms. The average probability of litigation in our samples is 50 percent, while the unconditional probability of being sued in the broad population is much lower. As a consequence, concerns arise as to whether our findings misstate the economic significance of *TIMELINESS* in affecting overall litigation rates.

To address this concern, we estimate Equation (2) using a logistic regression among all firms in the Compustat-CRSP-I/B/E/S universe with bad earnings news, i.e., those for which the last consensus is lower than the first consensus. Overall, we have 423 sued observations and 64,532 non-sued observations, for an unconditional litigation risk of roughly 0.65 percent. In untabulated results, we find that the coefficient on quintile-ranked *TIMELINESS* is -0.57 ($t = 8.41$, $p < 0.01$), with a mean (median) marginal effect of roughly 0.31 percent (0.05 percent) moving from one quintile to the next quintile, which is approximately 47 percent (8 percent) of the unconditional probability of being sued for bad earnings news in the overall population.[15] Thus, the effect of

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[15] Since litigation risk is concentrated in a relatively small sample of firms in the broad population, the predicted probability of being sued in the full population of firms is highly right-skewed. As a result, the distribution of individual marginal effects in the full population is also highly right-skewed, which produces a mean marginal effect that is much higher than the median. The median marginal effect in the full sample (approximately 8 percent of the 0.65 percent base rate) is comparable to the mean and median marginal effects in the earnings-matched (approximately 5 percent of the 50 percent base rate) and probability-matched samples (approximately 11 percent of the 50 percent base rate).

TIMELINESS on this baseline risk appears economically significant when compared to the probability of litigation in the overall population.

Fundamental Differences between Sued and Non-Sued Firms

One potential concern with our main findings is that sued firms' information environments may be fundamentally different from those of non-sued firms. Perhaps news tends to be less timely, or analysts react in a less timely manner to news, for sued firms versus non-sued firms due to differences in industry membership, business models, or other factors. Thus, our main analysis may simply be picking up innate differences in information flow or other firm characteristics between sued and non-sued firms.

We address this concern in two ways. First, we benchmark sued firms against themselves and estimate Equation (2) utilizing sued firms that also had negative earnings news in the quarter prior to the quarter that triggered litigation. We measure total earnings news in this quarter as the ending consensus forecast less the beginning consensus, scaled by beginning stock price. The results from this regression are in Table 5. Again, we find that negative earnings news is less timely in the litigation quarter, even when sued firms are compared to themselves.

Second, we assess whether differences in timeliness between sued and non-sued firms exist outside the litigation quarter by examining the first fiscal quarter that begins after the filing of the lawsuit. We require total news in this quarter to be negative for sued firms and match these firms to non-sued firms in the same quarter with the closest total news. Figure 2 plots the timeliness of earnings news using the same methods as in Figure 1. The striking difference in timeliness between sued and non-sued firms evident in Figure 1 disappears. In fact, news revelation appears slightly more timely for sued firms, but a t-test reveals no significant difference in *TIMELINESS* across samples ($t = -0.29$, untabulated).[16] Combined with the above result (utilizing sued firms as their own control), this evidence suggests that our main finding is not driven by industry or firm differences between sued and non-sued firms.

Timeliness Measurement

The final potential concern pertains to how we measure our *TIMELINESS* variable, and whether this definition of timeliness could somehow "mechanically" lead to our results. This argument proceeds as follows. The *TIMELINESS* measurement period for sued firms ends at the class period end date, when sued firms often reveal bad news and suffer a very large one-day stock price drop. Alternatively, non-sued firms do not necessarily have a bad news event at the end of their measurement period. Thus, the lower likelihood of a significant news event for non-sued firms at the end of the measurement window could lead to the lower *TIMELINESS* measure for sued firms as compared to non-sued firms.

While it is difficult to separate this argument regarding a mechanical relation from the effect of interest in our study, we conduct an additional untabulated test. First, for each sued firm we calculate the distance between the end of the class period and the largest one-day stock price drop during the quarter. This distance measure is less than or equal to 14 days for 75 percent of sued firms. Using our probability-matched sample, we then take the date of non-sued firms' largest one-day stock price drop and add the distance variable for the matching sued firm in order to create a "pseudo-class period" over which to measure the timeliness of earnings news for non-sued firms. Using this alternative measure of timeliness, we find results that are statistically and economically

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[16] Inferences are similar if we examine the patterns in Figure 2 using a multivariate framework as in Table 4. In addition, we also compare sued and matched non-sued firms in the fiscal quarter prior to the lawsuit quarter and find patterns virtually identical to those presented in Figure 2 (no significant difference in news timeliness).

TABLE 5

Conditional Logit Regression Results Examining the Effect of the Timely Revelation of Earnings News on the Incidence of Litigation Using the Firm as its Own Control

$$PROB(SUED = 1) = F\left(\beta_1 TIMELINESS + \beta'(CONTROLS)\right) \qquad (2)$$

Variable	Coeff.	t-statistic	Marg. Effect
TIMELINESS	−0.51	(2.01)**	−0.021
NEWS	−0.50	(0.97)	−0.021
SIZE	−6.77	(3.72)***	−0.279
RET	−0.09	(3.68)***	−0.004
BM	11.01	(2.90)***	0.453
VOLATILITY	−0.11	(0.30)	−0.004
SHARE_TURN	−0.56	(0.74)	−0.023
FOLLOW	−0.11	(0.85)	−0.005
MIN_RET	1.97	(0.35)	0.081
INSIDER	−0.43	(1.46)	−0.018
Pseudo R^2	42.7%		
n	356		

*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

This table reports the results of a logistic regression examining the impact of the timely revelation of bad news on the incidence of litigation. We obtain all class action securities lawsuits not involving a restatement filed in Federal court from 1996–2005 from the RiskMetrics Securities Class Action Services (SCAS) database. Consistent with Skinner (1997), we benchmark sued firms against themselves, and estimate Equation (2) utilizing those sued firms that also had negative earnings news in the quarter prior to the quarter that triggered litigation.

All variables are defined in Appendix A.

similar to those observed in our main analyses. Thus, our results do not appear to be mechanically driven by how we construct our measure.[17]

Reconciliation to Prior Work

Our main finding indicates that non-litigation quarters involve timelier revelation of bad news. However, a number of prior studies consistently find that quarters triggering litigation contain more managerial earnings warnings in the form of press releases than non-litigation quarters (Francis et al. 1994; Skinner 1997; Field et al. 2005). How can these two results be reconciled? One explanation is that our timeliness measure captures sources of news revelation beyond earnings warnings. Another possibility is that the different results may be attributable to different samples, time periods, or design choices.

To ensure the negative relation we document between *TIMELINESS* and litigation risk is attributable to our measure, we conduct the following test. We randomly select 100 sued firms with total news earnings equal to at least 0.20 percent of share price and their earnings news-matched

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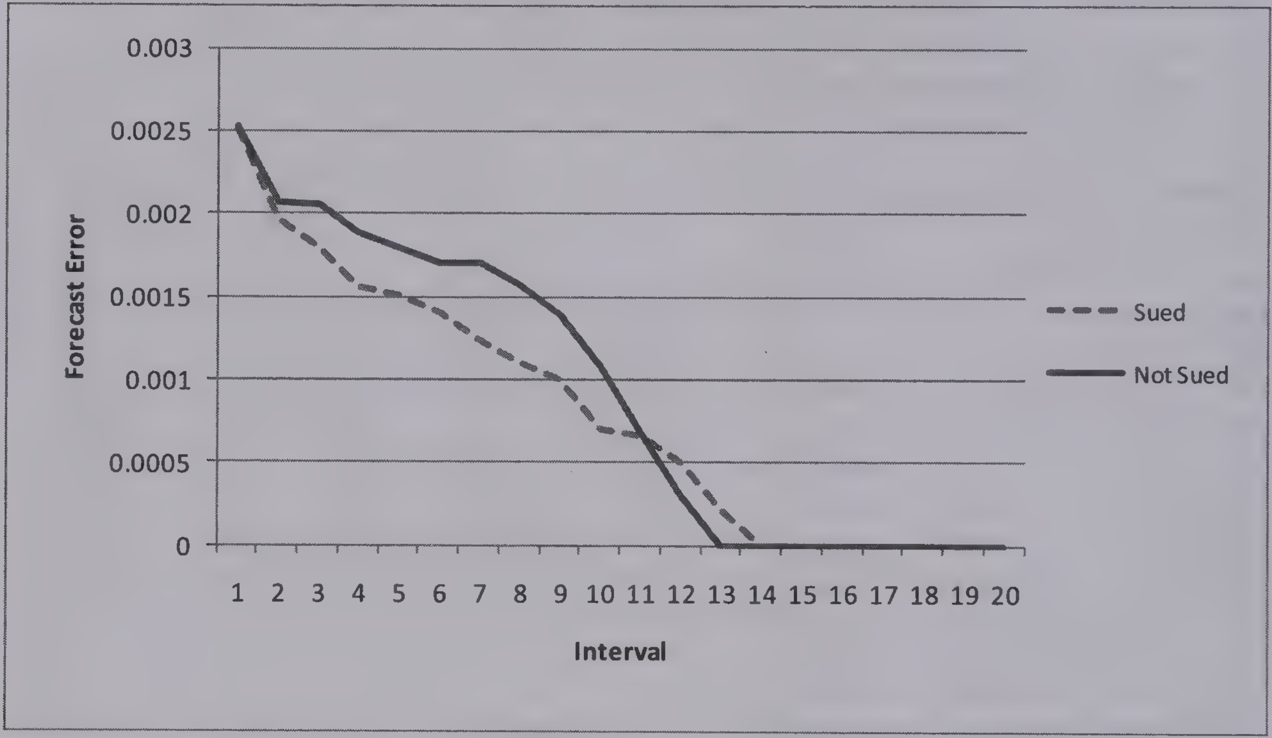
[17] As an additional test, we also find similar results if we measure *TIMELINESS* using the proportion of total earnings news that is revealed up to the midpoint of the quarterly revelation window. This measure is not influenced by the revelation of news at the very end of the measurement period.

FIGURE 2
Timeliness of Forecast Revisions after the Class Period for Firms Sued and Firms Not Sued



This figure is constructed in an identical fashion to Figure 1, except that we focus on the first quarter after the filing of the lawsuit. For each firm we then divide the quarterly revelation window into 20 equal intervals, and calculate the median forecast error across firms by interval.

firm, to maintain comparability to Field et al. (2005). The results are similar if we limit the sample to dismissed suits, settled suits, or suits with earnings news of at least 1 percent of market value of equity. For these 200 firm-quarters, we search Factiva and Lexis-Nexis for earnings warnings in the form of a press release during the litigation quarter. For press releases that give explicit earnings guidance or warn of lower earnings or sales before the earnings announcement date, we code an indicator variable, *WARNING*, equal to 1. We then estimate the following logistic regressions, the first of which is similar to the specification used in prior studies:

$$PROB(SUED = 1) = F(\alpha + \beta_1 WARNING + \beta_2 NEWS + \beta_3 SIZE + \beta_4 RET + \beta_5 HIGHLIT + \beta_6 VOLATILITY + \beta_7 SHARE\_TURN); \quad (3a)$$

$$PROB(SUED = 1) = F(\alpha + \beta_1 TIMELINESS + \beta_2 NEWS + \beta_3 SIZE + \beta_4 RET + \beta_5 HIGHLIT + \beta_6 VOLATILITY + \beta_7 SHARE\_TURN). \quad (3b)$$

The only difference between Equations (3a) and (3b) is that Equation (3a) uses *WARNING* to proxy for timely disclosure (consistent with prior work), while Equation (3b) uses our timeliness measure.[18] Estimates are presented in Table 6. In the first column, for Equation (3a), the coefficient

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[18] Equation (3a) includes the significant variables from Field et al. (2005, Table 4, column 1). Since we wish to reconcile to prior studies, we estimate an unconditional model, but results are similar if a conditional model is used.

TABLE 6

Timeliness versus Earnings Warnings

	(1) Earnings Warning		(2) Timeliness	
Variable	**Coeff.**	**t-statistic**	**Coeff.**	**t-statistic**
Intercept	8.56	(−5.34)***	−6.13	(−3.78)***
WARNING	1.78	(3.77)***		
TIMELINESS			−0.97	(−4.84)***
NEWS	−0.18	(−1.51)	−0.16	(−1.35)
SIZE	0.62	(3.90)***	0.64	(3.56)***
RET	−0.06	(−5.51)***	−0.05	(−4.64)***
HIGHLIT	0.71	(1.40)	0.58	(1.03)
VOLATILITY	0.19	(1.16)	0.27	(1.54)
SHARE_TURN	0.48	(1.50)	0.44	(1.44)
Pseudo R^2	50.8%		54.2%	
n	200		200	

*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

This table reconciles our results to those obtained by the prior literature. More specifically, we select 100 sued and their corresponding earnings news match firms and collect whether an earnings warning was issued. We then run our main analysis with the earnings warning indicator variable in Column (1) and our timeliness measure in Column (2).

All variables are defined in Appendix A.

on *WARNING* is *positive* and significant ($t = 3.77$, $p < 0.01$), which is consistent with the findings of prior work in this area (e.g., Field et al. 2005).[19] In the second column, for Equation (3b), the coefficient on *TIMELINESS* is *negative* and significant ($t = -4.84$, $p < 0.01$).

Thus, like prior studies, we find that sued firms are more likely to warn than non-sued firms. However, the overall timeliness of news revelation for sued firms is lower compared to non-sued firms. This evidence suggests that our measure of timeliness, which focuses on revisions to the consensus analyst forecast, captures forms of news revelation that are not captured by managerial earnings warnings. In the next section, we explore the sources of news that drives revisions to the consensus analyst forecast in our sample.

What Drives Revisions to the Consensus Analyst Forecast?

We examine analysts' research reports to provide evidence on the source behind revisions to the consensus analyst forecast. We begin by using the same 100 sued and 100 non-sued firms used in Table 6, and collect all meaningful research reports from the beginning of the quarter to the end of the class period available in Investext.[20] We find meaningful reports for 115 total firms (68 sued and 47 non-sued), including 18 matched pairs. Untabulated analysis of these reports yields two main results. First, the overwhelming majority of analyst revisions (89 percent) are driven by managerial disclosures, including both earnings warnings and other, nontraditional managerial disclosures, such as conference calls, presentations, webcasts, and private communications. Second, while non-sued firms' revisions are less likely to be triggered by explicit earnings warnings compared to sued firms (44 percent versus 94 percent, $p < 0.01$), non-sued firms' revisions are more likely to be triggered by nontraditional management disclosures (33 percent versus 6 percent, $p < 0.05$).[21] Overall, this analysis helps validate our new measure as a way to capture all forms of managerial disclosure.

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[19] We attempted to further reconcile our results to Field et al. (2005) by collecting data on management disclosure of earnings news from the same quarter in the prior year. Our findings with respect to the primary variables are very similar to Field et al. (2005). We (they) find that 84 percent (84 percent) of sued firms warn in the litigation quarter compared to 34 percent (44 percent) of non-sued firms. For the prior period disclosure instrumental variable, we (they) find that 16 percent (19 percent) of sued firms provide disclosure, while 18 percent (22 percent) of non-sued firms provide disclosure. While we do not find that the instrument is positively linked to warnings, we do find that this variable is negative and insignificant when we run the second-stage regression (inserting the predicted probability of a warning from the first stage), as in Field et al. (2005). We also find similar results using subsamples (e.g., dismissed versus settled, restricting to 1 percent of *MVE* for earnings news).

IV. CONCLUSION

This study explores whether the timelier revelation of bad earnings news is linked to lower litigation risk. To examine this question, we utilize a new measure that captures the timeliness of total earnings news via the evolution of the consensus analyst forecast. This timeliness measure captures all earnings news revealed to the market including traditional management disclosure, nontraditional management disclosures (e.g., conference calls, presentations, and private communication) and non-management earnings news. Thus, the measure captures the timeliness of earnings news across all revelation channels. Utilizing this measure, we find that timely revelation of bad earnings news deters litigation. This result holds for both settled and dismissed lawsuits, suggesting that timely news revelation deters the incidence of all types of litigation.

This study has two limitations. First, our measure of earnings news timeliness requires active analyst coverage, so it will not be effective for many small firms. Second, we can never be sure that managers who revealed news in an apparently untimely fashion actually had possession of this news at an earlier date. In fact, plaintiffs can rarely be sure of the date of managerial knowledge acquisition. All empirical studies of disclosure therefore suffer from this limitation. Nevertheless, the implications of our findings for managers clearly indicate that it is best to release bad earnings news early to reduce the chance of litigation.

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[20] We ignore research reports that represent mere reactions to the prior quarter's earnings announcements and focus only on reports that discuss a "material" revision to the analyst's prior earnings forecast for the current quarter. We define a material revision as one that is at least equal to 0.1 percent of share price. If reports by multiple analysts all pertain to the same news revelation (e.g., a management earnings warning), then we retain a single report.

[21] The proportions cited above are for the 18 matched-pairs (36 firms) only. However, non-sued to sued comparisons among all 115 firms are inferentially similar (53 percent versus 91 percent for warnings, $p < 0.01$, and 30 percent versus 4 percent for non-traditional disclosures, $p < 0.01$).

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APPENDIX A
VARIABLE DEFINITIONS

BM = ratio of total shareholder equity (from Compustat) in the litigation quarter to market value of equity at the beginning of the quarterly revelation window;

FOLLOW = number of analysts following the firm during the quarterly revelation window;

HIGHLIT = indicator variable that is set to 1 when the firm operates in a high litigation risk industry as defined by Francis et al. (1994) (SIC codes 2833–2838 and 8731–8734 (biotech); SIC codes 3570–3577 and 7370–7374 (computer); 3600–3674 (electronics); and 5200–5961 (retail)), and is equal to 0 otherwise;

INSIDER = for sued firms, net selling (shares sold minus bought/shares outstanding) of insiders over the class period, less net selling over the previous 365 days. For non-sued firms, net selling (shares sold minus bought/shares outstanding) of insiders over the 365 days ending in the current quarter, less net selling over the previous 365 days;

MIN_RET = minimum daily return over the litigation quarter;

NEWS = for sued firms, this is the "earnings revelation" at the end of the class period less the beginning consensus forecast (which is measured at least one day after the prior quarter earnings announcement) scaled by stock price one day after the prior quarter earnings announcement. When the class period end date corresponds with an earnings announcement date, the "earnings revelation" is equal to I/B/E/S actual EPS. When the class period end date does not correspond with an earnings announcement, the "earnings revelation" is defined as the consensus forecast as of the class period end date plus up to five days;

NEWS = for non-sued firms is defined as follows. For lawsuits where the class period ends on the earnings announcement day, total earnings news for matched non-sued firms is measured as actual earnings minus the beginning consensus, scaled by beginning stock price. For all other cases, total earnings news is measured as the "ending" consensus less the beginning consensus, scaled by beginning stock price. The "ending" consensus for earnings-matched non-sued firms is measured as the same number of days after the prior quarter earnings announcement date as the ending consensus for the corresponding sued firm. For probability-matched non-sued firms, the ending consensus is the last consensus forecast of the quarter. The consensus forecast at any given date is the median of the most recent forecast available for all analysts. This variable is expressed as a percentage;

RET = cumulated daily stock return beginning the day after the prior quarter earnings announcement and ending on the class period end date. Expressed as a percentage;

SHARE_TURN = average daily turnover (volume/shares outstanding) over the quarterly revelation window, expressed as a percentage;

SIZE = log of market value (in billions) of the firm obtained from CRSP and measured on the day after the prior quarter's earnings were announced;

SKEWNESS = skewness of daily stock returns over the litigation quarter;

SMOOTHNESS = average absolute daily deviation of the consensus revision from the smoothest revision that could have occurred had the firm released its earnings news evenly throughout the litigation quarter. The smoothest possible revision is defined as the total earnings news divided by the number of days in the quarterly announcement window;

TIMELINESS = average proportion of total news that was revealed up to a given day in the quarterly revelation window. The quarterly revelation window is the first day after the prior quarter earnings announcement to the end of the class period. To illustrate, assume that a quarterly revelation window is four days, the beginning consensus forecast is \$0.50 per share, and the ultimate earnings revelation is \$0.10 per share, implying total (unscaled) news of \$0.40 per share during the quarterly revelation window. If the consensus forecast was revised to \$0.30 on the second day of the quarterly window, then 50 percent [(\$0.50 – \$0.30)/(\$0.50 – \$0.10)] of the total news had been revealed at that point. Furthermore, assume that the consensus forecast is revised to \$0.20 on day three of the quarterly window, then 75 percent [(\$0.50 – \$0.20)/(\$0.50 – \$0.10)] of the total news was revealed up to that point. Finally, the consensus forecast is revised to \$0.10 on the fourth day, suggesting 100 percent of the news was disclosed. Hence, the value of our timeliness measure for this example is 56 percent (0% + 50% + 75% + 100%)/4. Hence, higher (lower) values of this variable imply more (less) timely revelation of bad earnings news;

VOLATILITY = standard deviation of daily returns over the quarterly revelation window, expressed as a percentage; and

WARNING = indicator variable that is set equal to 1 when a firm issues an earnings warnings or preannouncement of earnings, and is equal to 0 otherwise.

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Mandatory IFRS Adoption and Institutional Investment Decisions

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ABSTRACT: We examine whether the mandatory introduction of International Financial Reporting Standards leads to an increase in institutional investor demand for equities. Using a large ownership database covering all types of institutional investors from around the world, we find that institutional holdings increase for mandatory IFRS adopters. Changes in holdings are concentrated around first-time annual reporting events. Second, we document that the positive IFRS effects on institutional holdings are concentrated among investors whose orientation and styles suggest they are most likely to benefit from higher quality financial statements, including active, value, and growth investors. These results are consistent with holdings changes being associated with the financial reporting regime change. Finally, we show that increased institutional holdings are concentrated in countries in which enforcement and reporting incentives are strongest, and where the differences between local GAAP and IFRS are relatively high. Overall, our study helps shed new light on the channels by which IFRS information becomes impounded in market outcomes.

Keywords: *accounting regulation; IFRS; standard-setting; accounting quality; comparability; institutional investment decisions.*

JEL Classifications: *G11; K22; M41; M42.*

Data Availability: *The data used in this study are available from the commercial sources identified in the paper.*

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I. INTRODUCTION

Regulators have claimed that investors and issuers of securities will benefit from the mandatory adoption of International Financial Reporting Standards (IFRS). They claim that for many countries IFRS require higher quality measurement and recognition rules; they are more transparent than local GAAP; and that common IFRS financial reporting across countries will enhance comparability (EC Regulation No. 1606/2002, European Council 2002). Theory predicts that the mandated introduction of higher quality financial reporting can improve stock market liquidity and reduce the cost of capital (Leuz and Wysocki 2008), and empirical evidence on the market outcomes of mandatory IFRS adoption generally supports these predictions (Daske et al. 2008; Li 2010). An indirect implication of such findings is that market participants find IFRS reporting to be incrementally informative. In this paper we test directly whether mandated IFRS adoption affects institutional investor decisions. Institutional investors are an interesting group of financial statement users because they are economically important and because they are often regarded as sophisticated users who have the time, resources, and expertise to process the information in complex financial statements. Establishing a causal link between mandatory IFRS reporting and institutional investor decision outcomes helps to shed new light on the channels by which IFRS information becomes impounded in market outcomes.

Using a global sample of 10,852 unique firms from 45 countries over the 2003–2006 period, we report a number of results that are new to the literature. First, after controlling for standard economic determinants of institutional holdings, we show that over the two-year period 2005–2006 institutional ownership increases by more than 4 percent and the number of institutional investors increases by almost ten for mandatory IFRS adopters, relative to non-adopters. Results are consistent for different analyses based on annual firm-level as well as quarterly firm-level and country-level changes in institutional holdings. Second, we document that the positive IFRS effects on institutional holdings are concentrated among investors whose orientation and styles suggest they are most likely to benefit from higher quality financial statements. For example, institutional holdings in mandatory IFRS adopters increase significantly for active investors, but changes are much lower or insignificant for passive investors. Similarly, IFRS-related holdings increases are substantially higher for value and growth investors than for index and income investors. Finally, we document that the increases in institutional holdings associated with IFRS are not homogeneous across countries. Our findings indicate that institutional holdings increase for first-time mandatory adopters primarily in countries in which enforcement and reporting incentives are strongest, and where the differences between local GAAP and IFRS are relatively high. Inferences are unaffected by various sensitivity tests, including different sample definitions and additional control variables.

Our paper is most closely related to Bradshaw et al. (2004) and Covrig et al. (2007). Bradshaw et al. (2004) is the first study to show that institutional stock ownership is related to firm-level accounting choices, although they do not consider the role of accounting standards. Covrig et al. (2007) show that stock ownership by foreign mutual funds increases with the voluntary adoption of International Accounting Standards (IAS) by non-U.S. firms. Our paper extends this line of research by investigating the effects of mandatory IFRS adoption on institutional holdings. Our paper is also related to two concurrent studies reporting that foreign mutual fund ownership increases following mandatory IFRS introduction (Yu 2010; DeFond et al. 2011). However, both of those studies focus on the mechanisms through which IFRS attracts foreign investors. Yu (2010) examines the IFRS effects of lower information asymmetry, measured primarily by the accounting distance between the investees' and investors' accounting standards. DeFond et al. (2011) examine the IFRS effects of increased comparability based on the similarity of accounting standards of investee firms within industries and across different countries.

We contribute to the literature in several ways. First, we provide evidence that institutional investor decisions respond to IFRS reporting. This helps to explain the mechanism through which the IFRS-related capital market outcomes reported in Daske et al. (2008) and Li (2010) occur. Second, we employ a research design that enhances our ability to link the observed ownership effects to the financial reporting regime change and helps rule out alternative explanations (Daske et al. 2008, 1132) as follows: (1) we identify the timing of changes in institutional holdings relative to first-time IFRS reporting events; (2) we examine the institutional holdings effects of mandatory IFRS adoption with reference to differences in the predicted demand for and use of financial reporting information by different investors; and (3) we focus on changes in holdings rather than levels of holdings, thereby helping to control for firm- and country-level characteristics that determine holdings independently of the financial reporting regime. Our focus on mandatory IFRS adoption helps to mitigate potential issues of selection bias and omitted correlated factors in voluntary adoption studies (Leuz and Wysocki 2008).

Finally, we introduce a new research design in examining the institutional context of IFRS reporting effects. Our approach is different from that of prior work (Daske et al. 2008; Li 2010) because we test the impact of complementary institutional features by using a single model based on institutional "bundles" rather than several models focusing on different institutional characteristics. Our findings on the role of institutions contribute to a growing literature highlighting that the introduction of higher quality accounting standards is necessary, but not sufficient, for higher quality financial reporting and associated benefits (Li 2010).

The remainder of the paper is organized as follows. In the next section we discuss prior related studies and elaborate on the motivation of our study. In Section III we describe in detail the research design and data. We discuss our findings in Section IV. Finally, in Section V we present our conclusions.

II. THEORETICAL BACKGROUND AND MOTIVATION

Proponents of IFRS claim that IFRS have advantages over local accounting standards in many countries for several reasons. First, IFRS may be more capital-oriented and, therefore, more useful to investors (Hail et al. 2010). Second, IFRS can reduce the choice of accounting methods, thus constraining managerial discretion (International Accounting Standards Board [IASB] 1989; Ashbaugh and Pincus 2001; Barth et al. 2008). Third, IFRS require accounting measurements and recognition that better reflect a firm's underlying economic position, hence providing more relevant information for investment decisions (IASB 1989; Barth et al. 2008). Fourth, IFRS increase required disclosures, thereby mitigating information asymmetries between firms and their shareholders (Leuz and Verrecchia 2000; Ashbaugh and Pincus 2001). Aside from the higher financial reporting quality argument, proponents of IFRS also claim that harmonization around IFRS increases comparability of firms across markets and countries, hence facilitating cross-border investment and integration of capital markets (Armstrong et al. 2010; DeFond et al. 2011).

In light of these potential advantages, advocates of IFRS often argue that they can be beneficial for investors, issuers, and economies because enhanced financial reporting can result in increased investor demand for equities and lower cost of capital (European Council 2002). Theory suggests that these outcomes will follow if (1) market liquidity increases and price protection falls due to lower information asymmetries among investors (Baiman and Verrecchia 1996; Verrecchia 2001); (2) risk sharing improves due to increased awareness of investment opportunities by investors (Merton 1987; Easley and O'Hara 2004); or (3) the estimation risk facing investors falls (Jorgensen and Kirschenheiter 2003; Lambert et al. 2007; Hughes et al. 2007). In each case investment and portfolio decisions depend on financial reporting, and a change to a higher quality and homogeneous financial reporting regime is predicted to result in an increase in demand for equities and investment holdings.

On the other hand, there are several reasons why the expected benefits of IFRS can be questioned. First, reducing accounting alternatives may result in a less true and faithful representation of the firm's underlying economics (Barth et al. 2008). Second, increasing managerial flexibility, because of the principles-based nature of IFRS, may augment the opportunity for earnings management (Barth et al. 2008). Third, and perhaps most importantly, an increasing number of scholars emphasize that dysfunctional reporting incentives combined with weak enforcement mechanisms can reduce financial reporting quality, even when high-quality accounting standards are implemented (Ball et al. 2003; Burgstahler et al. 2006; Ball 2006).

To the extent that financial reporting under IFRS enhances the quality and comparability of accounting information provided by issuers, we predict that mandatory IFRS adoption is associated with an increase in the demand for equity in IFRS adopting firms. We also predict that IFRS-related changes in holdings are more likely to be observed for investors whose investment orientations and styles rely most heavily on firm-specific financial information (Bushee and Goodman 2007).

The expected effects of mandatory IFRS adoption are likely to depend on the institutions of the adopting country (Hail et al. 2010). In line with this argument, Daske et al. (2008) and Li (2010) document that the capital market effects of IFRS are more pronounced in countries with stricter enforcement regimes and therefore better IFRS implementation; stronger reporting incentives and therefore higher quality financial reporting; and higher divergence between IFRS and local GAAP and therefore a larger change of domestic accounting rules.[1] Accordingly, we predict that the IFRS-related effects on institutional holdings for first-time mandatory adopters are likely to be greater in countries with higher quality institutions and countries with less conformity between domestic GAAP and IFRS.

III. RESEARCH DESIGN AND DATA DESCRIPTION

Identification Strategies

An important concern in studies of the impact of institutional change is to provide evidence that observed effects reflect causality and rule out alternative explanations. Our baseline results, using a difference-in-differences research design similar to Daske et al. (2008), document increases in institutional holdings after mandatory IFRS adoption. Our main challenge is to develop convincing evidence that increases in institutional holdings result directly from the financial reporting regime change and are not due to other contemporaneous regulatory or market changes affecting IFRS countries. Our research design includes two strategies designed to address this challenge.

Our first strategy uses information on the timing of firm-specific IFRS reporting to infer whether causality runs from financial reporting to institutional holdings changes. Therefore, we examine changes in holdings for annual and quarterly periods relative to first-time IFRS reporting events. Our tests include both firm-level as well as country-quarter analysis similar to the country-month analysis of Daske et al. (2008).[2]

In the second set of tests, we use investor type and investor style as instruments reflecting investor demand for, and use of, financial reporting information. Investor type is defined as active or passive. Active investors often use firm-specific information, including financial reporting numbers and disclosures, to determine the portfolio weights of individual stocks. In contrast, passive investors do not seek to take security-specific views and rely on decision rules that do not depend on financial reporting information. Thus, we predict that institutional holdings changes will be more significant for active investors than for passive investors if institutional holdings changes are causally linked to the financial reporting regime change.

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[1] We note, however, that Francis et al. (2009) provide evidence suggesting that an improved information environment enhances asset allocation, irrespective of a country's legal regime and enforcement mechanisms. Also, in a related study Daske et al. (2011) document that "serious" mandatory IFRS adopters experience higher liquidity and lower cost of capital compared to "label" adopters, providing evidence on the firm-level heterogeneity in the capital market effects of IFRS adoption.

[2] We conduct country-quarter analysis rather than country-month analysis because our institutional ownership data is published at the end of each calendar quarter.

Similarly, within the subset of active investors, some investors have investment styles that are more dependent on accounting numbers than others (Bushee and Goodman 2007). Our holdings data allow us to identify several common stock-oriented investment styles including value, growth, index, and income investing. Value investors seek to determine whether stock market values are fairly priced relative to fundamental values estimated using firm-specific accounting numbers, and take long (short) positions relative to their benchmark in under-valued (over-valued) stocks. Growth investors seek to forecast future value-relevant outcomes that will determine future capital gains, for example by identifying stocks with above-average earnings growth potential. Financial statements and forward-looking disclosures are important inputs to the decision models of many such investors. In contrast, income investors usually focus on short-term dividends and dividend yield, while index investors invest in proportion to stock index weights. In the latter two investment styles, financial statement information is less important in investment decisions. Therefore, we predict that changes in institutional holdings attributable to the IFRS regime change are more likely to be observed for value and growth investors than for income and index investors.

Our empirical tests progressively build evidence consistent with causality running from IFRS adoption to changes in institutional holdings. First, we conduct firm-level analysis based on different annual measurement periods relative to first-time IFRS reporting events. Then, we perform additional firm-level analysis using quarterly changes in institutional holdings. Next, we conduct analysis at the country-quarter level. Finally, we examine how annual changes in institutional holdings depend on investor demand for financial reporting information, reflected in investor orientation and style.

Firm-Level Annual and Quarterly Analyses

The firm-level analyses employ a difference-in-differences design similar to Daske et al. (2008), utilizing data on both voluntary and mandatory adopters of IFRS in countries where IFRS are mandated (treatment sample firms) and non-IFRS adopters in other countries (benchmark sample firms), and using versions of the following model:

$$\begin{aligned}\Delta \mathit{Institutional\ Holdings} = {} & \delta_0 + \delta_1 \mathit{First\text{-}Time\ Mandatory} + \delta_2 \mathit{Voluntary} \\ & + \delta_3 \mathit{Voluntary{*}Mandatory} + \Sigma\gamma_j \Delta \mathit{Controls}_j + \varepsilon. \end{aligned} \tag{1}$$

For the annual firm-level analysis *ΔInstitutional Holdings* is the change in an institutional holdings proxy over a year. The marginal effects of IFRS reporting on institutional holdings are captured by three binary indicator variables *First-Time Mandatory*, *Voluntary*, and *Mandatory * Voluntary*, coded based on the reporting standards used in each firm-year (Worldscope code WS07536). *First-Time Mandatory* refers to observations for firms that never reported under IFRS before mandatory adoption and equals 1 for all firm-years with IFRS reporting periods ending on or after the local mandated IFRS adoption date (i.e., December 31, 2005), and 0 otherwise. It captures the extent to which institutional holdings of first-time mandatory adopters are different after IFRS adoption. *Voluntary* relates to observations for firms that voluntarily switched to IFRS reporting before it was mandated and equals 1 for all firm-years with IFRS reporting by voluntary adopters, and 0 otherwise. It captures the extent to which institutional holdings are different for firms that voluntarily adopt IFRS. *Voluntary * Mandatory* equals 1 for all voluntary adopter firm-years ending on or after the mandated local IFRS adoption date, and 0 otherwise. It captures the extent to which institutional holdings of voluntary IFRS adopters are different in the mandatory IFRS period.

Focusing on changes in institutional holdings in Equation (1) enables us to develop insights about the timing of any changes in institutional ownership relative to firm-level IFRS adoption events, as discussed below. The changes model also provides more powerful tests, given that a firm serves as its own control because both current-year and prior-year levels data are required to be included in the changes sample. This requirement helps alleviate concerns that unmodeled factors other than IFRS adoption, including possible database selection biases, could explain differences in the levels of institutional holdings between treatment and benchmark samples, and across time, thus increasing confidence that documented results reflect causality running from the IFRS reporting regime change.

Our baseline analysis of Equation (1) involves measuring changes in institutional holdings over calendar-year intervals. In this case the IFRS indicator coefficient estimates capture the average annual IFRS effects over the two-year period 2005–2006. Since holdings changes are measured over identical periods for both treatment and benchmark samples, the calendar-year approach controls effectively for unmodeled factors affecting both treatment and benchmark samples. However, to ensure that our inferences are not sensitive to the measurement interval for holdings changes, we also report results based on two alternative annual windows spanning firms' fiscal year-ends. The window labeled −2/+2 measures holdings changes over the last two quarters of a fiscal year and the first two quarters of the next fiscal year, while the annual window labeled −1/+3 measures changes over the last quarter of a fiscal year and the first three quarters of the next fiscal year. A possible advantage of examining changes over these alternative windows is that IFRS information contained in annual financial statements becomes available to investors during the measurement interval. However, a significant disadvantage of the alternative windows is that heterogeneity in fiscal year-end dates within and across the treatment and benchmark samples may lead to weaker control for unmodeled factors. Hence, our baseline calendar-year results could be viewed as more conservative estimates of overall IFRS effects that better control for unmodeled global factors underlying holdings changes.

Having established our baseline results using annual windows, we use versions of Equation (1) to address the identification problem by allowing the coefficients on *First-Time Mandatory* and *Voluntary* $*$ *Mandatory* to depend on the arrival of firm-specific IFRS information within the mandated IFRS adoption period. Specifically, we conduct firm-quarter analysis by re-estimating Equation (1) using institutional holdings changes measured over quarterly intervals. We supplement Equation (1) by adding indicator variables that capture incremental reporting-time effects during the mandated IFRS period 2005–2006. Incremental holdings changes for first-time mandatory adopters are captured by indicator variables $\mathit{First\text{-}Time\ Mandatory}_q$ ($q = -2, -1, +1, +2, +3$) set equal to 1 in quarter q and 0 in other quarters, using the convention that $q = -1$ is the quarter of the fiscal year-end and $q = +1$ is the quarter following the fiscal year-end.[3] The coefficients on *Voluntary* $*$ $\mathit{Mandatory}_q$ capture incremental reporting-time holdings changes for voluntary adopters in the mandatory IFRS period in a similar manner.

While financial reporting lags vary across countries, we believe that it is reasonable to expect that financial statements will usually be released late in quarter +1 or early in quarter +2.[4] Therefore, we predict incremental investor reaction to financial statement information in quarter +2 and significantly positive coefficients of $\mathit{First\text{-}Time\ Mandatory}_{+2}$. Delayed response to financial statement information will be indicated if the coefficients on $\mathit{First\text{-}Time\ Mandatory}_{+3}$ are significant. We predict that incremental holdings changes in quarter +3 (if any) will be lower in magnitude than those in quarter +2. If firms report earlier and investors react to information in annual financial statements within quarter +1, then $First\text{-}Time\ Mandatory_{+1}$ will also be positive. If firms do not report sufficiently early in quarter +1 for investors to react in the same quarter, then $First\text{-}Time\ Mandatory_{+1}$ will capture any anticipation effects, as will the coefficients on $First\text{-}Time\ Mandatory_q$ ($q = -2,-1$). The interpretation of the coefficients on the reporting-time indicator variables $Voluntary * Mandatory_q$ is similar, with $Voluntary * Mandatory_{+2}$ being most likely to capture investor reaction to financial statement information.

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[3] For example, for firms where the first-time IFRS fiscal period ends in December 2005, $\mathit{First\text{-}Time\ Mandatory}_{+1}$ is set equal to 1 for the first quarter of 2006, while $\mathit{First\text{-}Time\ Mandatory}_{-1}$ equals 1 in the fourth quarter of 2005.

[4] DeFond et al. (2007) report considerable variation in earnings announcement dates, with the median of country-level median earnings reporting lags being 83.25 days. Annual reports containing full financial statements are likely to follow at least several weeks later, depending on statutory reporting rules.

Country-Quarter Analysis

We also conduct country-level analysis relating aggregate changes in institutional holdings to changes in an index tracking the actual rate of reporting under IFRS in each country. The research design follows closely the country-month analysis of Daske et al. (2008). We estimate the following regression pooled over countries and quarters:

$$\Delta Institutional\ Holdings = \delta_0 + \delta_1 \Delta IFRS\ Adoption\ Rate + \Sigma\gamma_j Controls_j + \mu. \qquad (2)$$

In Equation (2) *ΔInstitutional Holdings* is the change in the median institutional holdings in a country-quarter and *ΔIFRS Adoption Rate* is the change in an index of cumulative IFRS adoption within a country based on the publication of mandatory IFRS financial statements and measured in two ways described below. Consistent with the firm-quarter analysis, we predict that *ΔIFRS Adoption Rate* will be associated with changes in institutional holdings with a one-quarter lag, reflecting the time taken for institutional investors to process information and rebalance portfolios.

We construct the IFRS adoption rate index for a country in two ways. First, we define IFRS adoption with reference only to the reporting of first-time IFRS annual financial statements. In this case and in line with the firm-quarter analysis, we assume that financial statement publication occurs in the quarter following the fiscal year-end. We assign a firm-level adoption counter a value of 1 starting in the quarter following the first-time IFRS fiscal year-end. In each calendar-quarter *IFRS Adoption Rate* is the average value of the firm-level adoption counter for the set of mandatory adopters in the country. The index has a maximum value of 1 in the quarter when the final firm in a country reports under IFRS for the first time. Similar to Daske et al. (2008), we label the index based on annual financial reporting as $IFRS\ Adoption\ Rate_{FYE}$.

If IFRS information arrives as a result of quarterly or interim announcements during the IFRS adoption year, then $IFRS\ Adoption\ Rate_{FYE}$ may understate the true rate of cumulative IFRS information arrival in the first-time adoption year and overstate the true rate in the following year. To address this possibility we also construct the *IFRS Adoption Rate* index with reference to quarterly, interim, and annual earnings announcement dates (Worldscope item WS05905), assuming that quarterly or interim financial reporting is under IFRS in the mandatory adoption year. Under this definition of the *IFRS Adoption Rate* index, in the quarter of an announcement we increase the firm-level adoption counter value by $1/n$, where n equals 4, 2, or 1 for quarterly, semi-annual interim, and annual announcements, respectively.[5] In each calendar-quarter *IFRS Adoption Rate* is the average value of the firm-level adoption counter for the set of mandatory adopters in the country. The index has a maximum value of 2 in the quarter when the final firm in a country reports annual financial statements under IFRS for the first time, having previously reported quarterly or interim statements. Similar to Daske et al. (2008), we label this variable $IFRS\ Adoption\ RATE_{INTERIM}$.

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[5] In some countries firms vary in the frequency of reporting. For example, some U.K. firms voluntarily report quarterly while being required to report semi-annual interim financial statements. Therefore, we define the firm-level reporting frequency for a given year (n) by counting the actual number of announcements reported by each firm in the year.

The Role of Institutions

We analyze the institutional context of IFRS financial reporting effects using a new approach. Our approach is similar to Daske et al. (2008) and Li (2010) in that we model IFRS effects conditional on proxies for the likelihood of high-quality enforcement and application of IFRS, and measures of accounting difference between local GAAP and IFRS. Our approach is different because we estimate the impact of complementary institutional features in a single model; we also use measures of accounting difference capturing the changes in accounting that can be expected when moving from an issuer's local GAAP to IFRS.

Collinearity between institutional proxies may arise because different institutions in a country do not develop independently and they can complement one another. This perspective predicts that institutional "bundles" will emerge (Wysocki 2011). This is confirmed by Leuz et al. (2003) and Leuz (2010) who apply cluster analysis to institutional proxies that capture the strength of securities regulation, legal enforcement, investor protection, disclosure, and transparency of reporting practices. We classify countries into three clusters defined in Leuz (2010) and characterized as outsider economies with strong outsider protection and legal enforcement (*Cluster 1*); insider economies with stronger rule enforcement (*Cluster 2*); and insider economies with weaker rule enforcement (*Cluster 3*).[6] We then allow the IFRS effects captured by *First-Time Mandatory*, *Voluntary*, and *Voluntary* $*$ *Mandatory* to vary across clusters. We predict that IFRS-related holdings changes for first-time mandatory adopters are likely to be more pronounced in the cases of *Clusters 1* and *2* because reporting incentives and enforcement are predicted to lead to higher quality reporting.

We consider whether IFRS-related changes in institutional holdings depend on the extent of accounting change involved in moving from local GAAP to IFRS reporting. We do not model the dependence of institutional holdings on country-pair differences between an investor's home country GAAP and an issuer's home country GAAP (Yu 2010). Rather we examine how the differences between local GAAP and IFRS influence changes in institutional holdings generally. Holding reporting incentives and enforcement constant, the mandatory switch to IFRS in countries with large differences between domestic accounting standards and IFRS should result in larger improvements in financial reporting quality and in comparability, to the benefit of all institutional investors irrespective of their location.

We estimate accounting differences using the two distinct measures proposed by Ding et al. (2005, 2007), who analyze a comprehensive set of 111 accounting items reported in the *GAAP 2001* survey of national accounting rules conducted by seven large auditing firms. *Absence* captures missing local GAAP financial reporting rules relative to IFRS. *Divergence* captures inconsistencies between local GAAP and IFRS treatments. Ding et al. (2007) show that these two dimensions are uncorrelated and argue that they cannot be considered as substitutes or complements. *Absence* implies an accounting issue is not important enough in a specific jurisdiction and/or is beyond the standard-setter's competence, while *Divergence* means the issue is important in the country and the standard-setter is competent and confident enough to adopt a non-IAS approach (Ding et al. 2009, 160).[7] We believe that the Ding et al. (2005, 2007) measures are appropriate in our research setting for four main reasons. First, our focus is on the IFRS-related ownership effects across institutional investors irrespective of their location. The *Absence* and *Divergence* measures capture the potential accounting changes that can benefit investors generally. Second, the Ding et al. (2005, 2007) measures are based on a comprehensive list of potential discrepancies. Third, the distinction between absence and divergence could be important.[8] Fourth, empirical evidence in Garcia Osma and Pope (2011) indicates that *Divergence* explains country-level adjustments to local GAAP numbers on first-time IFRS adoption, whereas alternative measures of accounting difference do not or they have the wrong sign.

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[6] Consistent with institutions being associated with financial reporting quality, Leuz (2010) reports higher disclosure and more informative earnings in *Cluster 1* relative to *Cluster 2*, and in *Cluster 2* relative to *Cluster 3*.

[7] *Divergence* may also indicate that the national GAAP covered a particular issue before it was covered by IAS.

We predict that IFRS-related changes in institutional holdings for first-time mandatory adopters will be positively related to *Divergence* and *Absence* if differences between alternative accounting regimes are important to investors. To implement these measures in empirical tests we generate binary indicators from the *Divergence* and *Absence* continuous variables using the medians across IFRS adoption countries as the classification cut-off.

Empirical Proxies

Institutional Holdings

In contrast to previous and concurrent research that focuses on the ownership of mutual funds (Covrig et al. 2007; Yu 2010; DeFond et al. 2011), this is the first study to examine the links between IFRS reporting and global institutional ownership including investors other than mutual funds. Our institutional holdings data are drawn from the Thomson Financial Ownership (TFO) quarterly data feed and include domestic and foreign holdings of mutual funds, pension funds, insurance companies, hedge funds, private equity funds, and venture capital funds from 45 countries.[9] An advantage of our data is that the data capture a much higher proportion of total institutional ownership. For example, we document average ownership of almost 22 percent, in contrast to Covrig et al. (2007), Yu (2010), and DeFond et al. (2011) who report a mean value of mutual fund ownership between 2 percent and 10 percent. Under the assumption that decisions by non-mutual fund institutional owners reflect informed investor opinion, the expanded scope of our institutional investor universe enhances the power of our tests to detect IFRS-related effects.[10]

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[8] Based on the same data source, Bae et al. (2008) propose two measures (*gaapdif1* and *gaapdif2*) of differences in accounting standards at the country-pair level. Moreover, these measures are a blend of absence of local GAAP rules and divergence between local GAAP and IAS and they are not intended to provide a comprehensive catalogue of country-level differences from IAS. Bae et al. (2008) construct country scores based on differences relating to 21 (*gaapdiff1*) or 52 (*gaapdiff2*) "key" accounting items, selected because they appear in the past research literature (*gaapdiff1*) and because they appear frequently in the survey. The focus on country-pairs restricts the set of accounting rules considered because countries that differ from IFRS are also required to have similar GAAP. As a consequence, relative to *Absence* and *Divergence*, the Bae et al. (2008) measures are expected to be noisy measures of the distance between local GAAP and IAS for two reasons. First, they exclude some important sources of differences. For example, *gaapdiff1* excludes 12 standards in IAS, 11 of which affect recognition and measurement, including IAS 39 (*Financial Instruments*), where *Divergence* is often high and accounts for 6 out of the 111 items considered by Ding et al. (2005, 2007). Second, *gaapdiff1* also covers two standards (IAS 1, *Presentation of Financial Statements*, and IAS 24, *Related Party Disclosures*) where there is a high degree of local GAAP conformity with IAS (81 percent and 73 percent, respectively). The *gaapdiff1* measure with respect to these two items captures *Absence* only. We thank an anonymous reviewer for helpful comments on the relative merits of the alternative measures of accounting difference.

[9] Thomson Reuters state that the database includes holdings on more than 44,000 global equity issues. The source of the data depends on the region. Specifically, in the case of Europe, Middle East, Africa, South/Central America, and Asia, information on equity holdings is processed primarily from *Shareholder*, *Trade*, and *Mutual Fund Portfolio Reports*, which gather data from annual and interim reports, stock exchanges, regulatory bodies, investor relations departments, and third-party vendors. In the case of U.K. and Ireland holdings, data above a 0.015 percent threshold is sourced from inspection of firms' registers of shareholders. In the case of the U.S., security information is gathered from SEC 13D, 13G, 10K, 20F, and 13F filings and from mutual fund and pension fund portfolio reports.

To obtain our holdings proxies we employ the following procedures. First, we retrieve disaggregated holdings data on the number and value of institutional shares from the Historical Holdings Files for each quarter in the period 2003–2007. Second, using the Historical Security File we identify all types of common and registered equity securities owned by institutional investors for each firm. ADR holdings are converted to the equivalent number of shares of the underlying equity class by multiplying ADR holdings by the ADR-underlying conversion ratio. From this file we also collect data on various items including share price, outstanding shares, and security identifiers. Finally, we retrieve data on investor orientation (active or passive) and investor style (value, growth, income, and index) from the Historical Owner File.

In line with prior studies of institutional ownership we consider the aggregate percentage ownership of institutional shareholders (Covrig et al. 2007; DeFond et al. 2011) and the total number of institutional investors (O'Brien and Bhushan 1990; Walther 1997; Bradshaw et al. 2004). Percentage ownership is calculated as the total market value of shares owned by institutional investors (or investors having particular orientations or style) divided by the total market value of the firm at the end of a quarter. The number of institutional investors is the count of investors with recorded holdings at the end of a quarter.

Control Variables

Following prior literature (Leuz et al. 2010; DeFond et al. 2011), we include control variables documented to be associated with institutional holdings: *Nanalysts* (the number of analysts at the fiscal year-end from I/B/E/S) to proxy for richness of the information environment; *Market Index* (Worldscope item WS05661), a dummy variable that equals 1 if the firm is included in any national stock market index and *MSCI Index*, a binary variable set equal to 1 if the firm is included in the MSCI World Index, to control for the firm's visibility to investors;[11] *ADR* (WS11496), a dummy that takes the value of 1 if the firm has an ADR listed on a U.S. exchange, to proxy for the degree of visibility and information environment richness of non-U.S. firms; *Big4* (WS07800), a dichotomous variable equal to 1 if the firm is audited by a Big 4 firm or their predecessors, to control for investor preferences for auditors with a strong reputation; *Size* (WS07210), defined as the natural logarithm of market value of equity in U.S. dollars at year-end, to proxy for liquidity, information environment, and fiduciary concerns; *ROE*, measured as net income before extraordinary items (WS01551) divided by total assets at the beginning of the year (WS02999), to control for accounting performance; *RET*, calculated as log of (RI_{t+1}/RI_t), where RI is the DataStream Total Return Index on first January, to capture the effect of stock market performance over the fiscal year; *RetVar*, measured as the standard deviation of monthly stock returns over the fiscal year, to control for firm risk.

In addition, we include in our set of controls a number of proxies for firm fundamentals that are potentially related to institutional holdings, namely *Dividend Yield*, computed as total dividends

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[10] Further, mutual fund investment decisions are partially driven by (retail) customer funds flows and, therefore, mutual fund ownership changes reflect effects related to private investor sentiment and liquidity needs. In contrast, many of the non-mutual fund investors in our database are longer-term investors, e.g., pension funds and insurance companies, whose investment decisions are less susceptible to swings in private investor sentiment and liquidity.

[11] *Market Index* is available as a Worldscope data item, but is time-invariant and available for the most recent year of our study, i.e., 2006. *MSCI Index* is available on a time-varying basis. We thank Old Mutual Asset Management for providing these data.

(WS05376) divided by market value of equity at year-end (WS08001); *Book-Market*, defined as book value of equity (WS03501) divided by market value of equity at year-end (WS08001); *Earnings-Price*, calculated as net income (WS01551) divided by market value of equity at year-end (WS08001); and *Leverage*, defined as total liabilities (WS03351) divided by total assets at year-end (WS02999). Following Daske et al. (2008), we also include *US GAAP* (WS07536), a dummy variable equal to 1 if a non-U.S. firm prepares its financial statements according to U.S. financial reporting rules, and *Market Benchmark*, calculated as the yearly mean of the dependent variable from all observations in the benchmark sample. The latter variable is used to help us disentangle the IFRS effects from effects due to changes in the general economic conditions or changes in institutional holdings unrelated to IFRS reporting. All continuous variables are winsorized at the extreme 1 percentiles.

To perform the firm-quarter analysis, we incorporate in Equation (1) only those control variables that can be measured at the end of a quarter, i.e., *Size*, *Ret*, *RetVar*, and *Market Benchmark*. In the country-quarter analysis based on Equation (2) we include the natural logarithm of *Market Value* (WS08001), measured as stock price times the number of shares outstanding (in U.S. dollars) at the end of quarter *q*–3; *Return Variability*, defined as the standard deviation of the previous 12 monthly stock returns measured at time *q*–3; *Market Benchmark*, defined as the mean dependent value from all observations in the benchmark sample; and the level and the change in the dependent variables measured at time *q*–3.

Sample and Descriptive Statistics

We include all listed firms in the Worldscope database in the period 2003–2006 with complete data for the independent variables and for which ownership data are available in the TFO database.[12] For firms that adopt IFRS for fiscal years ending December 2005, this sample period allows us to compare changes in institutional holdings in the year prior to mandatory IFRS adoption, the transition year, and the year after first-time adoption. Applying these criteria yields a sample of 35,160 firm-years for 10,852 unique firms and 45 countries. Table 1 describes the sample composition across years and countries. Panel A indicates that 9.61 percent (4.92 percent) of firm-years in the overall sample are mandatory (voluntary) IFRS adopters. Mandatory IFRS reporters represent 14.15 percent (21.93 percent) of sample observations for fiscal years ending in 2005 (2006), indicating that a substantial number of firms adopt IFRS for the first time for fiscal years ending in 2006 rather than 2005. Panel B of Table 1 shows the country composition of the IFRS treatment sample, which includes 11,926 observations from 3,865 firms and 24 countries, and the benchmark sample, which comprises 23,234 firm-year observations from 6,987 firms and 21 countries.[13] As in prior studies adopting a similar sampling frame, the U.S. accounts for approximately 43 percent of benchmark sample observations. We test whether our main results are sensitive to the weight of the U.S. in the benchmark sample in robustness checks.

Table 2 describes the institutional holdings variables. Panel A reveals that over the full

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[12] We constrain the sample to end in 2006 because extending the sample period beyond 2006 would increase the risk that any observed changes in institutional holdings in treatment firms relative to benchmark firms are due to factors other than IFRS adoption.

[13] The apparent discrepancy between the 3,380 total IFRS mandatory adopters reported in Panel A and the 3,865 total treatment firms reported in Panel B is due to some treatment countries not requiring IFRS reporting by all listed companies, e.g., because, the firms do not report consolidated financial statements. Singapore is excluded from our sample because it mandated adoption of IFRS in 2003, in contrast to all other countries in our treatment sample where the adoption date was 2005.

TABLE 1

IFRS Adoption by Year and Sample Composition by Country

Panel A: IFRS Adoption for Full Sample by Year

		First-Time Mandatory Adopters		*Voluntary* Adopters	
Year	**Total Firm-Years**	**Firm-Years**	**Percent**	**Firm-Years**	**Percent**
2003	8,385	0	0.00	458	5.46
2004	8,364	0	0.00	468	5.60
2005	8,450	1,196	14.15	399	4.72
2006	9,961	2,184	21.93	406	4.08
Total	35,160	3,380	9.61	1,731	4.92

Panel B: Sample Composition by Country

Treatment	**Firms**	**Firm-Years**	**Benchmark**	**Firms**	**Firm-Years**
Australia	482	1,271	Argentina	36	121
Austria	42	138	Bermuda	10	26
Belgium	63	226	Brazil	76	240
Czech Republic	9	26	Canada	459	1,530
Denmark	99	300	Chile	72	145
Finland	90	304	China	33	95
France	395	1,377	Colombia	10	22
Germany	282	953	Egypt	15	40
Greece	94	262	India	64	175
Hong Kong	529	1,451	Indonesia	95	261
Hungary	19	61	Israel	62	197
Ireland	41	143	Japan	2,097	6,838
Italy	160	546	Korea (South)	246	602
Luxemburg	14	53	Malaysia	372	1,018
The Netherlands	95	316	Mexico	50	142
Norway	72	235	New Zealand	27	72
Philippines	66	163	Russia	12	34
Poland	72	215	Taiwan	418	1,141
Portugal	27	101	Thailand	133	357
South Africa	113	405	Turkey	41	77
Spain	96	342	United States	2,659	10,101
Sweden	112	238			
Switzerland	150	376			
U.K.	743	2,424			
Total	3,865	11,926	Total	6,987	23,234

The complete sample consists of a maximum 35,160 firm-year observations from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis. We exclude countries with less than 20 firm-year observations.

Panel A reports descriptive information for the following indicators: (1) *First-Time Mandatory* that refers to observations of firms that never reported under IFRS before mandatory adoption and equals 1 for all firm-years with IFRS reporting periods ending on or after the local mandated IFRS adoption date (i.e., December 31, 2005), and 0 otherwise; and (2) *Voluntary* that relates to observations of firms that voluntarily switched to IFRS reporting before it was mandated and equals 1 for all firm-years with IFRS reporting by voluntary adopters, and 0 otherwise.

Panel B reports the number of firms and firm-years for: (1) the treatment sample, i.e., countries that mandated IFRS reporting and (2) the benchmark sample, i.e., countries that do not allow or do not require IFRS reporting.

TABLE 2

Descriptive Statistics for Institutional Holdings

Panel A: Full Sample

Variable	**Obs.**	**Mean**	**Median**	**Std. Dev.**
Percentage Ownership	33,131	21.69	6.93	0.291
ΔPercentage Ownership	21,726	2.67	0.87	0.079
Number of Investors	34,330	74.88	18.00	150.100
ΔNumber of Investors	22,826	10.38	3.00	28.130

Panel B: Percentage Ownership for Full Sample by Investor Type

Investor Type	**Obs.**	**Mean**	**Median**	**Std. Dev.**
Active	33,131	18.62	6.63	0.245
Passive	33,131	3.22	0.00	0.062
Growth	33,131	5.40	1.75	0.083
Value	33,131	5.94	1.22	0.095
Income	33,131	0.55	0.00	0.019
Index	33,131	2.67	0.00	0.052

Panel C: Number of Investors for Full Sample by Investor Type

Investor	**Obs.**	**Mean**	**Median**	**Std. Dev.**
Active	34,330	63.72	16.00	133.47
Passive	34,330	11.16	1.00	19.77
Growth	34,330	23.35	7.00	49.32
Value	34,330	18.30	4.00	39.44
Income	34,330	2.38	0.00	7.67
Index	34,330	6.53	0.00	11.03

The complete sample consists of a maximum 35,160 firm-year observations from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis. *Percentage Ownership* is defined as the total market value of shares owned by institutions divided by the total market value of the firm at year-end, where both items are measured in U.S. dollars. *Number of Investors* refers to the total number of institutional investors of the firm at year-end. Δ denotes changes in variables computed over year *t*–1 to *t*. Additional descriptive information is provided conditioning on: (1) Investment Orientation, i.e., active versus passive investors and (2) Investment Style, i.e., value/growth versus index/income investors.

sample mean (median) institutional ownership is 21.69 percent (6.93 percent) of issued equity and the mean (median) number of institutional shareholders is 74.88 (18.00). The changes in both institutional holdings proxies are, on average, positive over the sample period. Panels B and C provide insights to the importance of different investor types in our data. Most firms have active institutional investors—on average there are 63.72 active investors owning 18.62 percent of equity. In contrast, passive investors have lower overall equity stakes than active investors and are fewer in number—on average 11.16 passive investors own equity stakes totaling 3.22 percent of equity. Analysis of investment styles reveals that growth and value investors hold stakes in a large proportion of sample firm-years and holdings are economically significant. For example, value investors hold mean equity stakes of 5.94 percent.

TABLE 3

Descriptive Statistics of Independent Variables

Variable (n = 35,160)	Mean	Median	Std. Dev.
Size	5.99	5.86	1.853
ROE (%)	6.34	9.68	0.414
RET (%)	19.38	18.29	0.429
RetVar	0.102	0.086	0.062
Leverage	0.521	0.515	0.262
Dividend Yield	0.016	0.009	0.022
Book-Market	0.669	0.539	0.608
Earnings-Price	0.004	0.047	0.219
Nanalysts	4.426	2.000	6.064
Big4	0.855	1.000	0.351
MSCI Index	0.103	0.000	0.304
Market Index	0.527	1.000	4.999
ADR	0.067	0.000	0.251
US GAAP	0.018	0.000	0.133

The complete sample consists of a maximum 35,160 firm-year observations from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis.
All financial variables are winsorized at the 1st and 99th percentiles.

Variable Definitions:
Size = the natural logarithm of market value of equity in U.S. dollars;
ROE = net income before extraordinary items divided by total assets at the beginning of the year;
RET = log of (RI_{t+1}/RI_t), where *RI* stands for Return Index on January 1st;
RetVar = standard deviation of monthly stock returns over the fiscal year;
Leverage = total liabilities divided by total assets;
Dividend Yield = total dividends divided by market value of equity;
Book-Market = book value of equity divided by market value of equity;
Earnings-Price = net income divided by market value of equity;
Nanalysts = number of analysts;
Big4 = dichotomous variable equal to 1 if the firm is audited by a Big 4 auditing firm or their predecessors, and 0 otherwise;
MSCI Index = binary variable equals 1 if the firm is included in the MSCI World Index, and 0 otherwise;
Market Index = dummy variable equals 1 if the firm is part of any stock market index, and 0 otherwise;
ADR = dummy that takes the value of 1 if the firm has an ADR listed on a U.S. exchange, and 0 otherwise; and
US GAAP = dummy variable equal to 1 if a non-U.S. firm prepares its financial statements according to the U.S. financial reporting rules, and 0 otherwise.

Table 3 describes the control variables used in our regression tests and Table 4 shows the correlations between the independent variables. Descriptive statistics are pooled over the full sample. The data do not suggest any unusual behavior.[14]

IV. EMPIRICAL RESULTS

In this section we discuss our empirical findings. First, we report univariate difference-in-differences results, followed by baseline regression results capturing the average effects of

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[14] Untabulated findings document low correlations among the institutional factors employed in our cross-section analysis, *Absence*, *Divergence*, and *Cluster 1* and *Cluster 2* indicators.

TABLE 4

Correlations between Independent Variables

Panel A: Correlations of Variables *First-Time Mandatory* to *Book-Market*

	First-Time Mandatory	*Voluntary*	*RET*	*ROE*	*Leverage*	*Dividend Yield*	*Book-Market*
Voluntary	−0.074						
RET	0.034	0.013					
ROE	0.045	−0.001	0.135				
Leverage	0.051	0.045	−0.009	−0.001			
Dividend Yield	0.063	0.002	−0.028	0.147	0.023		
Book-Market	−0.070	−0.008	−0.053	−0.019	−0.190	0.139	
Price-Earnings	0.028	−0.004	0.220	0.394	−0.152	0.177	0.084
Size	0.013	0.047	0.144	0.216	0.130	0.063	−0.246
RetVar	−0.069	−0.053	0.104	−0.237	−0.042	−0.270	−0.106
Nanalysts	0.034	0.077	−0.005	0.120	0.065	0.002	−0.227
Market Index	−0.005	−0.000	0.032	0.107	0.109	0.070	−0.017
Big5	−0.068	0.010	0.011	0.035	0.039	0.046	−0.057
ADR	0.061	0.065	0.021	0.024	0.036	0.064	−0.026
MSCI Index	0.041	0.033	−0.000	0.058	0.071	0.018	−0.090
US GAAP	−0.044	−0.013	−0.002	−0.008	−0.024	−0.029	−0.017

Panel B: Correlations of Variables *Price-Earnings* to *MSCI Index*

	Price-Earnings	*Size*	*RetVar*	*Nanalysts*	*Market Index*	*Big5*	*ADR*	*MSCI Index*
Size	0.270							
RetVar	−0.351	−0.354						
Nanalysts	0.101	0.701	−0.197					
Market Index	0.125	0.532	−0.230	0.359				
Big5	0.060	0.264	−0.080	0.179	0.139			
ADR	0.018	0.251	−0.051	0.245	0.132	0.068		
MSCI Index	0.049	0.413	−0.146	0.403	0.252	0.065	0.166	
US GAAP	−0.021	0.074	0.025	0.099	−0.009	0.029	0.195	0.067

The complete sample consists of a maximum 35,160 firm-year observations from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis.

Pearson's correlation coefficients between independent variables are reported. All financial variables are winsorized at the 1st and 99th percentiles. For variable definitions see Tables 1 and 3.

IFRS adoption on holdings changes measured over different annual windows. Second, we provide further analysis showing that changes in institutional holdings are concentrated in the first quarter after first-time mandatory IFRS reporting. Third, we present results of the country-quarter analysis. Fourth, we report additional findings on the IFRS-related ownership effects across different types of investors. Fifth, we discuss results on the dependence between IFRS-related holdings changes and country-level institutional factors capturing reporting incentives, compliance, and accounting differences. Finally, we describe a series of additional tests and robustness checks.

TABLE 5

Difference-in-Differences Analyses of Ownership

Panel A: Difference-in-Differences Analysis for Δ*Percentage Ownership*

Sample	Pre-IFRS (a)	Transition Period (b)	Post-IFRS (c)	(b) − (a)	(c) − (a)
Benchmark (i)	2.14	2.20	3.95	0.06	1.81
	(n = 4,614)	(n = 4,759)	(n = 5,050)	(0.699)	(0.000)
Treatment (ii)	0.27	0.30	6.31	0.03	6.04
	(n = 2,358)	(n = 2,339)	(n = 2,606)	(0.845)	(0.000)
(ii) − (i)	−1.87	−1.90	2.36	−0.03	4.23
	(0.000)	(0.000)	(0.000)	(0.647)	(0.000)

Panel B: Difference-in-Differences Analysis for Δ*Number of Investors*

Sample	Pre-IFRS (a)	Transition Period (b)	Post-IFRS (c)	(b) − (a)	(c) − (a)
Benchmark (i)	7.21	7.17	18.53	−0.04	11.32
	(n = 5,017)	(n = 5,043)	(n = 5,294)	(0.936)	(0.000)
Treatment (ii)	0.64	1.81	23.10	1.17	22.46
	(n = 2,459)	(n = 2,393)	(n = 2,620)	(0.001)	(0.000)
(ii) − (i)	−6.57	−5.36	4.57	1.21	11.14
	(0.000)	(0.000)	(0.000)	(0.038)	(0.000)

The complete sample consists of a maximum 35,160 firm-year observations from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis.

Panels A and B report results from difference-in-differences analysis of the IFRS-related ownership effects; p-values in parentheses.

Percentage Ownership is defined as the total market value of shares owned by institutions divided by the total market value of the firm at year-end, where both items are measured in U.S. dollars. *Number of Investors* refers to the total number of institutional investors of the firm at year-end. Δ denotes changes in variables computed over year $t-1$ to t. The treatment sample consists of countries that mandated IFRS reporting and the benchmark sample includes countries that do not allow or do not require IFRS reporting. The pre-IFRS period refers to changes in 2004, while the transition period and the post-IFRS period relate to changes over 2005 and 2006, respectively.

Difference-in-Differences Analysis

Table 5 presents difference-in-differences analyses showing the contrast between institutional holdings changes for the treatment and benchmark samples over the pre-IFRS period (2004), the IFRS adoption year (2005) and the post-IFRS period (2006). We report results for changes in our two institutional holdings proxies, *Percentage Ownership* and *Number of Investors*.

The differences between treatment sample and benchmark sample holdings changes in the bottom row of each panel indicate that changes in institutional holdings are significantly higher in the benchmark sample in the pre-IFRS year and the transition year for both holdings proxies. In contrast, institutional holdings changes in the post-IFRS year are significantly higher in the treatment sample. Both panels reveal that institutional holdings grow over the sample period for both the treatment and the benchmark samples. Mean changes in percentage ownership and number of investors are uniformly positive for all sample periods. However, the final two columns indicate that differences in percentage ownership changes are significant only in the post-IFRS period. Results for the change in the number of investors indicate relatively large increases in the post-IFRS period and a small increase in the IFRS implementation year relative to the pre-IFRS year. The observation that benchmark sample holdings also increase in the post-IFRS period emphasizes the importance of controlling for common global factors underpinning institutional holdings changes.

The difference-in-differences results at the bottom right-hand side of each panel indicate that treatment sample firms have statistically and economically significantly higher holdings changes in the post-IFRS period compared to benchmark sample firms. These results provide the first evidence that mandatory IFRS reporting has a positive impact on institutional holdings. We now examine whether coincident changes in other determinants of institutional holdings can explain the IFRS reporting effects.

Baseline Findings

Table 6 presents the results of estimating Equation (1) for the full sample. These and subsequent regression results are estimated with industry-year fixed effects captured by indicator variables using the industry classifications in Campbell (1996), country dummy variables based on Worldscope nation code (WS06027), and White standard errors adjusted to account for correlation within firm clusters (Rogers 1994; Petersen 2009).

In Table 6, Panel A the changes in institutional holdings proxies are measured over calendar-year intervals, where the mandatory IFRS period is defined as 2005 and 2006. Hence, the estimated coefficients on *First-Time Mandatory* and *Voluntary * Mandatory* capture average effects over the two-year period. Empirical findings indicate that there is an increase in average institutional ownership of 2.2 percent per annum for first-time mandatory IFRS adopters, after controlling for global changes affecting benchmark sample firms and changes in other firm-level determinants of institutional ownership. Similarly, the number of investors in mandatory IFRS adopters increases by nearly five relative to benchmark firms in the IFRS adoption period. The positive coefficient on *Voluntary * Mandatory* suggests that voluntary adopters also experience an increase in institutional holdings at the time of mandated adoption. This result is in line with Yu (2010) and with the liquidity effects for voluntary adopters at the time of mandated adoption reported by Daske et al. (2008). A possible explanation is that voluntary adopters also benefit from increased comparability when IFRS is mandated (Daske et al. 2008; DeFond et al. 2011).

In Table 6, Panel B the changes in institutional holdings are measured over different annual intervals ordered relative to the fiscal year-end of each firm. Results for both alternative windows −2/+2 and −1/+3 are consistent with those in Panel A, indicating statistically and economically significant ownership effects for first-time mandatory IFRS adopters. *First-Time Mandatory* and *Voluntary * Mandatory* coefficient estimates for both holdings proxies are higher for the −2/+2 interval than for the calendar-year intervals. However, the R^2 statistics are considerably lower for both alternative windows, suggesting that this research design does not control as effectively for unmodeled factors.

Firm-Quarter Analysis

The results in Table 6 capture the average institutional holdings effects associated with IFRS reporting measured over annual intervals. To alleviate concerns that these results could reflect factors other than IFRS reporting, we now examine the timing of institutional ownership changes

TABLE 6

Changes in Institutional Holdings and IFRS Mandatory Adoption Firm-Annual Analysis

Panel A: Base Model

Independent Variables	*ΔPercentage Ownership* (1)	*ΔNumber of Investors* (2)
First-Time Mandatory	0.022	4.938
	(0.000)	(0.000)
Voluntary	0.008	1.970
	(0.002)	(0.091)
*Voluntary * Mandatory*	0.006	4.027
	(0.034)	(0.003)
ΔSize	0.046	18.38
	(0.000)	(0.000)
ΔROE	−0.002	0.134
	(0.162)	(0.718)
ΔRET	−0.013	−3.929
	(0.000)	(0.000)
ΔRetVar	−0.051	−0.087
	(0.000)	(0.976)
ΔLeverage	−0.013	−6.636
	(0.079)	(0.000)
ΔDividend Yield	−0.039	0.658
	(0.224)	(0.934)
ΔBook-Market	0.006	1.753
	(0.001)	(0.000)
ΔEarnings-Price	0.000	−3.352
	(0.925)	(0.000)
ΔNanalysts	0.002	1.623
	(0.000)	(0.000)
ΔBig4	−0.042	5.112
	(0.000)	(0.000)
ΔMSCI Index	−0.008	0.304
	(0.000)	(0.683)
ΔUS GAAP	0.004	2.094
	(0.450)	(0.475)
ΔMarket Benchmark	2.327	1.760
	(0.000)	(0.000)
Intercept	−0.088	−31.22
	(0.000)	(0.000)
Country Dummies	Yes	Yes
Industry-Year Dummies	Yes	Yes
No. Observations	21,726	22,826
No. Countries	45	45
Adj. R^2	12.5%	18.5%

(continued on next page)

TABLE 6 (continued)

Panel B: Alternative Annual Windows

	ΔPercentage Ownership		ΔNumber of Investors	
Independent Variables	−2/+2 (1)	−1/+3 (2)	−2/+2 (3)	−1/+3 (4)
First-Time Mandatory	0.025	0.017	8.977	6.718
	(0.000)	(0.000)	(0.000)	(0.000)
Voluntary	0.008	0.005	3.129	3.137
	(0.005)	(0.058)	(0.008)	(0.011)
*Voluntary * Mandatory*	0.011	0.012	6.022	3.799
	(0.001)	(0.000)	(0.000)	(0.001)
Control Variables	Yes	Yes	Yes	Yes
Country Dummies	Yes	Yes	Yes	Yes
Industry-Year Dummies	Yes	Yes	Yes	Yes
No. Observations	20,733	20,468	21,497	21,150
No. Countries	45	45	45	45
Adj. R^2	6.8%	5.5%	10.8%	7.7%

The complete sample consists of a maximum 35,160 firm-year observations from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis.

Panel A refers to the base model, where institutional holdings are measured at the end of each calendar year.

Panel B refers to two alternative annual windows: in models 1 and 3 holdings changes are measured over the last two quarters of a fiscal year and the first two quarters of the next fiscal year; in models 2 and 4 holdings changes are measured over the final quarter of a fiscal year and the first three quarters of the next fiscal year.

Financial variables are winsorized at the 1st and 99th percentiles. All models are estimated with White standard errors adjusted to account for correlation within firm clusters; p-values in parentheses. For all other control variable definitions see Table 3.

Variable Definitions:

Percentage Ownership = total market value of shares owned by institutions divided by the total market value at firm-year level, where both items are measured in U.S. dollars;

Number of Investors = total number of institutional investors at firm-year level;

Δ = changes in variables computed over year $t-1$ to t;

First-Time Mandatory = observations of firms that never reported under IFRS before mandatory adoption and equals 1 for all firm-years with IFRS reporting periods ending on or after the local mandated IFRS adoption date (i.e., December 31, 2005), and 0 otherwise;

Voluntary = observations of firms that voluntarily switched to IFRS reporting before it was mandated and equals 1 for all firm-years with IFRS reporting by voluntary adopters, and 0 otherwise; and

*Voluntary * Mandatory* = 1 for all voluntary adopter firm-years ending on or after the mandated local IFRS adoption date, and 0 otherwise.

during 2005–2006 by isolating incremental changes over the quarters spanning firms' fiscal year-end dates. Results are reported in Table 7.

The significant positive coefficients on *First-Time Mandatory* and *Voluntary * Mandatory* indicate that, on average, changes in institutional holdings are positive over the eight quarters in the mandatory IFRS period. However, the incremental reporting-time coefficients indicate that changes in holdings also depend on annual financial reporting events. Significant fractions of the IFRS-related holdings changes for first-time mandatory adopters reported in Table 6 are concentrated in quarter +2, the quarter after the release of most IFRS financial statements. For example, the coefficient on $First\text{-}Time\ Mandatory_{+2}$ for percentage ownership indicates an average incremental ownership change in quarter +2 of 1.4 percent. In contrast, incremental changes in holdings for first-time mandatory firms in other quarters before and after the release of annual financial

TABLE 7

Changes in Institutional Holdings and IFRS Mandatory Adoption Firm-Quarter Analysis

Independent Variables	*ΔPercentage Ownership* (1)	*ΔNumber of Investors* (2)
First-Time Mandatory	0.003	0.535
	(0.000)	(0.000)
Voluntary	0.002	0.435
	(0.006)	(0.021)
*Voluntary * Mandatory*	0.002	0.501
	(0.025)	(0.013)
First-Time Mandatory$_{-2}$	−0.000	−0.322
	(0.914)	(0.057)
First-Time Mandatory$_{-1}$	0.001	0.544
	(0.358)	(0.007)
First-Time Mandatory$_{+1}$	−0.000	0.188
	(0.886)	(0.347)
First-Time Mandatory$_{+2}$	0.014	4.302
	(0.000)	(0.000)
First-Time Mandatory$_{+3}$	−0.002	1.081
	(0.001)	(0.000)
*Voluntary * Mandatory*$_{-2}$	−0.003	−1.128
	(0.004)	(0.000)
*Voluntary * Mandatory*$_{-1}$	−0.003	−0.437
	(0.005)	(0.104)
*Voluntary * Mandatory*$_{+1}$	−0.004	−1.062
	(0.001)	(0.000)
*Voluntary * Mandatory*$_{+2}$	0.017	6.850
	(0.000)	(0.000)
*Voluntary * Mandatory*$_{+3}$	0.000	1.238
	(0.903)	(0.000)
ΔSize	0.015	8.696
	(0.000)	(0.000)
ΔRET	−0.009	−6.328
	(0.000)	(0.000)
ΔRetVar	−0.023	−2.590
	(0.000)	(0.013)
ΔMarket Benchmark	1.390	1.112
	(0.000)	(0.000)
Intercept	−0.012	−3.484
	(0.000)	(0.000)
Country Dummies	Yes	Yes
Industry-Year Dummies	Yes	Yes
No. Observations	85,741	89,368
No. Countries	45	45
Adj. R^2	9.5%	15.5%

The sample in models 1 and 2 consists of a maximum 85,741 and 89,368 firm-quarter observations, respectively, from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis.

(continued on next page)

TABLE 7 (continued)

Financial variables are winsorized at the 1st and 99th percentiles. All models are estimated with White standard errors adjusted to account for correlation within firm clusters; p-values in parentheses. For all other control variable definitions see Table 3.

Variable Definitions:
Percentage Ownership = total market value of shares owned by institutions divided by the total market value of the firm at the end of a quarter, where both items are measured in U.S. dollars;
Number of Investors = total number of institutional investors at firm-quarter level;
Δ = changes in variables computed over quarter $q-1$ to q;
First-Time Mandatory = observations of firms that never reported under IFRS before mandatory adoption and equals 1 for all firm-years with IFRS reporting periods ending on or after the local mandated IFRS adoption date (i.e., December 31, 2005), and 0 otherwise;
Voluntary = observations of firms that voluntarily switched to IFRS reporting before it was mandated and equals 1 for all firm-years with IFRS reporting by voluntary adopters, and 0 otherwise;
*Voluntary * Mandatory* = 1 for all voluntary adopter firm-years ending on or after the mandated local IFRS adoption date, and 0 otherwise; and
$First\text{-}Time\ Mandatory_q$ ($q = -2, -1, +1, +2, +3$) = 1 in quarter q and 0 in other quarters, using the convention that $q = -1$ is the quarter of the fiscal year-end and $q = +1$ is the quarter following the fiscal year-end. The indicator variables $Voluntary * Mandatory_q$ are defined in an exactly analogous manner to $First\text{-}Time\ Mandatory_q$.

statements are generally small in economic terms, and with three exceptions, statistically insignificant.[15]

The coefficients on $Voluntary * Mandatory_{+2}$ indicate that the incremental holding changes for voluntary adopters in quarter +2 are also positive and significant in economic terms, again accounting for a large fraction of the overall change documented for voluntary adopters in the mandated IFRS period. However, in contrast to the results for mandatory adopters, incremental holdings changes for voluntary adopters in the three prior quarters −2, −1, and +1 are significantly negative, implying that institutional holdings in these firms fall in the quarters leading up to the mandating of IFRS for other firms and suggesting that holdings changes follow different dynamics for voluntary compared to first-time adopters. Overall, the results in Table 7 provide reassurance that the main results reported in Table 6 are associated with financial reporting events in the mandatory IFRS period.

Country-Quarter Analysis

We now report the results of the country-quarter analysis, based on Equation (2). In this analysis we run regressions of the quarterly aggregate change in median institutional holdings in a country on the change in the adoption rate in that country in the previous quarter. In the first analysis we estimate the adoption rate index assuming that first-time adoption occurs by the end of the quarter following the fiscal year-end of first-time adoption ($IFRS\ Adoption\ Rate_{FYE}$). We can measure changes in this variable over five quarters (four quarters in 2006 and the first quarter in 2007), yielding 120 country-quarter observations over the 24 treatment countries. In the second analysis we base the adoption rate index on actual annual, interim, and quarterly announcement dates ($IFRS\ Adoption\ Rate_{INTERIM}$). We can measure changes in this variable over nine quarters (four quarters in each of 2005 and 2006 and the first quarter of 2007) yielding 216 country-quarter observations over the 24 treatment countries. Panel A of Table 8 reports descriptive statistics for

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[15] The sum of the $First\text{-}Time\ Mandatory_q$ coefficients in Table 7 closely reconcile with the *First-Time Mandatory* coefficient in Table 6. From Table 7 the estimated total holdings change over the two-year period 2005–2006 is $8 \times First\text{-}Time\ Mandatory + \Sigma_q First\text{-}Time\ Mandatory_q$, giving 0.037 (10.07) for percentage ownership (number of investors). The equivalent estimated changes from Table 6 over the two years equal to $2 \times First\text{-}Time\ Mandatory$, giving 0.044 (9.876) for the above two holdings proxies, respectively.

TABLE 8

Country-Quarter Analysis of the Effects of Mandatory IFRS Adoption on Institutional Holdings

Panel A: Descriptive Statistics of Dependent and Independent Variables Used in the Country-Quarter Analysis

Variable	Obs.	Mean	Median	Std. Dev
Δ*Percentage Ownership*	120	0.42	0.00	0.038
	216	0.25	−0.00	0.031
Δ*Number of Investors*	120	0.85	0.00	5.732
	216	0.53	0.00	5.254
Δ*IFRS Adoption Rate*$_{FYE}$ (%)	120	18.13	6.76	0.253
Δ*IFRS Adoption Rate*$_{INTERIM}$ (%)	216	19.66	14.14	0.168
Market Value	120	592.2	377.2	900.700
	216	517.6	329.1	779.000
Return Variability	120	0.076	0.075	0.016
	216	0.078	0.076	0.017
Δ*Market Benchmark*$_{PercentageOwnership}$	120	−0.21	0.00	0.008
	216	0.07	0.02	0.012
Δ*Market Benchmark*$_{Number\ of\ Investors}$	120	−0.03	0.03	1.466
	216	0.16	0.03	2.352

Panel B: Regression Analysis

	Δ*Percentage Ownership*$_{Q+1}$		Δ*Number of Investors*$_{Q+1}$	
Independent Variables	**Annual Reports (1)**	**Interim and Annual Reports (2)**	**Annual Reports (3)**	**Interim and Annual Reports (4)**
Δ*IFRS Adoption Rate*$_{FYE,Q}$	0.042	—	7.354	—
	(0.002)		(0.002)	
Δ*IFRS Adoption RATE*$_{INTERIM,Q}$	—	0.050	—	12.58
		(0.002)		(0.000)
Log (Market Value$_{Q-3}$*)*	−0.025	−0.010	−6.332	1.345
	(0.309)	(0.154)	(0.222)	(0.579)
Return Variability$_{Q-3}$	0.075	−0.327	9.750	−34.49
	(0.889)	(0.117)	(0.927)	(0.376)
Dependent Variable$_{Q-3}$	−2.040	−1.048	0.070	−0.343
	(0.000)	(0.000)	(0.907)	(0.455)
Δ*Dependent Variable*$_{Q-3}$	2.111	1.326	−0.106	0.056
	(0.000)	(0.000)	(0.808)	(0.866)
Δ*Market Benchmark*$_{Q+1}$	−0.032	0.080	−0.273	0.021
	(0.771)	(0.288)	(0.176)	(0.807)
Country Dummies	Yes	Yes	Yes	Yes
No. Observations	120	216	120	216
Adj. R^2	55.6%	39.2%	15.4%	7.3%

The sample in models 1 and 3 consist of a maximum 120 country-quarter observations from 24 treatment sample countries during a five-quarter period, where mandatory IFRS annual reports become initially available (i.e., from January 2006 to March 2007). The sample in models 2 and 4 consists of a maximum 216 country-quarter observations

(continued on next page)

TABLE 8 (continued)

during a nine-quarter period (i.e., we extend the above window by four quarters, from January 2006 to March 2008, in order to allow for interim reporting effects). We include only firms that report under IFRS for the first time in the year IFRS become mandatory.

Panel A reports descriptive statistics for the dependent and independent variables during the above two periods.

Panel B reports the coefficient estimates of regressions of country-level change in institutional holdings on the change in the country-level IFRS adoption rate.

We aggregate all firm-level variables in a given country and quarter by computing medians. All models are estimated with White standard errors; p-values in parentheses.

Variable Definitions:
Δ*Percentage Ownership* (Δ*Number of Investors*) = quarterly change in the median percentage ownership (number of investors) in a country;
Δ*IFRS Adoption Rate* = quarterly change in the cumulative index of IFRS adoption for a country;
Market Value = stock price times the number of shares outstanding (in U.S. dollars) at the end of quarter Q–3;
Return Variability = standard deviation of the previous 12 monthly stock returns measured at time Q–3;
Market Benchmark = mean dependent value from all observations in the benchmark sample. We include lagged level and change in the dependent variable measured at time Q–3; and
Δ = changes in variables computed over quarter Q–1 to Q.

both country-quarter samples for the aggregate changes in percentage ownership and number of investors, for changes in adoption rates and for control variables.

Inferences from the two adoption rate estimation methodologies are identical. Table 8, Panel B shows that both changes in aggregate percentage ownership and changes in aggregate numbers of investors are positively and significantly related to changes in IFRS adoption rates occurring in the previous quarter. For example, in the case of percentage ownership the coefficient of *IFRS Adoption Rate*$_{FYE}$ is 0.042 and has a p-value of 0.002, and in the case of the number of investors the estimate of *IFRS Adoption Rate*$_{INTERIM}$ is 12.58 and has a p-value of 0.000 (see models 1 and 4, respectively). The results suggest that investors respond in their portfolio investment decisions to IFRS financial statement information as it becomes available.

It might appear from Panel B that *IFRS Adoption Rate*$_{FYE}$ is more powerful at capturing variation in aggregate holdings changes than *IFRS Adoption Rate*$_{INTERIM}$ because the R^2 statistics are higher. However, results in models 1 and 3 are not directly comparable with those in models 2 and 4 because *IFRS Adoption Rate*$_{INTERIM}$ is estimated over four additional quarters in 2005. In untabulated tests we also estimate models 2 and 4 over the shorter five-quarter period with 120 observations. When we do this, R^2 statistics increase to 52.4 percent and 14.7 percent and the coefficients on the change in the adoption rate are 0.0421 and 11.27, respectively. Thus, results for the two adoption rate indices are qualitatively similar.

Overall, these country-quarter results reflect evidence that changes in the information environment associated with adoption of IFRS within countries are associated with increases in institutional investor holdings.

Investor Orientation and Style Analysis

Next we examine whether IFRS effects on institutional holdings are concentrated among the investors most likely to make use of financial statement information in investment decisions. Since results for the different annual intervals are robust to the selection of measurement interval, we report results for tests using the calendar-year intervals.

Table 9 presents results partitioned by investment orientation, where we decompose holdings into active and passive investors; and by investment style where we decompose holdings into growth, value, index, and income investors. Panel A contains results for changes in percentage ownership and Panel B for changes in the number of institutional investors. Consistent with our predictions we find that mandatory IFRS adoption is associated with significant increases in the

TABLE 9

Changes in Institutional Holdings and IFRS Mandatory Adoption Partitioning by Investment Orientation and Investment Style

Panel A: Δ*Percentage Ownership*

Independent Variables	Active (1)	Passive (2)	Growth (3)	Value (4)	Index (5)	Income (6)
First-Time Mandatory	0.023	−0.001	0.010	0.010	0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.311)	(0.419)
Voluntary	0.009	0.000	0.004	0.004	−0.000	−0.000
	(0.001)	(0.788)	(0.003)	(0.008)	(0.984)	(0.345)
*Voluntary * Mandatory*	0.008	−0.002	0.000	0.004	0.000	0.000
	(0.011)	(0.000)	(0.763)	(0.010)	(0.081)	(0.970)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Country Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
No. Observations	21,726	21,726	21,726	21,726	21,726	21,726
No. Countries	45	45	45	45	45	45
Adj. R^2	10.2%	7.7%	7.2%	2.9%	6.0%	3.4%

Panel B: Δ*Number of Investors*

Independent Variables	Active (1)	Passive (2)	Growth (3)	Value (4)	Index (5)	Income (6)
First-Time Mandatory	4.185	0.758	1.978	1.775	0.714	0.023
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.541)
Voluntary	1.909	0.060	0.835	0.395	0.007	0.098
	(0.071)	(0.710)	(0.089)	(0.223)	(0.947)	(0.098)
*Voluntary * Mandatory*	3.238	0.795	1.262	1.668	0.932	0.038
	(0.008)	(0.000)	(0.025)	(0.000)	(0.000)	(0.570)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Country Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
No. Observations	22,826	22,826	22,826	22,826	22,826	22,826
No. Countries	45	45	45	45	45	45
Adj. R^2	17.6%	16.8%	16.7%	13.0%	19.8%	7.0%

The complete sample consists of a maximum 35,160 firm-year observations from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis. The sample is partitioned by investment orientation, i.e., active versus passive investors and by investment style, i.e., growth/value versus index/income investors.

All models are estimated with White standard errors adjusted to account for correlation within firm clusters; p-values in parentheses. Financial variables are winsorized at the 1st and 99th percentiles. For all other control variable definitions see Table 3.

Variable Definitions:

Percentage Ownership = total market value of shares owned by institutions divided by the total market value of the firm at year-end, where both items are measured in U.S. dollars;

Number of Investors = total number of institutional investors of the firm at year-end;

Δ = changes in variables computed over year *t*–1 to *t*;

First-Time Mandatory = observations of firms that never reported under IFRS before mandatory adoption and equals 1 for all firm-years with IFRS reporting periods ending on or after the local mandated IFRS adoption date (i.e., December 31, 2005), and 0 otherwise;

(continued on next page)

TABLE 9 (continued)

Voluntary = observations of firms that voluntarily switched to IFRS reporting before it was mandated and equals 1 for all firm-years with IFRS reporting by voluntary adopters, and 0 otherwise; and
Voluntary $*$ *Mandatory* = 1 for all voluntary adopter firm-years ending on or after the mandated local IFRS adoption date, and 0 otherwise.

percentage ownership of active investors, but not of passive investors. The coefficient on *First-Time Mandatory* for ownership of active investors (0.023) is almost identical to the coefficient obtained for overall ownership in Panel A of Table 6, while the coefficient in the model for passive investor is close to zero. These results suggest that IFRS-related ownership increases are attributable to active investors. The *First-Time Mandatory* results for investor style are also consistent with predictions. They indicate that ownership changes are very similar and significant for value and growth investors, who are predicted to make more use of financial statements in their investment decision processes. In contrast, changes in ownership by index and income investors do not appear to be related to first-time mandatory adoption.

Results for changes in the number of investors in Table 9, Panel B are consistent with our findings for percentage ownership in indicating strong IFRS first-time adoption effects for active investors, and value and growth investors. The main difference between results in Panel B is that the average number of passive investors also increases with mandatory IFRS adoption. However, while the coefficient on *First-Time Mandatory* in the case of passive investors is 0.758, the average first-time adoption increase for active investors is almost six times as high, with a coefficient of 4.185. Similarly, the investor style results show that the average first-time mandatory adoption increase in the number of index investors is less than half the increase for growth and value investors, while the corresponding increase in the average number of income investors is statistically insignificant. Empirical findings are similar for voluntary adopters.

Overall, we document that the positive institutional holdings effects associated with mandatory IFRS adoption are not present across all types of investors. Specifically, we find that the ownership effects are considerably stronger for active and, in particular, value and growth investors, i.e., for investors who depend to a greater extent on financial statement information. These results are consistent with institutional holdings responding to common and higher quality financial reporting when financial statement numbers are important in investment decision making.

Cross-Country Analysis

We now examine whether changes in institutional holdings in response to mandatory IFRS adoption depend on enforcement and financial reporting incentives as well as on differences between local GAAP and IFRS. Our empirical tests condition the estimated IFRS effects on indicator variables representing the country clusters of Leuz et al. (2003) and Leuz (2010) and on the two measures of accounting difference in Ding et al. (2005, 2007), namely *Divergence* and *Absence*.

Panel A in Table 10 describes the institutional variables. Note that four (eight) treatment (benchmark) countries are dropped from this analysis because the institutional data are not available, but the overall number of observations lost is low because they are not large countries. Panel B shows the results of paired t-tests of differences in the means of institutional clusters and accounting divergence measures across the 20 treatment sample countries and the 13 benchmark sample countries, revealing no significant differences in institutional characteristics between the two samples.

TABLE 10

Changes in Institutional Holdings and IFRS Mandatory Adoption Conditioning on Institutional Country Characteristics

Panel A: Institutional Factor Scores

Treatment Countries	*Cluster*	*Absence*	*Divergence*	Benchmark Countries	*Cluster*	*Absence*	*Divergence*
Australia	1	22 (0)	21 (0)	Argentina	3	47 (1)	33 (1)
Austria	2	34 (1)	36 (1)	Bermuda	NA	NA	NA
Belgium	2	22 (0)	32 (1)	Brazil	3	36 (1)	23 (0)
Czech Republic	NA	44 (1)	20 (0)	Canada	1	4 (0)	25 (0)
Denmark	2	31 (1)	21 (0)	Chile	2	31 (1)	28 (1)
Finland	2	22 (0)	31 (1)	China	NA	23 (1)	19 (0)
France	2	21 (0)	34 (1)	Colombia	3	NA	NA
Germany	2	18 (0)	38 (1)	Egypt	NA	22 (0)	23 (0)
Greece	3	40 (1)	28 (1)	India	3	18 (0)	19 (0)
Hong Kong	1	14 (0)	15 (0)	Indonesia	NA	12 (0)	12 (0)
Hungary	NA	40 (1)	26 (0)	Israel	1	15 (0)	18 (0)
Ireland	1	0 (0)	34 (1)	Japan	2	18 (0)	22 (0)
Italy	3	27 (1)	37 (1)	Korea (South)	2	NA	NA
Luxemburg	NA	54 (1)	17 (0)	Malaysia	1	30 (1)	13 (0)
The Netherlands	2	10 (0)	25 (0)	Mexico	3	0 (0)	18 (0)
Norway	2	7 (0)	17 (0)	New Zealand	1	23 (1)	20 (0)
Philippines	3	24 (1)	14 (0)	Russia	NA	38 (1)	29 (1)
Poland	NA	23 (1)	30 (1)	Taiwan	3	19 (0)	23 (0)
Portugal	3	29 (1)	22 (0)	Thailand	3	29 (1)	7 (0)
South Africa	1	7 (0)	1 (0)	Turkey	NA	47 (1)	24 (0)
Spain	2	28 (1)	29 (1)	United States	1	6 (0)	23 (0)

(continued on next page)

TABLE 10 (continued)

Treatment Countries	*Cluster*	*Absence*	*Divergence*	**Benchmark Countries**	*Cluster*	*Absence*	*Divergence*
Sweden	2	10	26				
		(0)	(0)				
Switzerland	2	42	22				
		(1)	(0)				
United Kingdom	1	0	35				
		(0)	(1)				
Average	1.95	23.71	25.46	Average	2.13	23.22	21.06

Panel B: Differences between Benchmark and Treatment Sample

	Cluster	*Absence*	*Divergence*
Benchmark (i)	2.08	21.23	20.92
	(n = 13)	(n = 13)	(n = 13)
Treatment (ii)	1.95	20.40	25.90
	(n = 20)	(n = 20)	(n = 20)
(ii) − (i)	−0.13	−0.83	4.98
	(0.659)	(0.855)	(0.107)

Panel C: Cross-Country Regression Analysis

Independent Variables	*ΔPercentage Ownership* (1)	*ΔNumber of Investors* (2)
First-Time Mandatory	−0.044	−9.348
	(0.000)	(0.007)
First-Time Mandatory ∗ Cluster 1	0.064	12.85
	(0.000)	(0.000)
First-Time Mandatory ∗ Cluster 2	0.020	4.101
	(0.003)	(0.146)
First-Time Mandatory ∗ Absence	0.008	4.829
	(0.206)	(0.044)
First-Time Mandatory ∗ Divergence	0.035	7.987
	(0.000)	(0.000)
Voluntary	−0.016	−7.208
	(0.413)	(0.315)
Voluntary ∗ Cluster 1	0.028	10.55
	(0.186)	(0.147)
Voluntary ∗ Cluster 2	−0.006	0.727
	(0.602)	(0.880)
Voluntary ∗ Absence	0.000	2.107
	(0.989)	(0.635)
Voluntary ∗ Divergence	0.023	5.946
	(0.022)	(0.114)
Voluntary ∗ Mandatory ∗ Cluster 1	0.041	−5.698
	(0.043)	(0.480)

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TABLE 10 (continued)

Independent Variables	*ΔPercentage Ownership* (1)	*ΔNumber of Investors* (2)
*Voluntary * Mandatory * Cluster 2*	0.026	3.085
	(0.002)	(0.432)
*Voluntary * Mandatory * Absence*	0.007	−5.551
	(0.414)	(0.188)
*Voluntary * Mandatory * Divergence*	0.009	−10.88
	(0.352)	(0.016)
Cluster 1	0.006	1.389
	(0.624)	(0.551)
Cluster 2	0.002	2.220
	(0.815)	(0.204)
Absence	−0.009	−3.366
	(0.211)	(0.124)
Divergence	0.004	−3.634
	(0.553)	(0.124)
Control Variables	Yes	Yes
Country Dummies	Yes	Yes
Industry-Year Dummies	Yes	Yes
No. Observations	20,924	22,020
No. Countries	33	33
R^2	13.1%	18.7%

The complete sample consists of a maximum 35,160 firm-year observations from 45 countries during the period 2003–2006 with sufficient institutional holdings data from TFO and financial data from Worldscope to perform the regression analysis.

Panel A reports raw values of the institutional factor scores for the treatment and benchmark sample countries, respectively. The treatment sample consists of countries that mandated IFRS reporting and the benchmark sample includes countries that do not allow or do not require IFRS reporting. Cluster classification is based on legal regulation quality, enforcement, and reporting practices variables and is taken from Leuz (2010), characterized as outsider economies with strong outsider protection and legal enforcement (*Cluster 1*); insider economies with stronger rule enforcement (*Cluster 2*); and insider economies with weaker rule enforcement (*Cluster 3*). *Absence* and *Divergence* are drawn from Ding et al. (2007); higher values represent more missing rules/disclosures from local GAAP and more differences between IFRS and local GAAP, respectively. We transform continuous values of *Divergence* and *Absence* into binary variables (in parentheses) splitting by the median of treatment sample countries; coded 1 = above median, and 0 otherwise.

Panel B shows differences in mean institutional factor scores between benchmark and treatment countries (only for countries with all available scores); p-values in parentheses for (ii) – (i).

Panel C reports coefficient estimates of the cross-country regression analysis; p-values in parentheses.

All models are estimated with White standard errors adjusted to account for correlation within firm clusters. Financial variables are winsorized at the 1st and 99th percentiles. For all other control variable definitions see Table 3.

Variable Definitions:

Percentage Ownership = total market value of shares owned by institutions divided by the total market value of the firm at year-end, where both items are measured in U.S. dollars;

Number of Investors = total number of institutional investors of the firm at year-end;

Δ = changes in variables computed over year $t-1$ to t;

First-Time Mandatory = observations of firms that never reported under IFRS before mandatory adoption and equals 1 for all firm-years with IFRS reporting periods ending on or after the local mandated IFRS adoption date (i.e., December 31, 2005), and 0 otherwise;

Voluntary = observations of firms that voluntarily switched to IFRS reporting before it was mandated and equals 1 for all firm-years with IFRS reporting by voluntary adopters, and 0 otherwise; and

*Voluntary * Mandatory* = 1 for all voluntary adopter firm-years ending on or after the mandated local IFRS adoption date, and 0 otherwise.

Empirical findings in Panel C of Table 10 are very consistent across the two institutional holdings change proxies.[16] They reveal large differences in the marginal IFRS effects across country clusters and between countries with high accounting divergence. The *First-Time Mandatory* main effect coefficient is significantly negative for both changes in ownership (–0.044) and changes in the number of investors (–9.348), indicating that *Cluster 3* first-time adoption firms with small accounting differences experience a relative decline in institutional holdings. The incremental coefficients of first-time mandatory adoption are positive for both *Cluster 1* and *Cluster 2* countries. The marginal first-time mandatory effects of accounting differences, as measured by *Divergence*, on institutional holdings are also significantly positive, suggesting that IFRS-induced changes in financial statement numbers are larger for firms in countries where accounting divergence is greater. In contrast, *Absence* is insignificant when interacted with *First-Time Mandatory*, in line with arguments that the absence of rules might imply that some accounting issues are not important in some jurisdictions. While we do not have predictions for the conditioning effects of institutional variables on *Voluntary* $*$ *Mandatory*, results also reveal differences in effects across clusters for changes in ownership percentage, but not in the number of investors. *Absence* and *Divergence* are less relevant for these firms, probably because they already report under IFRS.[17]

To sum up, results suggest that the impact of the mandatory introduction of IFRS on the investment decisions of institutions varies significantly across countries. In particular, in line with our predictions and consistent with capital market outcomes documented in prior studies (Daske et al. 2008; Li 2010), results indicate that the positive effects of mandatory IFRS adoption on institutional holdings are found primarily in countries with strong institutions.[18] Moreover, where there is high divergence between domestic GAAP and IFRS, the impact of mandatory IFRS introduction on institutional holdings is even higher.

Additional Analysis

We expand our empirical analysis by performing two additional tests. First, we repeat the analysis in Table 6 separately for emerging and developed market countries. This analysis can provide further reassurance that earlier results are not driven by flows of funds into developed markets due to unmodeled factors, e.g., other institutional features that affect market microstructure across sample countries.[19] Second, current research indicates that other recent EU market regulations affect market liquidity and the cost of capital (Christensen et al. 2011). To address concerns that positive institutional holdings effects are driven by other EU regulatory changes we estimate all models in Table 6 after excluding EU members. Untabulated results indicate that when institutional holdings change proxies are measured over calendar-year intervals, mandatory financial reporting under IFRS is associated with increased ownership only for developed countries. However, when we employ alternative annual measurement windows as in Panel B of Table 6, we find evidence of positive and significant mandatory IFRS effects for both emerging and developed countries. Similarly, we document positive and significant IFRS effects on institutional ownership for the −1/+3 and −2/+2 intervals but not for the calendar-year interval when focusing on non-EU countries.[20]

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[16] We also include main effects for the institutional variables to allow for the possibility that institutional holdings changes depend directly on the country context in which investment takes place. The *First-Time Mandatory*, *Voluntary*, and *Voluntary* $*$ *Mandatory* main effects can be interpreted as the IFRS effects applicable to countries belonging to *Cluster 3* when accounting *Divergence* and *Absence* are low.

[17] We note that the coefficient on *Voluntary* $*$ *Mandatory* $*$ *Divergence* is negative in the case of changes in the number of investors. As discussed in the context of Table 7 results, the dynamics of holdings changes for voluntary adopters appear to be complex.

[18] Covrig et al. (2007) find that institutional ownership increases on voluntary IFRS adoption for firms with low-quality information environments. If *Clusters 1* and *2* are regarded as conducive to higher quality information environments, then our results on institutional effects might appear to contradict Covrig et al. (2007). However, analysis reveals that the institutional variables underlying the clusters are not significantly correlated with the firm-level proxy for information environment quality employed by Covrig et al. (2007), i.e., *Number of Analysts* $*$ *Size*. We interpret this as indicating that the institutional clusters capture different characteristics of firms' environments. When we repeat the Covrig et al. (2007) analysis using their firm-level proxy, we confirm their result.

[19] To identify emerging markets we use the *Standard & Poor's and International Finance Corporation of World Bank* (S&P/IFC) emerging market index (Li and Hoyer-Ellefsen 2008). Accordingly, our sample includes 20 emerging and 21 developed countries; four countries (Hong Kong, Luxemburg, Bermuda, and Taiwan) are not classified.

Sensitivity Tests

To test the robustness of the baseline empirical findings reported in Table 6 we perform a number of sensitivity tests. First, we employ alternative samples, as follows: (1) we limit the sample to IFRS adoption countries only; (2) we define a benchmark sample to include a maximum of 150 randomly selected firms per country; (3) we exclude U.S. firms from the benchmark sample; (4) we limit the benchmark sample to U.S. firms; (5) we hold sample composition constant over time; (6) we exclude from the treatment sample firms that did not switch to IFRS in the sample period; and (7) we limit the treatment sample to EU countries. We also include the following additional control variables: (1) the total number of shares outstanding at year-end (WS05301) to proxy for new equity issuance (Bradshaw et al. 2004); (2) the ADR indicator in levels rather than in changes, given that the latter variable is dropped from the regressions due to lack of variability; and (3) the percentage of closely held shares (WS08021) to capture the effect of ownership structure and free float (Leuz et al. 2010).

We then examine the possibility that the changes in institutional holdings we observe are driven by the IFRS-related changes in liquidity documented by Daske et al. (2008). We test whether liquidity changes account for changes in investor holdings by introducing exogenous controls for liquidity changes, defining liquidity as the Amihud (2002) illiquidity measure, the bid-ask spread, and the frequency of zero returns, and by modeling the liquidity controls as endogenously determined, using market value, share turnover, return variability, a binary indicator controlling for U.S. GAAP reporting, and a market benchmark as first-stage instruments for liquidity.

To provide reassurance that our results are not driven by growth in the number of institutional investors and their holdings over time, we also restrict the analysis to holdings by institutional investors for whom we observe data in both the pre- and post-IFRS periods.

Since some of our control variables depend on accounting measurement rules, their use in the pre- and post-IFRS periods could introduce noise or bias into the analysis. Noise will reduce the likelihood of finding statistically significant results, but bias threatens the conclusions drawn from the analysis if any bias is correlated with the IFRS indicators and institutional investment in the pre-mandatory adoption period. To help address this concern, we interact the three IFRS indicators with all accounting-based control variables (i.e., *ROE*, *Leverage*, *Dividend Yield*, *Book-Market*, and *Earnings-Price*).

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[20] The sensitivity of annual window results is likely driven by heterogeneity in the timing of the annual financial reporting cycles across sample firms. For example, in the case of developed countries, the percentage of the treatment sample firm-years having a December fiscal year-end is considerably higher than for the benchmark sample (i.e., 68 percent compared to 46 percent). However, this relation is reversed in the case of emerging market countries where the proportion of treatment (benchmark) sample firm-years having December fiscal year-ends is 61 percent (80 percent).

Finally, we repeat our cross-country analysis after incorporating *Divergence* in continuous form. This helps mitigate concerns regarding the clustering of this dummy variable where a number of countries with only slightly different divergence scores are coded differently.[21]

When we implement each of these sensitivity tests we find that all the main empirical findings and inferences in Table 6 are qualitatively identical to those reported in the paper.

V. CONCLUSIONS

Based on annual and quarterly firm-level analyses and quarterly country-level tests we report evidence of economically significant increases in institutional holdings in mandatory IFRS adopters. After first-time IFRS reporting, institutional ownership and the number of investors increase for mandatory IFRS adopters relative to a benchmark sample of non-adopters. We also show that increases in institutional holdings are concentrated among investors whose investment orientation and styles rely more heavily on financial statement information. This raises confidence that the holdings effects we observe are driven by the financial reporting regime change, and not by changes in unmodeled factors that might also determine institutional investment. In further analysis we document that the positive impact of mandatory IFRS adoption on institutional holdings is restricted to countries where enforcement and reporting incentives are strong and where divergence between local accounting standards and IFRS is relatively high.

Our study is subject to several caveats that might suggest avenues for future research. First, while our research is designed to address the identification problem, we cannot rule out entirely the possibility that other concurrent regulatory or economic changes affect IFRS adoption countries, but they would have to be related to the financial reporting cycle and affect only certain investor types within those countries. Second, our findings suggest that voluntary adopters also experience an increase in institutional holdings following the mandating of IFRS. Moreover, there are potentially interesting differences in patterns of holdings changes for first-time mandatory and voluntary adopters in relation to both the financial reporting cycle and the role of accounting differences. It is possible that our results for voluntary adopters reflect comparability externalities after mandatory IFRS adoption by other peers. However, our evidence does not shed direct light on the validity of this conjecture. Future work could address the challenge of identifying comparability effects in international settings. Finally, we do not investigate the supply side of the increase in institutional holdings in first-time IFRS adopters. Further work might investigate the extent to which institutional ownership changes are associated with reductions in holdings of investors who are not captured by our data, e.g., insiders and family block-holders, and with secondary equity offerings and capital restructuring programs that increase the supply of equity.

Overall, our study helps shed new light on the channels underpinning the capital market effects of IFRS adoption reported in Daske et al. (2008) and Li (2010). The evidence supports the argument that IFRS adoption is beneficial for institutional investors and issuers of securities.

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An Analysis of Multiple Consecutive Years of Material Weaknesses in Internal Control

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ABSTRACT: The primary objective of the current study is to empirically reexamine the relation between material weaknesses in internal control (MW) and cost of equity (CE). We direct particular emphasis to the way non-remediation, as well as remediation, of MW affects a firm's CE. This study utilizes a dataset that contains a large sample of second-year MW non-remediation cases, as well as third-, fourth-, and fifth-year non-remediation cases. The findings provide evidence that reporting MW, absent any remediation, in multiple consecutive years has a significant negative impact on CE. However, the current study also shows that the market views favorably a reduction in the number of MW (i.e., partial remediation). Our study helps to reconcile conflicting results in the literature devoted to the relation between MW and CE.

Keywords: *internal control; monitoring; remediation; material weaknesses; cost of equity.*

JEL Classifications: *M41; M42.*

Data Availability: *Available from sources identified within the article.*

I. INTRODUCTION

The corporate scandals in the United States around the turn of the 21st century (e.g., Enron, WorldCom, Tyco, etc.) culminated in the passage of the Sarbanes-Oxley Act of 2002 (SOX). This Act requires firms to report material weaknesses in internal control (hereafter, MW) related to the reliability of financial reporting to the Securities and Exchange Commission (SEC). The SOX reporting requirements have given rise to a plethora of research, much of which

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has focused on the impact of MW on a firm's cost of equity (hereafter, CE).[1] For example, Ashbaugh-Skaife et al. (2009; hereafter, ACKL) find a significant positive relation between MW (reported under SOX Sections 302 and 404) and CE. In contrast, Ogneva et al. (2007; hereafter OSR) find no significant relation between MW (reported under SOX Section 404) and CE. The above research recognizes the potential impact of remediation of MW on CE, but the time periods covered in previous studies encompass, at most, the initial two consecutive years of regulated reporting. Furthermore, previous studies have not considered the impact of the number of reported MW on the relation between CE and remediation.

The primary objective of the current study is to reexamine the relation between MW and CE. We direct particular emphasis to examining the way non-remediation of MW in multiple consecutive years affects CE, as well as the impact of remediation of MW on CE. We utilize a dataset that contains a large sample of second-year MW non-remediation cases, as well as third-, fourth-, and fifth-year non-remediation cases. Thus, this study differentiates between firms that report MW in only one year and firms that report MW in two or more consecutive years. The current study also considers the number of MW in each year, as well as the specific types of MW. As such, this study extends earlier studies that consider the potential impact of MW and remediation (or lack thereof) on a firm's CE.

Our findings offer three important contributions to the existing literature. First, they indicate that MW reported in multiple consecutive years have a progressively larger and statistically significant negative impact on CE when firms that partially remediate are excluded from the sample. That is, the market notices that some firms are slow to remediate MW, whereas other firms take timely remediation steps to address MW. Second, our study shows that the number and specific types of MW are significant factors in understanding the relation between MW and CE. In fact, even if a firm does not remediate all MW in a given year, the market views favorably a reduction in the number of MW (i.e., partial remediation). Third, given the richness of the dataset, the current study helps to reconcile conflicting results in the prior literature on the effects of MW.

II. BACKGROUND AND HYPOTHESES

Background

The value of SOX Section 404 has been debated within private and public circles since its passage (e.g., see Ernst & Young 2005; U.S. House of Representatives 2005). Most recently, the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 ordered an additional investigation into the costs versus the benefits of Section 404 (U.S. House of Representatives 2010). Recent internal control research has used evidence from SOX Sections 302 and 404 to examine the impact of MW on CE. For example, OSR do not find evidence that a higher CE is associated with reporting SOX Section 404 MW, after controlling for firm characteristics and predicted analyst forecast error. Consistent with the results posited by OSR, Beneish et al. (2008) also find no significant relation between SOX Section 404 MW and CE. However, Beneish et al. (2008) do find a significant and positive relation between SOX Section 302 MW and CE. In contrast to the research noted above, ACKL find a significant and positive relation between SOX Sections 302 and 404 MW and CE. ACKL note that their model's risk integration, along with sample-selection procedures, differentiates their model and results from the model used and the results obtained by OSR.

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[1] Although not the focus of this study, researchers have also examined the impact of reporting MW on stock price (e.g., see Gupta and Nayar 2007; Beneish et al. 2008; Hammersley et al. 2008; Kim and Park 2009).

Key distinctions exist between Sections 302 and 404 of SOX. That is, Section 302 is a quarterly internal control disclosure assessment under which managers are required to recognize and report MW. Section 404 is an annual internal control assessment that requires management to report MW. Additionally, Section 404 requires accelerated filers to obtain an audit opinion attesting to the results of management's internal control system assessment.[2] Given the points cited above, the current study focuses upon disclosures of MW related to SOX Section 404. However, in reconciling conflicts in the prior literature, we also consider SOX Section 302 MW.

Previous studies that examine the relation between MW and CE cover time periods extending no longer than two years. Thus, the previously cited studies use only a two-year sample period when considering remediation of MW. Nevertheless, prior research considers the remediation of MW, if that remediation occurs within the subsequent reporting period. For example, OSR account for remediation of Section 404 MW if the remediation occurs within the time period of November 2004 through January 2006. OSR (2007, 1272) find "little evidence that firms that remediate their internal control weaknesses have lower cost of equity." Beneish et al. (2008) also account for remediation of MW and do not find that such remediation significantly benefits firms through a lower CE. In contrast, ACKL (2009, 5) find "clear evidence ... that firms reporting effective internal controls or firms remediating previously disclosed ICDs [internal control deficiencies] benefit through lower cost of equity." The above points notwithstanding, remediation of MW was not the focus in earlier studies, nor could it have been given the time periods covered in these studies.

The current study focuses upon remediation of MW using an expanded dataset that considers MW in multiple consecutive years, reported under SOX Section 404, spanning a six-year time period (i.e., 2004–2009). We focus on remediation because taking timely remediation actions, with respect to MW, is an essential part (or at least should be an essential part) of a firm's internal control system.

Material Weaknesses and Cost of Equity

A firm that reports SOX Section 404 MW sends an explicit signal to market participants that the reliability of its accounting disclosures is less than desirable. Intuitively, this signal likely leads investors to apply a higher risk premium to their discounting of the firm's expected cash flows. That is, investors will likely demand a higher expected return from a firm reporting MW than they would from a similar firm that does not report MW. Demand for a higher expected return essentially equates to a higher CE.

Lambert et al. (2007) provide theoretical justification for the above intuition, outlining a theoretical framework that shows that the quality of accounting information disclosures affects CE via its impact on a firm's risk. This impact is the result of both direct and indirect effects. The direct effect on CE is the result of the market assessment of the covariance of the firm's cash flows with the cash flows of other firms in the market. The indirect effect on CE is the result of the effect of accounting information disclosure quality on a firm's real economic decisions, which, in turn, has an impact on the firm's expected value and the covariance of the firm's cash flows with cash flows of other firms in the market.

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[2] An accelerated filer is defined by the SEC as "(1) the issuer had an aggregate market value of voting and non-voting common equity held by non-affiliates of the issuer of $75 million or more, as of the last business day of the issuer's most recently completed second fiscal quarter; (2) the issuer had been subject to the reporting requirements of Section 13(a) or 15(d) of the Exchange Act [15 U.S.C. 78m(a) or 78o(d)] for a period of at least 12 calendar months; (3) the issuer previously had filed at least one annual report; and (4) the issuer was not eligible to use Forms 10-KSB and 10-QSB [17 CFR 249.310b and 17 CFR 249.308b] for its annual and quarterly reports" (SEC 2005).

In general, Lambert et al. (2007) show that higher (lower) quality accounting information disclosures can lower (raise) CE. In discussing the direct effect, they note "that higher quality accounting information and financial disclosures . . . unambiguously [move] a firm's cost of capital closer to the risk-free rate. Moreover, this effect is not diversifiable" (Lambert et al. 2007, 387). Although the indirect effects are more ambiguous, they too could clearly lead to a negative relation between the quality of accounting information disclosures and CE.

Building on the theoretical work of Lambert et al. (2007) and Easley and O'Hara (2004), as well as the empirical work of Francis et al. (2004, 2005), OSR argue that firms with MW are likely to have a higher CE due to higher information risk associated with these firms. Additionally, OSR (2007, 1256) determine that these firms are likely to be impacted by the fact that MW reflect poor management controls "which could increase business risk and the cost of equity." Building largely on the Lambert et al. (2007) framework, ACKL (2009, 7) argue that "the quality of a firm's internal control over financial reporting affects investors' assessment of firm risk and cost of capital because if a firm has weak internal control, then the quality or precision of its accounting signals is impaired." ACKL (2009, 7) then go on to predict that firms with MW "exhibit higher idiosyncratic risk, systematic (beta) risk, and higher cost of equity relative to firms with strong internal control."

In sum, there is reason to expect that MW negatively affect CE. Furthermore, it seems logical to assume that, as the number of multiple consecutive periods in which MW are disclosed increases, the severity of the negative effect increases. Of course, whether a negative relation between MW and CE actually exists is an empirical issue. As noted above, the empirical evidence to date is mixed.

Hypotheses

Based on the above discussions, considering MW and the remediation of MW over several years should provide a greater understanding of the impact of MW on CE than considering a single year of MW reporting. Whereas some firms are quick to take timely corrective actions in response to MW, other firms are slow to remediate and have reported MW in two, three, or more consecutive years. Thus, while firms that report MW in a single year may experience a negative impact on their CE, reporting MW in multiple consecutive years acts as a clear signal to the market that the firms in question are not effectively remediating their MW. This reasoning leads to the following two general hypotheses (stated in the alternative form):

H1: Disclosing MW within a particular year will have a negative effect on a firm's CE (i.e., higher CE).

H2: Disclosing MW within multiple consecutive years will have a larger negative effect on the CE for those firms making such disclosures than for firms that do not report MW in multiple consecutive years.

As a corollary to H2, we also test H3 below (stated in the alternative form):

H3: The greater the number of multiple consecutive years in which MW are disclosed, the larger the negative effect on the CE for those firms making such disclosures.

H2 and H3 imply that firms either fully remediate or do not remediate all of their MW from one year to the next. However, it is possible that firms reporting MW in multiple consecutive years may have remediated a portion (as opposed to all) of their MW. In this latter case, one would expect to empirically observe a reduction in CE as a result of these firms' partial remediation of their MW. This assumption leads to a fourth hypothesis (stated in the alternative form):

H4: Partial remediation of MW, within a particular year, will have a positive effect on a firm's CE (i.e., lower CE).

As will be discussed further in Section IV, to test H2 and H3 independent of H4 in the multivariate analyses, we first analyze the data in terms of firms that do not partially remediate their MW from one year to the next.[3] This analysis is followed by an analysis that includes all firms (i.e., including firms that do partially remediate their MW).

III. DESIGN OF STUDY

Sample

We draw our sample from the Audit Analytics database, which includes all SOX Section 404 auditor assessment reports (24,806) filed with the SEC during the time period of November 2004 (the effective date for accelerated filers reporting under Section 404) through December 2009. Table 1, Panel A, summarizes the initial sample composition, which includes 2,219 observations with MW and 22,587 observations without MW.

We apply four screens to arrive at the final sample. First, we eliminate duplicate observations to ensure that each firm has at most only one observation included for any given year (i.e., in cases of restatements we take the last filing). Second, we eliminate observations associated with firms that report MW in multiple non-consecutive years because such an approach allows us to distinguish firms that report MW in multiple consecutive years from firms that report MW in a single year (i.e., firms reporting MW in multiple non-consecutive years fall somewhere between these two extremes).[4] Third, similar to OSR, we eliminate observations associated with firms that report MW in their Section 302 reports and subsequently do not report MW in their Section 404 reports. Finally, we eliminate observations missing the data required to calculate the control variables or the data required to calculate CE. The final sample is composed of 16,946 observations, which includes 1,140 observations with MW and 15,806 observations without MW (the control sample).[5] However, the number of observations used within each analysis varies depending on the CE measure.

Dependent Variable

As discussed in Section II, an increased CE is a potential result of MW. Thus, the dependent variable in the current study is *cost of equity* (*CE*). For comparability with previous studies (i.e., OSR and ACKL), we calculate *CE* using five established methodologies that are discussed and developed by Claus and Thomas (2001), Gebhardt et al. (2001), Gode and Mohanram (2003), Easton (2004), and Hail and Leuz (2006). We refer to these measures as CE_{CT}, CE_{GLS}, CE_{GM}, CE_{EAST}, and CE_{HL}, respectively. We use multiple measures of CE to avoid spurious results that

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[3] Partial remediation is determined based on the number of MW reported. If the number of MW reported decreases as the firm continues to report MW in multiple consecutive years, then the firm is classified as a firm that partially remediates its MW.

[4] In response to a suggestion by one of the reviewers, all analyses are also performed with multiple non-consecutive MW firms included. The results do not differ significantly from those where the firms are eliminated. These results are available upon request.

[5] Of the 1,140 observations comprising the final sample, 805, or 70.6 percent, occur prior to 2007. In 2007, the SEC, and subsequently the PCAOB, redefined MW, moving from the definition of "a significant deficiency, or combination of significant deficiencies, that results in *more than a remote likelihood* that a material misstatement of the annual or interim financial statements will not be prevented or detected" (PCAOB 2004; emphasis added) to the definition, "a deficiency, or a combination of deficiencies, in internal control over financial reporting, such that there is a *reasonable possibility* that a material misstatement of the company's annual or interim financial statements will not be prevented or detected on a timely basis" (PCAOB 2007; emphasis in the original). Since the current study spans periods of time in which both definitions were used, we note that the change in definition may be one reason behind the decline in firms that report MW after the change in definitions took effect.

TABLE 1

Sample Selection and Categorization of Firms with Material Weaknesses (MW)

Panel A: Sample Selection[a]

MW Sample (Observations with MW)	
Total number of audit reports with MW for fiscal years 2004–2009	2,219
Eliminate duplicate observations due to financial restatements	(103)
Eliminate observations arising from reporting MW in non-consecutive years	(197)
Eliminate observations due to data restrictions imposed by the current study	(779)
Total number of material weakness observations identified for use in the current study	1,140
Control Sample (Observations without MW)	
Total number of audit reports without MW for fiscal years 2004–2009	22,587
Eliminate duplicate observations due to financial restatements	(317)
Eliminate observations associated with firms reporting MW in SOX Section 302 reports and subsequently reporting clean SOX Section 404 reports	(258)
Eliminate observations arising from reporting MW in non-consecutive years	(234)
Eliminate firm years due to data restrictions imposed by the current study	(5,972)
Total number of control observations identified for use in the current study	15,806
Total Sample	16,946

Panel B: Categorization of MW (Observations Associated with Firms Reporting Weaknesses)[b]

	MW Obs.	% of MW Obs.	% of Total Obs.
Single year	828	72.63%	4.89%
Two consecutive years	212	18.60%	1.25%
Three consecutive years	71	6.23%	0.42%
Four or more consecutive years[c]	29	2.54%	0.17%
Total Material Weakness Observations	1,140	100.00%	6.73%

[a] Panel A defines the steps employed to arrive at the final MW and control samples for firms that filed Section 404 reports during fiscal years 2004 through 2009.

[b] Panel B categorizes firms based on the number of consecutive years in which a firm reports MW. For example, if a firm reported MW in the previous two years (observations) and the current year, then the reported MW for the firm in the current year would classify the observation as a three-consecutive-years observation. This firm's observation from the previous year would be classified as two-consecutive-years observation and so on.

[c] As discussed in the text, a limited number of firms had MW in five and six consecutive years. Given the limited number of observations available in the final sample, MW beyond four years were not considered as separate categories.

could arise if any *CE* measure attributes are related to the variables of interest. Although we use all five measures to perform the analyses, only the average measure (i.e., the CE_{HL} measure) is reported in the tabulated results because of the high level of correlation among the measures and because we obtain similar results with each of the five measures.[6] The details of the five estimates of CE are described in Appendix A.

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[6] The results of analyses using all five measures are available upon request.

Following previous studies (i.e., OSR and ACKL), we calculate the measures of *CE* as of the end of June for each year of data availability (2004–2009). Each CE measure has specific data requirements. For example, each observation must have one- and two-year-ahead EPS forecasts (*FEPS1* and *FEPS2*) and either a long-term growth forecast (*LG*) or three-, four-, and five-year-ahead earnings forecasts (*FEPS3*, *FEPS4*, and *FEPS5*). Additionally CE_{EAST} requires $FEPS1 > 0$ and $FEPS2 \geq FEPS1$. While CE_{GM} has the same restrictions as CE_{EAST}, it additionally requires $FEPS1 \geq 0$ *and* $FEPS2 \geq 0$. Finally, CE_{CT} and CT_{GLS} require us to use only observations that have positive book values of equity.

Independent Variables

H2 and H3 predict that firms that report MW in multiple consecutive years will experience a larger negative impact on CE than will firms that report MW in only a single year. Accordingly, the key independent variables in the current study are the number of consecutive years (i.e., one, two, three, and four or more) in which firms report MW. The independent variables are specified as four indicator variables. The first indicator variable, *MW1*, is set equal to 1 for observations of MW associated with firms that have not reported MW previously (0 otherwise). The indicator variables for observations that represent MW reported in two, three, and four or more consecutive years are *MW2*, *MW3*, and *MW4*, respectively. More specifically, we assign the value 1 to one, and only one, of these variables for each observation of MW dependent on whether the MW reported within the observation represent consecutive MW occurring in two, three, or more years.[7]

For example, if a firm reported MW in the previous two years and the current year, then the MW reported in the current year would classify the observation as having MW in three consecutive years (*MW3*). Thus, the firm would be assigned a 1 to the *MW3* indicator variable and 0 to the *MW1*, *MW2*, and *MW4* indicator variables in the current year's observation. This firm's prior year observation would be assigned a 1 to the *MW2* indicator variable and 0 to the *MW1*, *MW3*, and *MW4* indicator variables, and so on. Within our control sample (i.e., firms reporting zero MW), each observation without MW is assigned a value of 0 to all indicator variables (i.e., *MW1*, *MW2*, *MW3*, and *MW4*). Although the inclusion of observations through fiscal year 2009 allows for the possibility of *MW5* and *MW6* variables, due to the limited number of observations that fall within these categories (six observations, after data restrictions have been applied), we include these observations in the *MW4* variable category in all analyses.

As shown in Table 1, Panel B, the final MW sample is composed of 828 *MW1* observations, 212 *MW2* observations, 71 *MW3* observations, and 29 *MW4* observations. As noted in the table, of the 1,140 observations in the sample with MW, only 1.3, 0.4, and 0.2 percent of the total observations report MW in two, three, and four or more consecutive years, respectively. These percentages are in contrast to the 4.9 percent of the total observations associated with reporting MW for the first time. In other words, firms seem to recognize that investors expect remediation of MW, and most firms in the current study respond accordingly. However, of the 4.9 percent of firms that report MW within a single year, 26.5 percent (i.e., 1.3 percent divided by 4.9 percent) of these firms continue to report MW in a second year and 8.2 and 4.1 percent continue to report MW in three and four or more years, respectively. Arguably, 26.5, 8.2, and 4.1 percent represent a significant number of firms that do not fully remediate MW in a timely manner.

Based on prior research, we include six control variables. The first control variable is *firm size*, measured as the logarithm of each firm's market capitalization (*MKT_CAP*). As firm size increases,

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[7] Since 2004 was the first year that firms were required to report in compliance with Section 404, the only possible observations of MW during this period would be *MW1*. Likewise, since 2005 was the second year that firms were required to report in compliance with Section 404, the only possible observations of MW during 2005 would be *MW1* and *MW2*, and so on.

CE will likely decrease, as the firm capitalizes on efficiencies associated with size (OSR). The second control variable is *firm age*, measured as the logarithm of the number of years the firm has existed within the CRSP database (*AGE_FIRM*). As firm age increases, the firm is likely to be in a better position to generate financial support through internal or external methods, resulting in a lower CE (OSR).

A firm's *financial stability* is the third control variable. To proxy for stability, we compute the sum of earnings before extraordinary items for the past two years (Doyle et al. 2007). If the sum is less than 0, then the firm is assigned an indicator variable (*LOSS*) of 1, and 0 otherwise. This variable is included to account for the expectation that firms experiencing losses over the past two years will likely have a higher CE than firms that portray a more positive financial track record (OSR). *Firm complexity* is the fourth control variable. As a firm becomes more complex, we expect complexity to affect the CE (OSR). To measure firm complexity, an indicator variable is set equal to 1 if the firm reported a foreign currency translation adjustment (*FCR*), and 0 otherwise. The fifth control variable is associated with the existence of fluctuations in a firm's sales activities. This variable, *firm fluctuation*, exhibits itself through extreme sales growth (*SL_GR*). To measure extreme sales growth, we calculate year-by-year sales growth for years *t*–2 through *t* (where *t* is the current year) and rank each observation by decile, dependent on its respective average sales growth over the previous three years (ACKL). This variable captures the possibility that a rapidly changing environment could lead to a higher CE due to risks associated with fluctuating sales growth.

The sixth control variable is *predicted analyst forecast error*. Prior CE studies point to optimistic bias in long-term analyst forecasts as a potential source of error in the measurement of CE (Claus and Thomas 2001; Gode and Mohanram 2010). Gode and Mohanram (2010) find that controlling for analyst forecast error improves the relation between CE and realized returns. Whereas OSR find initial evidence that suggests a difference in CE exists between firms reporting MW and firms not reporting MW, this evidence dissipates after they control for firm characteristics and predicted analyst forecast error. Given that the current study already controls for firm characteristics similar to those employed in OSR, we include a predicted analyst forecast error control variable to account for potential analyst forecast bias. To calculate predicted analyst forecast error (*AVGPRED_ER*), we utilize the measurement developed by Hughes et al. (2008), as detailed in Appendix A.

Model Formulation

The current study empirically examines the effect of MW reported in multiple consecutive years upon CE through ordinary least squares (OLS) models that combine the MW indicator variables with control variables. Equation (1) tests whether reported MW result in a higher CE (H1). Additionally, the MW indicator variables within Equation (1) establish whether a negative CE impact is associated with reporting MW in multiple consecutive years (H2 and H3). All variables included in the model below are defined in Appendix B:

$$\begin{aligned} CE_i = {} & \beta_0 + \beta_1{*}MW1_i + \beta_2{*}MW2_i + \beta_3{*}MW3_i + \beta_4{*}MW4_i + \beta_5{*}MKT\_CAP_i \\ & + \beta_6{*}AGE\_FIRM_i + \beta_7{*}LOSS_i + \beta_8{*}SL\_GR_i + \beta_9{*}FCR_i + \beta_{10}{*}AVGPRED\_ER_i + \varepsilon_i. \end{aligned} \tag{1}$$

The current study also considers a modified version of Equation (1) that includes a variable for the number of MW (*NW*). This modified model is shown as Equation (2) below. In Equation (2), *NW* is measured as the number of MW reported under Section 404. With the combination of the *NW* variable and the number of consecutive years in which a firm reports MW (*MW1*, *MW2*, *MW3*, and *MW4*), Equation (2) tests whether *NW* has an incremental impact on CE (H4):

$$CE_i = \beta_0 + \beta_1 * MW1_i + \beta_2 * MW2_i + \beta_3 * MW3_i + \beta_4 * MW4_i + \beta_5 * NW_i + \beta_6 * MKT\_CAP_i + \beta_7 * AGE\_FIRM_i + \beta_8 * LOSS_i + \beta_9 * SL\_GR_i + \beta_{10} * FCR_i + \beta_{11} * AVGPRED\_ER_i + \varepsilon_i. \tag{2}$$

All regression analyses use cluster-robust standard errors to avoid overstating t-statistics due to multiple observations of the same firm (different fiscal years) within the dataset (Petersen 2009; Gow et al. 2010). The current study uses panel data and, as such, we control for potential MW reporting time trends by including time fixed-effects in the regression analyses. Additionally, we control for industry fixed-effects.[8]

IV. RESULTS

Univariate Analyses

Table 2 reports descriptive statistics for the dependent and independent variables. Table 2 also displays the test results for both a two-group mean comparison test and a Wilcoxon rank-sum test.[9] Table 2, Panel A, reports and compares the variables' mean and median values for the control sample with the same values for the MW sample. The results from the two-group mean comparison test and the Wilcoxon rank-sum test indicate that observations with MW tend to have a significantly higher CE than observations without MW, which supports H1. In line with prior research results (Doyle et al. 2007; Ashbaugh-Skaife et al. 2007), the results in Table 2, Panel A, also indicate that firms with MW tend to be smaller, younger, and report losses more frequently than the firms in the control sample. Additionally, the results indicate that firms reporting MW have a significantly higher predicted analyst forecast error (*AVGPRED_ER*), highlighting the need to include *AVGPRED_ER* within the analysis to control for analyst forecast error.

Differences between the characteristics of observations within each of the MW categories (*MW1*, *MW2*, *MW3*, and *MW4*) are reported in Table 2, Panels B, C, and D. Panel B displays the results of a comparison between the *MW1* and *MW2* observations. These results indicate that *CE* is significantly higher for *MW2* observations than for *MW1* observations. Additionally, the *MW2* observations are associated with a higher percentage of foreign currency translation adjustments (*FCR*), have a significantly higher predicted analyst forecast error (*AVGPRED_ER*), and report a significantly higher number of MW (*NW*). Although most of the variable values are higher for *MW3* observations than for *MW2* observations, Panel C shows no significant differences between the *MW2* and *MW3* observation variables. However, Panel D reveals that *CE* and *NW* are significantly lower for *MW4* observations than for *MW3* observations.

The above results provide weak preliminary support for H2 and H3. Even though the difference between *CE* for *MW2* and *MW3* observations is not statistically significant, the results indicate that firms that report MW in multiple consecutive years have a higher *CE* (12.8, 13.2, and 11.2 percent as compared to 11.1 percent for *MW2*, *MW3*, *MW4*, and *MW1*, respectively). However, the statistics indicate that *MW4* observations have a lower *CE* than *MW3* observations. Although this latter result appears to conflict with H2 and H3, the descriptive statistics reveal that the average number of MW reported by *MW4* observations is lower than the number of MW reported by *MW2* and *MW3* observations (see Table 2, Panel D). We address this issue in the "Multivariate Analyses" section below. In that section, we first analyze the data utilizing a restricted sample that excludes

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[8] We categorize industries according to the Fama-French 48 industry classification (Fama and French 1997).

[9] The two-group mean comparison test examines whether significant differences exist between the means of two groups. The Wilcoxon rank-sum test examines whether significant differences exist between the medians of two groups.

TABLE 2

Univariate Statistics: Differences in Characteristics between the Control and Material Weakness (MW) Samples[a]

Panel A: Comparison of Control Sample (No MW) and MW Sample

	Control Sample		MW Sample	
Variable	**Mean**	**Median**	**Mean**	**Median**
CE_{HL}	0.11	0.10	0.12***	0.11***
			(−9.30)	(−9.24)
MKT_CAP ($M)	6182.63	961.23	2835.01***	481.44***
			(5.09)	(13.39)
AGE_FIRM	17.45	12.00	13.96***	10.00***
			(6.98)	(7.14)
LOSS	0.18	0.00	0.29***	0.00***
			(−10.99)	(−11.39)
FCR	0.26	0.00	0.29**	0.00**
			(−2.19)	(−2.19)
SL_GR	5.80	6.00	5.96*	6.00*
			(−1.73)	(−1.85)
AVGPRED_ER	−0.02	−0.01	−0.03***	−0.02***
			(3.92)	(8.55)
NW	NA	NA	2.34	1.00
			NA	NA

Panel B: Comparison of *MW1* to *MW2*

	MW1 Observations		*MW2* Observations	
Variable	**Mean**	**Median**	**Mean**	**Median**
CE_{HL}	0.11	0.11	0.13***	0.12***
			(−5.71)	(−5.86)
MKT_CAP ($M)	2225.99	496.29	3925.09	428.16
			(−1.23)	(0.83)
AGE_FIRM	13.59	9.00	14.82	10.00*
			(−1.18)	(−1.69)
LOSS	0.28	0.00	0.30	0.00
			(−0.59)	(−0.90)
FCR	0.26	0.00	0.34**	0.00**
			(−2.29)	(−2.29)
SL_GR	6.06	6.00	5.98	6.00
			(0.30)	(0.30)
AVGPRED_ER	−0.02	−0.02	−0.03**	−0.03**
			(2.58)	(2.18)
NW	2.19	1.00	2.69***	2.00***
			(−2.81)	(−4.28)

(continued on next page)

TABLE 2 (continued)

Panel C: Comparison of *MW2* to *MW3*

	MW2 Observations		*MW3* Observations	
Variable	**Mean**	**Median**	**Mean**	**Median**
CE_{HL}	0.13	0.12	0.13	0.13
			(−0.53)	(−0.58)
MKT_CAP ($M)	3925.09	428.16	7050.94	544.73
			(−0.68)	(−0.28)
AGE_FIRM	14.82	10.00	15.14	13.00
			(−0.17)	(−0.89)
LOSS	0.30	0.00	0.32	0.33
			(−0.37)	(−0.52)
FCR	0.34	0.00	0.33	0.00
			(0.12)	(0.12)
SL_GR	5.98	6.00	5.49	5.00
			(1.16)	(1.12)
AVGPRED_ER	−0.03	−0.03	−0.03	−0.02
			(−0.77)	(−0.30)
NW	2.69	2.00	3.17	2.00
			(−1.31)	(−0.73)

Panel D: Comparison of *MW3* to *MW4*

	MW3 Observations		*MW4* Observations	
Variable	**Mean**	**Median**	**Mean**	**Median**
CE_{HL}	0.13	0.13	0.11*	0.11**
			(1.91)	(2.05)
MKT_CAP ($M)	7050.94	544.73	2198.21	285.80
			(0.54)	(−0.15)
AGE_FIRM	15.14	13.00	15.45	14.00
			(−0.11)	(−0.67)
LOSS	0.32	0.33	0.35	0.00
			(−0.38)	(−0.06)
FCR	0.33	0.00	0.43	0.00
			(−0.88)	(−0.88)
SL_GR	5.49	5.00	4.37*	4.00*
			(1.73)	(1.66)
AVGPRED_ER	−0.03	−0.02	−0.01	−0.01*
			(−1.52)	(−1.69)
NW	3.17	2.00	2.00*	1.00
			(1.92)	(1.83)

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*, **, *** Statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

[a] This table presents descriptive statistics for the current study's dependent and independent variables. Means are compared using a two-group mean comparison test (t-test) and the t-values are displayed in parentheses below the mean values. Medians are compared using a Wilcoxon rank-sum test and the z-values are displayed in parentheses below the median values. All variables are defined in Appendices A and B, unless noted otherwise. *MKT_CAP* is reported in thousands of dollars and *AGE_FIRM* is reported in years.

firms that are partially remediating their MW during the years in which they are reporting MW in multiple consecutive years.[10] We then analyze the data using the full sample identified in Table 1.

Table 3 presents the average number of MW reported by a given firm, as well as the number of firms, within each category of MW. These data indicate that, on average, the number of MW reported by a given firm declines monotonically as firms continue to report MW in multiple consecutive years. However, the average number of MW reported within the *MW1* observations increases as the number of multiple consecutive years also increases (see Table 3). More specifically, the average number of MW reported ranges from an average of 1.78 MW reported in the first year for firms reporting MW in a single year to 4.64 MW reported in the first year for firms reporting MW in four or more consecutive years. Overall, the results from Table 3 provide descriptive evidence that partial remediation is occurring. In addition as shown in Table 3, note b, the percentage of firms that are partially remediating their MW is also progressively increasing as we move from firms with *MW2* to those with *MW3* to those with *MW4* (i.e., the percentages are 14.62, 32.39, and 48.28 percent, respectively).

Multivariate Analyses

Table 4 presents OLS regression results for Equations (1) and (2). These results indicate that firm size (*MKT_CAP*), firm age (*AGE_FIRM*), firm financial stability (*LOSS*), firm complexity (*FCR*), and predicted analyst forecast error (*AVGPRED_ER*) are significantly associated with a firm's CE. These outcomes are consistent with the findings of OSR and ACKL.

As indicated in Section II, H2 and H3 do not consider the case where a firm may only partially remediate its MW. One way for a firm to signal partial remediation is through a decline in the number of MW reported in multiple consecutive years. The univariate analyses above (Table 3) indicate that firms are partially remediating and doing so at a progressively increasing rate (i.e., the more consecutive years in which a firm reports MW the more likely that a firm will participate in partial remediation).

Since partial remediation is occurring, and at a progressively increasing rate, we eliminate firms that partially remediate MW from our sample to ensure that we are able to capture the MW and CE relation (i.e., to test H2 and H3 independent of H4). With this restricted sample, we run an OLS regression using Equation (1). The results of this analysis are included in Table 4 as column (1). The results show that the indicator variables, which classify firms into categories by the number of multiple consecutive years of reporting MW, are significant for the *MW2*, *MW3*, and *MW4* observations. These results support H1, in cases where firms report MW in multiple consecutive years. That is, reporting MW results in a higher CE. The results indicate that CE is higher by approximately 1.0 percent when MW are reported in two consecutive years, 2.7 percent when MW are reported in three consecutive years, and 3.4 percent when MW are reported in four or more consecutive years. The significant *MW2*, *MW3*, and *MW4* coefficients are larger than the MW1 coefficient, and the MW4 coefficient is significantly larger than the MW3 coefficient, which is significantly larger than the MW2 coefficient. These latter results support H2 and H3.

The OLS regression results for Equation (1) (Table 4, column (1)) also indicate that if a firm reports MW for the first time, then CE is not significantly higher. This insignificant *MW1* coefficient is similar to the findings of OSR and is examined in more detail in the "Reconciling Results of Prior Research and the Current Study" section below. Nevertheless, evidence from the current study

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[10] All univariate analyses were also run using a restricted sample (i.e., eliminating partially remediating firms). Once these firms are eliminated, our results indicate that the CE increases progressively as the number of consecutive years a firm reported MW increases. Additionally, the number of MW reported increases as the number of multiple consecutive years increases. These results are available from the authors upon request.

TABLE 3

Average Number of Material Weaknesses (MW) Reported and Number of Firms within Each Multiple Consecutive Year Category[a]

Panel A: Average # of MW Reported in Only One Year, and # of Firms

	Average # MW Reported	# of Firms[b]	Std. Dev.
MW1	1.783	624	1.734

Panel B: Average # of MW Reported in Two Consecutive Years, and # of Firms

	Average # MW Reported	# of Firms[b]	Std. Dev.
MW1	2.898	137	2.568
MW2	2.268	142	1.875

Panel C: Average # of MW Reported in Three Consecutive Years, and # of Firms

	Average # MW Reported	# of Firms[b]	Std. Dev.
MW1	4.439	41	4.348
MW2	3.500	44	3.331
MW3	3.000	47	3.593

Panel D: Average # of MW Reported in Four or More Consecutive Years, and # of Firms

	Average # MW Reported	# of Firms[b]	Std. Dev.
MW1	4.640	26	3.239
MW2	3.615	26	3.336
MW3	3.500	24	2.085
MW4	2.000	29	1.336

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[a] Panels A, B, C, and D report the average number of MW that are attributed to firms that report MW in one, two, three, or four consecutive years, respectively.

[b] Although one would expect the number of firms to remain consistent across time, the use of I/B/E/S data to arrive at the CE estimates precludes this occurrence. However, the number of observations remains consistent with the previously defined samples used in both Tables 1 and 2. The total number of *MW1* observations is equal to 828, the total number of *MW2* observations is equal to 212, the total number of *MW3* observations is equal to 71, and the total number of *MW4* observations is equal to 29. Of the 29 firms that report *MW4*, 14 out of the 29 (i.e., 48.28 percent) firms had partially remediated their MW. For *MW3*, 23 out of 71 (32.39 percent) firms had partially remediated their MW. For *MW2*, 31 out of the 212 (14.62 percent) firms had partially remediated their MW.

suggests that if a firm does not remediate MW after the first year of reporting, then the firm will experience a higher CE as the firm continues to report MW.

To assess the impact of partial remediation (H4), we begin by running an OLS regression of Equation (1) using the full sample of firms. These results are listed in Table 4, column (2) and show that the indicator variables, which classify firms into categories by the number of multiple consecutive years of reporting MW, are significant for the *MW2* and *MW3* observations. The results indicate that CE is higher by approximately 1.0 percent when MW are reported in two consecutive

TABLE 4

The Relation between Multiple Consecutive Material Weaknesses (MW) and Cost of Equity (CE_{HL})

CE Measure	(1) CE_{HL}	(2) CE_{HL}	(3) CE_{HL}
Intercept	0.109***	0.106***	0.106***
	(0.004)	(0.005)	(0.005)
MW1	0.001	0.002	0.003
	(0.001)	(0.001)	(0.003)
MW2	0.010**	0.010***	0.010***
	(0.004)	(0.003)	(0.003)
MW3	0.027***	0.018***	0.017***
	(0.004)	(0.006)	(0.006)
MW4	0.034***	−0.001	−0.002
	(0.003)	(0.008)	(0.008)
NW			0.001**
			(0.000)
MKT_CAP	−0.004***	−0.004***	−0.004***
	(0.000)	(0.000)	(0.000)
AGE_FIRM	0.001**	0.001**	0.001**
	(0.000)	(0.001)	(0.001)
LOSS	0.016***	0.016***	0.017***
	(0.002)	(0.002)	(0.002)
FCR	0.003***	0.003***	0.003***
	(0.001)	(0.001)	(0.001)
SL_GR	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)
AVGPRED_ER	−0.137***	−0.124***	−0.143***
	(0.014)	(0.017)	(0.028)
Industry Fixed Effects	Included	Included	Included
Year Fixed Effects	Included	Included	Included
Observations	14,934[a]	15,116	15,116
Adj. R^2	24.30%	23.40%	24.50%

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*, **, *** Statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

[a] The column (1) analysis contains fewer observations because firm observations are removed if the firm partially remediated MW (reduced the number of MW reported in multiple consecutive years).

Columns (1) and (2) report results for the following OLS regression:

$$CE_i = \beta_0 + \beta_1{*}MW1_i + \beta_2{*}MW2_i + \beta_3{*}MW3_i + \beta_4{*}MW4_i + \beta_5{*}MKT\_CAP_i + \beta_6{*}AGE\_FIRM_i + \beta_7{*}LOSS_i + \beta_8{*}SL\_GR_i + \beta_9{*}FCR_i + \beta_{10}{*}AVGPRED\_ER_i + \varepsilon_i. \tag{1}$$

Column (3) reports results for the following OLS regression:

$$CE_i = \beta_0 + \beta_1{*}MW1_i + \beta_2{*}MW2_i + \beta_3{*}MW3_i + \beta_4{*}MW4_i + \beta_5{*}NW_i + \beta_6{*}MKT\_CAP_i + \beta_7{*}AGE\_FIRM_i + \beta_8{*}LOSS_i + \beta_9{*}SL\_GR_i + \beta_{10}{*}FCR_i + \beta_{11}{*}AVGPRED\_ER_i + \varepsilon_i. \tag{2}$$

Robust standard errors are listed in parentheses. All variables are defined in Appendices A and B.

years and higher by 1.8 percent when MW are reported in three consecutive years. The significant *MW2* and *MW3* coefficients are larger than the *MW1* coefficient and the *MW3* coefficient is significantly larger than the *MW2* coefficient. In comparing the figures in column (1) with those in column (2), it is clear that partial remediation has a positive impact on CE (i.e., reduces CE) for *MW3* and *MW4*. In fact, due to the large percentage of firms that partially remediate MW in the *MW4* case (i.e., 48 percent), the sign on *MW4* actually reverses (although it is not statistically significant). For *MW3*, 32.39 percent of the firms partially remediate their MW and this also has a positive impact (i.e., reduction) on the CE of the firms, although this impact is not enough to reverse the sign. In the case of *MW2*, only 14.62 percent of the firms partially remediate, with no statistical impact on the analysis. These results support H4.

We also examine the impact of partial remediation through Equation (2). Equation (2) allows us to directly examine the impact of the number of MW on CE, using all observations within the sample. The results from Table 4, column (3), indicate that the number of MW is positively and significantly related to CE. This evidence provides additional support for H4 that a reduction in the number of MW (i.e., partial remediation) results in a lower CE for firms that report MW in multiple consecutive years. More specifically, firms experience a 0.1 percent decrease in CE for each of the MW that are remediated. The two analyses provided in columns (2) and (3) indicate that partial remediation, rather than the presence or absence of MW, is a factor that leads to a lower CE.[11]

Overall, the results of Table 4 provide evidence in support of all four of our hypotheses. To summarize, the results indicate that reporting MW leads to a higher CE. Moreover, absent partial remediation, reporting MW in multiple consecutive years results in a higher CE than that obtained if a firm reports MW in a single year and CE is progressively higher as firms continue to report MW in consecutive years. Additionally, the evidence suggests that firms that report MW in multiple consecutive years and partially remediate their MW over the multiple consecutive years experience a lower CE.

Reconciling Results of Prior Research and the Current Study

Based on data filed with the SEC from November 2004 through January 2006, and after controlling for key firm characteristics, OSR (2007, 1256) conclude that SOX Section 404 MW "do not appear to be directly associated with cost of equity." They also conclude that remediation of MW does not lower CE. In contrast, based on reports filed with the SEC under SOX Sections 302 and 404 during the time period of November 2003 through September 2005, ACKL find that MW are associated with a lower CE. ACKL also find, based on intertemporal change analyses, that remediation of MW is associated with a decrease in CE. As we show in the subsequent analyses, these differing results appear to be driven by the fact that ACKL include Section 302 MW, as well as Section 404 MW, whereas OSR only consider Section 404 MW. Results from the current study indicate that, while CE is not higher during the first year of MW reporting (consistent with OSR), there is a progressively significant and negative impact on CE when a firm reports MW in two, three, and four consecutive years (consistent with ACKL), as displayed in Table 4, column (1). The current study also finds that remediation of Section 404 MW is associated with a lower CE (consistent with ACKL), especially when such remediation occurs after reporting MW in multiple consecutive years.

ACKL note that "[o]ne potential explanation for the 'no association' result in the OSR study is due to their research design choice to 'look ahead' to a future SOX 404 audit opinion to determine their coding of firms' internal control status" (ACKL 2009, 37). ACKL perform two steps to reconcile their results with OSR. First, they replicate the OSR approach using the ACKL Section 404 MW data. Within this step, firms that report Section 302 MW and subsequently do not report Section 404 MW are coded as firms that *do not* report Section 404 MW. The OLS regression results of this first step closely resemble those of OSR. As a second step, ACKL recode firms that report Section 302 MW and do not report Section 404 MW as firms that *do* report Section 404 MW, resulting in OLS regression results that are similar to those obtained within the first part of the ACKL study. ACKL follow most of the procedures utilized by OSR. However, the OSR study does not suffer from this "look-ahead bias" because it eliminates observations that report Section 302 MW and subsequently report no Section 404 MW. Thus, the replication performed by ACKL does not exactly follow OSR.

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[11] As noted previously, earlier studies did not fully examine this issue because they did not include more than two years of data. The constrained samples were due, in large part, to the time period in which those studies were completed.

In light of the similar focus of the current study to the OSR and ACKL studies, this section attempts to reconcile the differences between OSR and ACKL using currently available data, as well as reconcile the results from the current study to the results from these two prior studies. Our reconciliation follows a three-step procedure.[12] As a first step, we replicate the OSR and ACKL studies within their respective time periods. The ACKL replication includes both Section 302 MW and Section 404 MW. On the other hand, only Section 404 MW are used in the OSR replication. The replications follow the steps employed within the original two studies. In untabulated findings, the replications of these studies produce results similar to those noted in the preceding paragraph. Since the main difference between OSR and ACKL is that ACKL include MW reported under both Section 302 and Section 404, the current study also replicates ACKL after removing Section 302 MW from the sample. This additional procedure examines whether Section 302 MW are the driving force behind the differing results between the OSR and ACKL studies. After removing Section 302 MW from the current study's replication of ACKL, these results reconcile to the current study's replication of OSR.[13]

It is important to recall that the OSR and ACKL studies were conducted several years prior to the current study. More specifically, OSR base their data set on annual SEC filings reported no later than January 2006 and ACKL base their data set on annual and quarterly SEC filings reported no later than September 2005. In contrast, the current study includes Section 404 MW from annual SEC filings reported from 2004 through 2009. Given the availability of additional data, we use the same data as used in Table 4, column (1) (i.e., the sample that excludes firms that partially remediate their MW), and as part of the second step in the reconciliation process, we separate the Section 404 MW into two distinct time periods, with the earlier period paralleling the OSR time frame. The later time period starts at the point that the earlier one ends. More specifically, we split MW into two subsamples dependent upon the time period in which they occur: (1) November 2004 through January 2006, and (2) February 2006 through December 2009. Given that the results from the current study indicate the *MW1* coefficient is insignificant, as a variation on the above data split, we also split the set of observations with MW into the following two different subsamples: (a) *MW1* observations, and (b) *MW2*, *MW3*, and *MW4* observations. After splitting the data, the second step examines the differences and similarities between the types of MW reported in the current study's subsamples and the types of MW reported in the OSR and ACKL replications. The types of MW are taken from Audit Analytics, which categorizes each MW into one of 21 different types, as listed in Table 5. The listed MW names provide a description of MW that are included in each type.

The above subsamples include the following: (1) MW derived from the OSR replication, (2) all (i.e., Section 302 and Section 404) MW derived from the replication of ACKL, (3) Section 302

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[12] We thank one of the anonymous reviewers for suggesting that we follow the first two of these three steps.

[13] Details associated with the reconciliation results discussed in this paragraph are available from the authors upon request.

MW derived from the replication of ACKL, (4) Section 404 MW derived from the replication of ACKL, (5) Section 404 MW reported from November 2004 through January 2006 (i.e., the current study's earlier time period), (6) Section 404 MW reported from February 2006 through December 2009 (i.e., the current study's later time period), (7) *MW1* observations from the current study, and (8) *MW2*, *MW3*, and *MW4* observations from the current study.[14]

In untabulated analyses, results of two-group mean comparisons indicate that the types of MW reported within the total ACKL MW sample differ significantly from the types of MW reported within the OSR MW sample. To provide a clearer picture of the ACKL MW sample composition, we separate the total ACKL sample into Section 302 MW and Section 404 MW. The ACKL Section 404 MW type composition is similar to the OSR MW type composition. However, the ACKL Section 302 MW type composition differs significantly from the OSR MW type composition. More specifically, these results suggest that entity-level types of MW (i.e., information technology, software and security issues, ethical or compliance issues with personnel, etc.) are more prevalent within the ACKL Section 302 MW sample.[15,16]

A comparison between the MW type composition of the OSR MW sample and the current study's earlier time period 404 MW sample reveals that the types of MW reported within the earlier time period closely resemble those reported within the OSR MW sample. The observations from the current study's later time period 404 MW sample differ significantly from the OSR MW sample and the observations from the current study's earlier time period. We then compare the types of MW reported in the ACKL Section 302 MW sample and the types of MW reported in the current study's later time period 404 MW sample to the types of MW reported in the OSR MW sample. These latter results indicate that the types of MW reported in the OSR MW sample differ significantly from the types of MW reported in the ACKL Section 302 MW sample and the current study's later time period 404 MW sample. In particular, the ACKL Section 302 MW and the current study's later time period 404 MW include a significantly higher percentage of entity-level MW than in the OSR sample. The similarities in the composition between the ACKL 302 MW sample and the current study's later time period 404 MW sample help to explain why the overall findings from the current study are consistent with the ACKL finding of a significant association between MW and CE. Table 5 displays the types of MW reported in the current study's earlier and later time periods as well as a two-group mean comparison between the types of MW from these two subsamples.

To investigate the current study's insignificant *MW1* coefficient, we examine differences in the MW composition types reported within the *MW1* observations and the *MW2*, *MW3*, and *MW4* observations (Table 6, column (4)). The results of this analysis reveal that the two groups of observations differ significantly across several types. For example, *MW1* observations have a significantly lower ($p < 0.01$) occurrence of types of MW that involve training, information technology, and journal entry control issues. As an additional analysis, Table 6 compares the *MW1* observation types (column (2)) and the *MW2*, *MW3*, and *MW4* observation types (column (3)) to the types of MW within the OSR sample (column (1)). These results indicate that the current study's *MW1* observations have a type composition similar to the OSR sample and have a significantly higher ($p < 0.01$) occurrence of MW for only three of the MW types. On the other hand, significant differences appear between the OSR sample and the current study's multiple

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[14] The subsamples from the current study are taken from the MW sample that does not include firms that partially remediate. This sample includes 795 *MW1* observations, 181 *MW2* observation, 48 *MW3* observations, and 15 *MW4* observations.

[15] Entity-level weaknesses are weaknesses in company-level controls that encompass the control environment of the financial reporting process. Entity-level weaknesses are more difficult to remediate.

[16] Details associated with these results are available from the authors upon request.

TABLE 5

Comparison of Material Weakness (MW) Types between Firms that Report MW in Earlier and Later Time Periods[a]

MW Type[b]	11/04–1/06 Observations (1) % of Firms	2/06–12/09 Observations (2) % of Firms
Accounting documentation, policy, and/or procedure	93.3%	99.4%*** (−5.65)
Accounting personnel resources and competency/training	42.9%	49.5%* (−1.87)
Ethical or compliance issues with personnel	1.4%	7.8%*** (−3.83)
Inadequate disclosure controls (timely, accurate, complete)	0.0%	4.0%*** (−3.41)
Ineffective or understaffed audit committee	0.4%	1.2% (−1.25)
Ineffective regulatory compliance issues	0.7%	1.4% (−0.86)
Information technology, software, security, and access issues	17.4%	19.2% (−0.66)
Insufficient or non-existent internal audit function	1.4%	3.2% (−1.56)
Journal entry control issues	8.9%	10.8% (−0.91)
Management/board/audit committee investigations	0.7%	3.0%*** (−2.17)
Material and/or numerous auditor/YE adjustments	51.8%	60.1%** (−2.26)
Non-routine transaction control issues	15.6%	19.1% (−1.26)
Restatement or non-reliance of company filings	52.5%	42.5%*** (2.815)
Restatement of previous 404 disclosures	12.8%	19.4%** (−2.45)
SAB 108 adjustments noted	0.0%	0.5% (−1.14)
Scope (disclaimer of opinion) or other limitations	2.8%	1.1%** (1.98)
SEC or other regulatory investigations and/or inquiries	0.0%	0.6% (−1.31)
Segregations of duties/design of controls (personnel)	17.7%	12.0%** (2.32)
Senior management competency, tone, or reliability issues	1.8%	7.2%*** (−3.33)
Untimely or inadequate account reconciliations	30.5%	22.7%** (2.53)
Treasury control issues	0.7%	1.5% (−1.02)
Observations	287	752

(continued on next page)

TABLE 5 (continued)

*, **, *** The statistical significance between sample means is indicated at the 0.10, 0.05, and 0.01 levels, respectively.

[a] The 11/04–1/06 Observations are those observations within our sample that are reported in the same time period as the sample used by Ogneva et al. (2007), November 2004 through January 2006. The 2/06–12/09 Observations are those observations within our sample that are reported after the time period employed within the Ogneva et al. (2007) study, February 2006 through December 2009. This table compares the percentage of firms reporting specific types of MW during the earlier time period with the percentage of firms reporting specific MW during the later period. Means are compared using a two-group mean comparison test (t-test) and the t-value results are displayed in parentheses.

[b] MW Type is defined in the Audit Analytics Database. Audit Analytics categorizes each MW within one of 21 different categories.

consecutive year MW observations sample, as indicated in Table 6, column (3). In other words, the types of MW in multiple consecutive years center on policy and personnel issues (e.g., accounting documentation, policy, or procedure; ethical or compliance issues with personnel; information technology, software, security, and access issues). These entity-level MW take longer to remediate, either due to cost or difficulty in implementing change. Also, the types of MW reported in the current study's multiple consecutive year observation sample are similar to the types of MW reported within the ACKL Section 302 sample.

As a third and final step in the reconciliation process, we examine the impact of MW and remediation of MW on CE, within the current study's subsamples, through untabulated OLS regressions of Equation (1). We also include the control firms in these regressions. The results that utilize the current study's earlier and later time period samples indicate that the *MW1* coefficient is insignificant for the earlier time period sample. Alternatively, the *MW1*, *MW2*, *MW3*, and *MW4* coefficients are each positive and significant for the later time period sample. The regression results from the *MW1* observation sample indicate that *MW1* observations taken as a complete group are not significantly related to a higher CE. However, the regression results from the multiple consecutive year MW observations group indicate that the *MW2*, *MW3*, and *MW4* coefficients are significant and positive. Thus, the regression results indicate that the types of MW that are similar to those reported within the OSR study are not significantly associated with a higher CE. However, the types of 404 MW that are similar to those reported within the ACKL study (i.e., the 302 MW) are significantly associated with a higher CE.[17]

In sum, the current study helps to reconcile the differing results in the OSR and ACKL studies, in terms of the types of MW reported under Sections 302 and 404 and the different time periods in which the MW are reported. This reconciliation shows that MW, in general, do indeed lead to a higher CE. In addition, remediation of these MW appears to result in a lower CE. Both of these findings support the original findings of the ACKL study.

V. ROBUSTNESS CHECKS

The robustness results discussed below are based on firms that do not partially remediate their MW when they report MW in multiple consecutive years (i.e., the MW sample from Table 4, column (1)). Although not discussed below, in untabulated results, we use the full MW sample and find results similar to those obtained in Table 4, column (2).[18]

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[17] The results of these regressions are available from the authors upon request.

[18] The results that use the full MW sample are available from the authors upon request.

TABLE 6

Comparison of Material Weakness (MW) Types Reported by Firms in the Current Study to MW Types Reported by Firms in Ogneva et al. (2007) (OSR)[a]

MW Type[b]	OSR Sample (1)	*MW1* Obs. (2)	*MW2, MW3* and *MW4* Obs. (3)	(4)
Accounting documentation, policy, and/or procedure	93.5%	97.2%*** (−2.91)	98.9%*** (−2.89)	98.9% (−1.38)
Accounting personnel resources and competency/training	47.0%	44.0% (0.93)	61.7%*** (−3.25)	61.7%*** (−4.39)
Ethical or compliance issues with personnel	1.8%	5.3%*** (−2.71)	8.0%*** (−3.52)	8.0% (−1.38)
Inadequate disclosure controls (timely, accurate, complete)	0.0%	1.7%** (−2.44)	6.9%*** (−5.00)	6.9%*** (−3.90)
Ineffective or understaffed audit committee	0.3%	0.9% (−1.14)	1.1% (−1.12)	1.1% (−0.16)
Ineffective regulatory compliance issues	0.3%	1.1% (−1.30)	1.6%* (−1.65)	1.6% (−0.60)
Information technology, software, security, and access issues	18.6%	15.2% (1.42)	32.4%*** (−3.61)	32.4%*** (−5.51)
Insufficient or non-existent internal audit function	1.5%	2.1% (−0.73)	4.8%** (−2.27)	4.8%** (−2.02)
Journal entry control issues	10.9%	8.1% (1.50)	18.6%** (−2.46)	18.6%*** (−4.28)
Management/board/audit committee investigations	1.2%	2.0% (−0.95)	3.7%* (−1.95)	3.7% (−1.40)
Material and/or numerous auditor/YE adjustments	54.7%	55.3% (−0.18)	66.5%*** (−2.64)	66.5%*** (−2.78)
Non-routine transaction control issues	16.9%	15.7% (0.47)	27.1%*** (−2.81)	27.1%*** (−3.66)
Restatement or non-reliance of company filings	52.7%	48.9% (1.14)	31.9%*** (4.67)	31.9%*** (4.23)
Restatement of previous 404 disclosures	13.3%	19.9%*** (−2.62)	7.4%** (2.05)	7.4%*** (4.05)
SAB 108 adjustments noted	0.0%	0.4% (−1.16)	0.00% NA	0.0% (0.87)
Scope (disclaimer of opinion) or other limitations	3.8%	1.9%* (1.94)	0.5%** (2.27)	0.5% (1.30)
SEC or other regulatory investigations and/or inquiries	0.3%	0.1% (0.58)	1.6%* (−1.65)	1.6%*** (−2.76)
Segregations of duties/design of controls (personnel)	19.5%	12.1%*** (3.22)	20.2% (−0.19)	20.2%*** (−2.89)
Senior management competency, tone, or reliability issues	3.3%	5.2% (−1.42)	6.9%* (−1.93)	6.9% (−0.92)
Untimely or inadequate account reconciliations	32.0%	24.1%*** (2.71)	28.7% (0.77)	28.7% (−1.30)
Treasury control issues	0.6%	0.9% (−0.58)	2.7%** (−1.99)	2.7%* (−1.89)
Observations	338	795	244	

(continued on next page)

TABLE 6 (continued)

*, **, *** The statistical significance between sample means is indicated at the 0.10, 0.05, and 0.01 levels, respectively.

[a] The OSR Sample is composed of observations that were collected to perform a replication of the Ogneva et al. (2007) study. The *MW1* observations are observations from the current study that are associated with firms that have reported MW for the first time. The *MW2*, *MW3*, and *MW4* observations are observations from the current study that are associated with firms that are reporting MW for at least a second time. This table compares the percentage of firms reporting specific types of MW for the first time and for at least a second time (columns (2) and (3)) with the types of MW reported within the Ogneva et al. (2007) study. Additionally, this table compares the difference between the types of MW observed in firms that are reporting MW for the first time compared to firms that are reporting MW for at least a second time (column (4)). Means are compared using a two-group mean comparison test (t-test) and t-values are displayed in parentheses.

[b] MW Type is defined in the Audit Analytics Database. Audit Analytics categorizes each MW within one of 21 different categories.

Control Variables

Prior research uses different metrics for *firm financial stability*, *firm complexity*, *firm fluctuation*, and *predicted analyst forecast error* (OSR; ACKL). To confirm that the results are robust to different specifications for each of these variables, we recomputed the analyses after substituting different proxies for each of these control variables in Equation (1).[19] We check the robustness of the firm financial stability measure using two additional metrics in lieu of the two-year loss measure: (1) the Altman Z-score decile ranking (Doyle et al. 2007), developed by Altman (1968), and (2) leverage (Ashbaugh-Skaife et al. 2007). With respect to firm complexity, we substitute the number of business segments (Doyle et al. 2007) in place of the foreign currency translation metric. Additionally, in lieu of extreme sales growth as a proxy for firm fluctuations, we repeat the analyses with a measure that represents restructuring (Doyle et al. 2007). The results remain robust to each of these alternative specifications.

The prior published CE literature examines the impact of analyst forecast bias on CE estimates and uses a predicted analyst forecast error measure as a control variable (Dhaliwal et al. 2005; OSR; Hughes et al. 2008). As such, the current study tests whether the regression results are also robust to two alternative predicted analyst forecast error estimates that are developed within the OSR and Gode and Mohanram (2010) studies. OSR employ a variation of the Hughes et al. (2008) measure as their proxy for analyst forecast error and include this measure as a control variable. Substituting the OSR measure into the previous analyses does not significantly affect the main results. As an alternative to the control variable approach, Gode and Mohanram (2010) remove the predicted analyst forecast error from the earnings estimates prior to calculating CE. Accordingly, we examine whether there is a difference in the results when the error estimates are removed directly from the analysts' forecasts. Results (untabulated) remain robust to using the CE_{EAST} measure of CE, which is the CE measure used by both ACKL and OSR. Appendix B describes the calculations for each of the above variables.[20]

Change in Cost of Equity

The main analyses measure the *CE* variable (i.e., the dependent variable) in terms of the absolute level of CE. However, to provide additional robustness to the results, we recomputed Equation (1) with the *CE* variable measured in terms of changes in CE. Given the possibility that other events and firm factors could concurrently affect CE, the results using the different measure of CE provide additional support for the main results concerning the positive relation between MW and CE, as well as the positive relation between reporting MW in multiple consecutive years and CE. However, because we use I/B/E/S data to calculate *CE*, the number of available observations is reduced when we measure CE in terms of its change.[21] We calculate the change in CE for each year *t* observation as the difference between CE as of the end of June, year *t*+1 and CE as of the end of June, year *t*. The June date is used because the annual reports, and thus Section 404 MW, for most firms are not filed with the SEC until after December and before the end of June. For example, we measure the change in CE for 2004 as the difference between CE at the end of June of 2005 and CE at the end of June of 2004. Accordingly, for the overwhelming majority of firms within the sample,[22] the change in CE is post-disclosure CE (but prior to the next disclosure) minus pre-disclosure CE. This method of measuring the change in CE is similar to that used by ACKL.

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[19] To avoid issues associated with multicollinearity, multiple metrics are not assigned to each independent control variable in the main analyses.

[20] The details associated with these results are available from the authors upon request.

Based on this change measure of CE, we recompute all prior analyses to assess whether the results remain robust to this new measure of the dependent variable. The findings show that the previous results are robust, and even stronger, when we measure CE as a change variable. More specifically, as shown in Table 7, the results indicate that MW are associated with an increase in CE that intensifies as a firm continues to report MW in consecutive years.

Difference-in-Differences

It may be possible that the differences in CE between firms that completely remediate MW and those that do not remediate MW are driven by omitted and unobserved differences over time. Another approach to mitigate this concern, aside from regression analyses that measure the change in CE as the dependent variable, is the difference-in-differences method. Accordingly, we examine the difference in the average CE prior to remediation and following remediation for both sets of firms (i.e., the set of firms that completely remediate and the set of firms that do not remediate). A significantly larger decrease in CE for firms that completely remediate relative to firms that do not remediate would provide additional evidence of the benefits of remediation.

To conduct the difference-in-differences analyses, we divide the sample into two-year time periods (2004–2005, 2005–2006, etc.) and measure the difference in CE for firms that completely remediate versus firms that do not remediate over each two-year period using all five *CE* measures that are described in Appendix A. Table 8 reports the results of the analyses using the CE_{HL} measure (i.e., the average measure). Limitations in the size of the control (no remediation) and treatment (remediation) samples prevent use of the 2008–2009 period.[23] The difference-in-differences results obtained using the average *CE* measure indicate a strong association between complete remediation and a lower CE. More specifically, Table 8 shows that the difference in CE for firms that remediated (i.e., treatment firms) as compared to the firms that did not remediate (i.e., the control firms) is larger in the time period after remediation. These findings corroborate our earlier findings that remediation of MW results in a

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[21] I/B/E/S coverage is not consistent throughout time for the firms under consideration in the current study.

[22] Eighty percent of firms in the sample have a December 31 year-end and will likely file their annual reports with the SEC between March and April of the following year. Additionally, the majority of firms within the sample (95 percent) have a year-end that ranges from June 30 to February 28. As such, the current method for calculating the change in CE takes the difference between pre-disclosure CE and post-disclosure CE.

[23] Limitations in the sample size are due to the inconsistency of available I/B/E/S estimates for the firms involved in the period under review.

TABLE 7

The Relation between Multiple Consecutive Material Weaknesses (MW) and Change in Cost of Equity (CE_{HL})[a]

CE Measure	(1) ΔCE_{HL}[b]
Intercept	−0.010*
	(0.005)
MW1	0.011***
	(0.002)
MW2	0.019***
	(0.005)
MW3	0.025**
	(0.010)
MW4	0.029***
	(0.003)
MKT_CAP	−0.001**
	(0.000)
AGE_FIRM	0.001**
	(0.000)
LOSS	0.011***
	(0.003)
FCR	0.001
	(0.001)
SL_GR	0.000
	(0.001)
AVGPRED_ER	−0.315***
	(0.027)
Industry Fixed Effects	Included
Year Fixed Effects	Included
Observations	12,873
Adj. R^2	12.10%

*, **, *** Statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

[a] Table 7 reports results for the following OLS regression:

$$\Delta CE_{HLi} = \beta_0 + \beta_1 {*} MW1_i + \beta_2 {*} MW2_i + \beta_3 {*} MW3_i + \beta_4 {*} MW4_i + \beta_5 {*} MKT\_CAP_i + \beta_6 {*} AGE\_FIRM_i + \beta_7 {*} LOSS_i + \beta_8 {*} SL\_GR_i + \beta_9 {*} FCR_i + \beta_{10} {*} AVGPRED\_ER_i + \varepsilon_i.$$

Compared with Equation (1), this regression uses changes specification for the CE variable as opposed to the levels specification of the CE variable.

[b] Robust standard errors are included within the parentheses. All variables are defined in Appendices A and B, unless otherwise noted. ΔCE_{HL} is measured as the difference between CE as of the end of June, year t+1 (post-disclosure) and CE as of the end of June year t (pre-disclosure).

CE decrease. However, the results based on CE_{HL} are stronger than when the other four CE measures are considered, suggesting that the above claim regarding the difference-in-differences analyses based on CE_{HL} should be tempered.[24]

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[24] The details of these results are available from the authors upon request.

TABLE 8

Average Cost of Equity (CE) Estimates: Difference-in-Differences Applied to Firms that Completely Remediate Versus Firms that do not Remediate (CE_{HL} Measure)[a]

Panel A: Effect of Remediation on CE_{HL} (Comparing 2005 to 2004)

	Remediation Sample	Non-Remediation Sample	Difference
2005	0.092	0.121	−0.029
2004	0.099	0.104	−0.005
05 – 04	−0.007	0.017	−0.024***
Standard Error			0.009
n	137	45	

Panel B: Effect of Remediation on CE_{HL} (Comparing 2006 to 2005)

	Remediation Sample	Non-Remediation Sample	Difference
2006	0.096	0.123	−0.027
2005	0.107	0.109	−0.002
06 – 05	−0.011	0.014	−0.025***
Standard Error			0.008
n	107	41	

Panel C: Effect of Remediation on CE_{HL} (Comparing 2007 to 2006)

	Remediation Sample	Non-Remediation Sample	Difference
2007	0.084	0.115	−0.031
2006	0.111	0.116	−0.005
07 – 06	−0.027	−0.001	−0.026***
Standard Error			0.009
n	63	29	

Panel D: Effect of Remediation on CE_{HL} (Comparing 2008 to 2007)

	Remediation Sample	Non-Remediation Sample	Difference
2008	0.100	0.133	−0.033
2007	0.105	0.107	−0.002
08 – 07	−0.005	0.026	−0.031**
Standard Error			0.015
n	41	15	

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*, **, *** Statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

[a] The remediation sample is composed of observations that report MW in the first year examined and remediate all MW by the next year examined. The non-remediation sample is composed of firms that report MW in both years. The difference-in-difference estimator is calculated as the difference between the difference of the mean CE estimates for the remediation sample and the difference between the mean CE estimates for the non-remediation sample. CE_{HL} is defined in Appendices A and B.

Returns Analyses

As an additional robustness check, we substitute stock market returns as the dependent variable in Equation (1). Prior event studies have examined the market's reaction to MW (Beneish et al. 2008; Hammersley et al. 2008; Ashbaugh-Skaife et al. 2009). CE is theoretically the expected return required by shareholders and an indirect measure of firm performance. However, substituting actual cumulative stock market returns as the dependent variable provides additional support as to whether there is a significant association between the number of multiple consecutive years in which MW are reported and firm returns. We measure stock returns for each observation as the firm's 12-month cumulative return minus the 12-month cumulative return of a CRSP market value-weighted index. The untabulated results reveal that reporting MW results in a lower return. Additionally, and in line with the results that employ CE as the dependent variable, the severity of the decrease in returns is significantly greater as the number of multiple consecutive years increases.[25,26]

Potential Explanations for Consecutive Material Weaknesses

Firms that report MW in multiple consecutive years (i.e., firms that are not quick to remediate MW) could be doing so for several different reasons, including the following three potential explanations.[27] First, the difficulty associated with remediating certain types of MW may limit a firm's ability to fully remediate MW. Second, it may be that a firm realizes the importance of remediation but does not have the financial resources necessary to remediate. Firms falling into this second potential explanation would likely be suffering from financial distress. Third, a firm may be slow to remediate MW because the monitoring component in its internal control framework is ineffective (i.e., ineffective monitoring is only one potential explanation for the lack of remediation).[28]

Table 9 displays comparisons of the types of MW between each of the MW groups (*MW1*, *MW2*, *MW3*, and *MW4*). Firms continue to report entity-level MW—MW that take a longer period of time to remediate—in multiple consecutive years (i.e., (1) accounting personnel resources, competency, and training, and (2) information technology, software, security, and access issues). Although it is clear that certain types of MW take a longer period of time to remediate, the relation between CE and specific types of MW has not yet been examined. Equation (3) uses all observations (firms with partial remediation are included) within our sample to test whether the types of MW reported impact CE:

$$CE_{HLi} = \beta_0 + \beta_n * \sum_{i=1}^{n} MW\_Type + \alpha_n * \sum_{i=1}^{n} Control\_Variables + \varepsilon_i. \tag{3}$$

As indicated by the OLS regression results in Table 10, entity-level MW are more closely associated with a higher CE. The results from the above analysis provide evidence that it is the

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[25] Sluggishness in revisions could cause analysts to not fully adjust their forecasts to reflect negative news (Guay et al. 2011). If the forecasts are not adjusted accordingly, then the CE measures would be biased upward at a progressive rate as firms continue to report MW consecutively, explaining the result. However, forecast sluggishness is included in the calculation of the predicted analyst forecast measure (see Appendix B). Since this variable is already accounted for within the predicted analyst forecast measure, we do not include an additional variable to account for forecast sluggishness.

[26] The details of these results are available from the authors upon request.

[27] We thank Daniel Beneish for originally suggesting the need to discuss possible explanations for firms not remediating MW.

[28] The Committee of Sponsoring Organizations of the Treadway Commission (COSO) clearly discusses the monitoring component of internal control (COSO 1992, 2009).

TABLE 9
Comparisons of Material Weakness (MW) Type[a]

MW Type[b]	*MW1* Obs.	*MW2* Obs.	*MW3* Obs.		*MW4* Obs.	
	(1)	(2)	(3)	(4)	(5)	(6)
Accounting documentation, policy, and/or procedure	97.2%	98.5%	100.0%	100.0%	100.0%	100.0%
		(−0.84)	(−1.10)	(−0.80)	(−0.66)	NA
Accounting personnel resources and competency/training	44.0%	60.3%***	64.3%**	64.3%	66.7%*	66.7%
		(−3.47)	(−2.58)	(−0.46)	(−1.75)	(−0.16)
Ethical or compliance issues with personnel	5.3%	9.9%**	2.4%	2.4%	6.7%	6.7%
		(−2.04)	(0.84)	(1.56)	(−0.23)	(−0.77)
Inadequate disclosure controls (timely, accurate, complete)	1.7%	3.8%	7.1%**	7.1%	33.3%***	33.3%**
		(−1.56)	(−2.43)	(−0.89)	(−8.34)	(−2.61)
Ineffective or understaffed audit committee	0.9%	1.5%	0.0%	0.0%	0.0%	0.0%
		(−0.62)	(0.63)	(0.80)	(0.38)	NA
Ineffective regulatory compliance issues	1.1%	0.8%	2.4%	2.4%	6.7%**	6.7%
		(0.32)	(−0.78)	(−0.85)	(−1.99)	(−0.77)
Information technology, software, security, and access issues	15.2%	32.1%***	33.3%***	33.3%	33.3%*	33.3%
		(−4.71)	(−3.12)	(−0.15)	(−1.92)	(0.00)
Insufficient or non-existent internal audit function	2.1%	6.9%***	0.0%	0.0%*	0.0%	0.0%
		(−3.02)	(0.96)	(1.75)	(0.57)	NA
Journal entry control issues	8.1%	16.0%***	28.6%***	28.6%*	13.3%	13.3%
		(−2.88)	(−4.51)	(−1.81)	(−0.73)	(1.17)
Management/board/audit committee investigations	2.0%	3.1%	7.1%**	7.1%	0.0%	0.0%
		(−0.76)	(−2.18)	(−1.17)	(0.55)	(1.06)
Material and/or numerous auditor/YE adjustments	55.3%	66.4%**	61.9%	61.9%	80.0%*	80.0%
		(−2.37)	(−0.83)	(0.53)	(−1.91)	(−1.27)
Non-routine transaction control issues	15.7%	26.7%***	26.2%*	26.2%	33.3%*	33.3%
		(−3.08)	(−1.79)	(0.07)	(−1.84)	(−0.52)
Restatement or non-reliance of company filings	48.9%	35.9%***	28.6%**	28.6%	6.7%***	6.7%*
		(2.77)	(2.58)	(0.87)	(3.26)	(1.75)
Restatement of previous 404 disclosures	19.9%	9.9%***	2.4%***	2.4%	0.0%*	0.0%
		(2.72)	(2.83)	(1.56)	(1.93)	(0.59)
Scope (disclaimer of opinion) or other limitations	1.9%	0.8%	0.0%	0.0%	0.0%	0.0%
		(0.90)	(0.89)	(0.57)	(0.53)	NA
SEC or other regulatory investigations and/or inquiries	0.1%	2.3%***	0.0%	0.0%	0.0%	0.0%
		(−3.41)	(0.24)	(0.99)	(0.14)	NA
Segregations of duties/design of controls (personnel)	12.1%	19.8%**	21.4%*	21.4%	20.0%	20.0%
		(−2.41)	(−1.77)	(−0.22)	(−0.92)	(0.11)
Senior management competency, tone, and reliability issues	5.2%	7.6%	7.1%	7.1%	0.0%	0.0%
		(−1.12)	(−0.55)	(0.10)	(0.91)	(1.06)
Untimely or inadequate account reconciliations	24.1%	25.2%	38.1%**	38.1%	33.3%	33.3%
		(−0.26)	(−2.04)	(−1.62)	(−0.82)	(0.32)
Treasury control issues	0.9%	1.5%	7.1%***	7.1%*	0.0%	0.0%
		(−0.62)	(−3.53)	(−1.90)	(0.38)	(1.06)
Observations	795	181	48		15	

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*, **, *** The statistical significance between sample means is indicated at the 0.10, 0.05, and 0.01 levels, respectively.

[a] Means are compared using a two-group mean comparison test (t-test) and the results are displayed as the t-value. Column (2) compares the *MW2* observations to the *MW1* observations. Column (3) compares the *MW3* observations to the *MW1* observations. Column (4) compares the *MW3* observations to the *MW2* observations. Column (5) compares the *MW4* observations to the *MW1* observations. Column (6) compares the *MW4* observations to the *MW3* observations.

[b] MW Type is defined in the Audit Analytics Database. Audit Analytics categorizes each MW within one of 21 different categories.

TABLE 10

The Relation between Material Weakness (MW) Type[a] and Cost of Equity (CE)[b]

Variables	CE_{HL}	Standard Errors
Intercept	0.109***	(0.004)
Accounting documentation, policy, and/or procedure	0.003	(0.004)
Accounting personnel resources and competency/training	0.001	(0.003)
Ethical or compliance issues with personnel	0.008***	(0.002)
Inadequate disclosure controls (timely, accurate, complete)	0.012	(0.019)
Ineffective regulatory compliance issues	−0.018	(0.014)
Information technology, software, security, and access issues	0.006**	(0.003)
Insufficient or non-existent internal audit function	0.003	(0.004)
Journal entry control issues	0.002	(0.009)
Management/board/audit committee investigations	−0.011	(0.011)
Material and/or numerous auditor/YE adjustments	−0.001	(0.006)
Non-routine transaction control issues	−0.008	(0.012)
Restatement or non-reliance of company filings	−0.001	(0.003)
Restatement of previous 404 disclosures	0.003	(0.004)
Scope (disclaimer of opinion) or other limitations	−0.008	(0.006)
SEC or other regulatory investigations and/or inquiries	0.009	(0.012)
Segregations of duties/design of controls (personnel)	0.008*	(0.004)
Senior management competency, tone, and reliability issues	0.000	(0.004)
Untimely or inadequate account reconciliations	0.000	(0.002)
Treasury control issues	0.009	(0.024)
MKT_CAP	−0.004***	(0.000)
AGE_FIRM	0.001**	(0.001)
FCR	0.004***	(0.001)
LOSS	0.015***	(0.002)
SL_GR	0.000	(0.000)
AVGPRED_ER	−0.139***	(0.029)
Industry Fixed Effects	Included	
Year Fixed Effects	Included	
Observations	14,934	
Adj. R^2	22.10%	

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*, **, *** Statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

[a] MW Type is defined as represented through the Audit Analytics Database. Audit Analytics categorizes each MW within one of 21 different categories.

[b] Table 10 presents the following OLS regression:

$$CE_{HLi} = \beta_0 + \beta_n * \sum_{i=1}^{n} MW\_Type + \alpha_n * \sum_{i=1}^{n} Control\_Variables + \varepsilon_i. \tag{3}$$

Compared with Equation (1), this regression examines MW type instead of the number of consecutive years MW is reported. Robust standard errors are included within the parentheses. Variables are defined in Appendices A and B, unless otherwise noted.

entity-level types of MW that tend to persist for several years (e.g., ethical or compliance issues with personnel; information technology, software, and security access issues; segregations of duties/design of controls) that have a significant negative effect on CE.

As evidenced by the regression results in Tables 4, 7, and 10, a firm's financial condition (proxied by the *LOSS*[29] variable) is a significant factor that is associated with a higher CE. Furthermore, Table 2 indicates that the *LOSS* variable is significantly different between firms that report MW and firms that do not report MW (i.e., poor financial conditions are associated with a higher CE). The significant difference between observations with and without MW indicates that a higher percentage of firms that report losses are also more likely to report MW. Moreover, weak evidence reveals that the percentage of firms that report losses increases as the number of consecutive years in which MW are reported increases. These findings suggest that financial constraints are another reason why firms do not quickly remediate MW.

The dataset restricts our ability to examine whether ineffective monitoring helps to explain a firm's inability to quickly remediate MW (i.e., the third potential explanation as to why firms do not quickly remediate MW). Nevertheless, the current study does have some indirect evidence related to this issue. This indirect evidence, shown in Tables 3 and 4, is based on the decreasing number of MW reported in multiple consecutive years (i.e., partial remediation) by firms. In other words, the decreasing number of MW reported in multiple consecutive years suggests that firms are indeed monitoring MW. COSO (2009, 2) defines effective monitoring as the ability of a firm to "identify and correct internal control problems on a timely basis." Thus, firms that continue to report MW in consecutive years may have an ineffective monitoring system. However, the decreasing number of MW reported in multiple consecutive years could be attributed to both the difficulty of remediation as well as a lack of financial resources (i.e., the first two explanations noted above). The current study's data do not enable us to separate these possibilities. Thus, we are unable to draw any definitive conclusions regarding this third potential explanation as to why firms do not quickly remediate MW.

VI. CONCLUDING COMMENTS

Academicians and practitioners recognize and document the importance of good internal control to the survival of organizations. However, the empirical evidence related to firms with poor internal control was scant, at best, until the passage of SOX. Mandatory reporting of material weaknesses (MW) under SOX has resulted in the creation of large empirical databases. These databases have spawned multiple studies. A key issue addressed in this literature concerns the relation between MW and CE. The primary purpose of the current study has been to shed new light on this relation, with particular emphasis on the role of remediation of MW.

Our findings provide evidence that MW negatively impact a firm's CE. We also find evidence of the value associated with remediation of MW. Specifically, the current study shows that the market penalty imposed upon a firm's CE, in the absence of any remediation of MW, increases in relation to the number of consecutive years in which the firm reports MW. However, the results from the current study also show that the market views favorably a reduction in the number of MW reported (i.e., partial remediation). In other words, remediation is not an all-or-nothing proposition. Due to the use of a much larger and richer dataset, the current study helps to reconcile the mixed findings in earlier studies that examine the association between MW and CE. In this latter regard, the overall findings from the current study corroborate those found by Ashbaugh-Skaife et al. (2007).

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[29] Loss is measured as the sum of earnings before extraordinary items (Compustat Data item IB) in years t and $t-1$. If this sum totals less than 0, then the firm is assigned an indicator variable of 1, and 0 otherwise.

As with nearly all empirical studies, ours has its limitations, of which two seem most notable. First, agency concerns are not considered in the current study. However, agency concerns are potentially an important issue that should be addressed in future research that examines the effects of multiple consecutive MW on organizations. Second, the current study does not consider the recent trend that some firms are following in implementing technology-based internal control monitoring systems. It would be interesting to explore the association between multiple consecutive years of MW and technology-based internal control monitoring systems. The above limitations notwithstanding, the current study provides a valuable step forward in enhancing the understanding of the way MW impact the survivability of firms.

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APPENDIX A

CALCULATIONS FOR COST OF EQUITY MEASURES AND PREDICTED ANALYST FORECAST ERROR

Data definitions that are common to all measurements:

P_0 = I/B/E/S reported price of common stock as of the end of June in year *t*;

$BVPS_i$ = book value of equity per share as of year-end *t*–1;

DPS = dividends per share paid during year *t*–1;

LG = I/B/E/S reported consensus long-term growth forecast as of June in year *t*;

$FEPS_i$ = I/B/E/S reported forecasted earnings per share for year *t*+*i*. Example: *FEPS1* and *FEPS2* are the one- and two-year-ahead consensus forecasts reported in June of year *t*.

When $FEPS > 2$ is not available, we calculate consensus forecasts using LG applied to the last available forecast;

$k =$ dividend payout ratio, calculated as dividends divided by net income; and

$r_f =$ risk-free rate, set equal to ten-year Treasury note yield as of the end of June year t.

Cost of Equity Measures

Claus and Thomas (2001)

$$P_0 = BVPS_0 + \frac{ABE_1}{(1+CE_{CT})} + \frac{ABE_2}{(1+CE_{CT})^2} + \frac{ABE_3}{(1+CE_{CT})^3} + \frac{ABE_4}{(1+CE_{CT})^4} + \frac{ABE_5}{(1+CE_{CT})^5} + \frac{ABE_5(1+g)}{(CE_{CT}-g)(1+CE_{CT})^5},$$

where: $ABE_i =$ abnormal earnings ($FEPS_i - CE_{CT} * BVPS_{i-1}$). As discussed above, $FEPS_i$ is equal to the consensus forecast, if available, if not, then $FEPS_i = FEPS_{i-1} * (1+LG)$;

$BVPS_i = BVPS_{i-1} - (k * FEPS_i) + FEPS_i$; and

$g =$ growth in abnormal earnings, $r_f - 0.03$.

Gebhardt, Lee, and Swaminathan (2001)

$$P_0 = BVPS_0 + \frac{ROEF_1 - CE_{GLS}}{(1+CE_{GLS})} BVPS_0 + \frac{ROEF_2 - CE_{GLS}}{(1+CE_{GLS})^2} BVPS_1 + TV,$$

$$TV = \sum_{i=3}^{T-1} \frac{ROEF_i - CE_{GLS}}{(1+CE_{GLS})^i} BVPS_{i-1} + \frac{ROEF_T - CE_{GLS}}{(1+CE_{GLS})^{T-1}} BVPS_{T-1},$$

where:

$ROEF_i =$ forecasted return on equity for year t. For years one through three $ROEF_i$ is calculated as $FEPS_i/BVPS_i$. $ROEF_i$ is calculated as a linear extrapolation to the median industry return on equity;

$BVPS_i = BVPS_{i-1} - (k * FEPS_i) + FEPS_i$; and

$T =$ forecast horizon set equal to 12.

Gode and Mohanram (2003)

$$CE_{GM} = A + \sqrt{A^2 + \frac{FEPS1}{P_0}\left(g_2 - (\gamma - 1)\right)},$$

where:

$$A = \frac{1}{2}\left((\gamma - 1) + \frac{k*FEPS1}{P_0}\right) \text{ and } g_2 = \frac{(FEPS2 - FEPS1)}{FEPS1},$$

$\gamma = 1 - (r_f - 0.03)$.

Easton (2004)

$$CE_{EAST} = \sqrt{\frac{(FEPS2 - FEPS1)}{P_0}}.$$

Hail and Leuz (2006)

$$CE_{HL} = \frac{CE_{CT} + CE_{GLS} + CE_{GM} + CE_{EAST}}{4}.$$

Predicted Analyst Forecast Error

The metric we utilize as a proxy for analyst forecast error mirrors that modeled by Hughes et al. (2008). We calculate forecast error associated with one-year-ahead (ER_F1_i) and two-year-ahead (ER_F2_i) forecasts, for each observation, separately as follows:

$$\begin{aligned} ER\_F1_i = {} & \alpha_0 + \alpha_1 {*} FABRET_i + \alpha_2 {*} MVE_i + \alpha_3 {*} COV1_i + \alpha_4 {*} ACCRUAL_i + \alpha_5 {*} LTG_i \\ & + \alpha_6 {*} LTSG_i + \alpha_7 {*} CHG\_PPE_i + \alpha_8 {*} CHG\_OLTA_i + \alpha_9 {*} QES_i + \alpha_{10} {*} ARET_i \\ & + \alpha_{11} {*} REV1_i + \varepsilon_i; \end{aligned}$$

$$\begin{aligned} ER\_F2_i = {} & \alpha_0 + \alpha_1 {*} FABRET_i + \alpha_2 {*} MVE_i + \alpha_3 {*} COV2_i + \alpha_4 {*} ACCRUAL_i + \alpha_5 {*} LTG_i \\ & + \alpha_6 {*} LTSG_i + \alpha_7 {*} CHG\_PPE_i + \alpha_8 {*} CHG\_OLTA_i + \alpha_9 {*} QES_i + \alpha_{10} {*} ARET_i \\ & + \alpha_{11} {*} REV2_i + \varepsilon_i, \end{aligned}$$

where:

ER_F1, *ER_F2* = median consensus analyst forecast error, measured as actual I/B/E/S earnings for the fiscal year being estimated minus the median analyst estimate divided by stock price. Earnings estimates and stock price are measured as of June of year *t* (prior to submission of financial reports indicating results of the internal control audit);

FABRET = future abnormal stock returns adjusted for size. Returns are accumulated for the 13-month period following error measurement;

MVE = market value of equity, measured as the share price multiplied by the number of shares outstanding from I/B/E/S. Measured as of June of year *t*;

COV1, *COV2* = coverage by analyst for each forecast, measured as the number of analysts that issued earnings forecasts within I/B/E/S at the time of measurement;

ACCRUAL = accounting accruals measured for most recent annual earnings (*t*–1). Measured using the balance-sheet method (change in current assets minus change in cash minus change in current liabilities plus change in current portion of long-term debt minus depreciation). Accrual amount is then divided by average total assets for the previous two years;

LG = long-term EPS growth rate forecast submitted by analysts, measured at June of year *t*;

LTSG = sales growth rate over the past five years;

CHG_PPE = change within the property, plant, and equipment account over year *t*–1 divided by average total assets over the past two years;

CHG_OLTA = change within other long-term assets account over year *t*–1 divided by average total assets over the past two years;

QES = median consensus analyst forecast error for the most recent quarter divided by stock price;

ARET = annual return calculated as the stock return over the past 12 months prior to the calculation of forecast error; and

REV1, *REV2* = analyst forecast revision during the three months prior to the calculation of the forecast error divided by stock price.

The above equations are estimated separately using a pooled sample. The pooled sample is obtained by gathering all available forecast data for the five years prior to the fiscal year for which we predict the error. After estimating coefficients for the models using all available previous data, we apply these coefficients to our data to obtain predicted forecast error estimates for the one- and two-year-ahead forecasts. We then average the one- and two-year-ahead forecasts and use the resulting variable (*AVGPRED_ER*) within our analyses.

APPENDIX B
DEFINITION OF VARIABLES

Dependent Variables

CE_{CT} = Claus and Thomas (2001) as described within Appendix A;
CE_{GLS} = Gebhardt et al. (2001) as described within Appendix A;
CE_{GM} = Gode and Mohanram (2003) as described within Appendix A;
CE_{EAST} = Easton (2004) as described within Appendix A; and
CE_{HL} = average of the four measures discussed within Appendix A.

Independent Variables

MW1 = if the observation represents material weaknesses reported for the first time (no previous MW reported), then it is assigned an indicator variable of 1, 0 otherwise;
MW2 = if the observation represents material weaknesses reported in a second consecutive year, then it is assigned an indicator variable of 1, 0 otherwise;
MW3 = if the observation represents material weaknesses reported in a third consecutive year, then it is assigned an indicator variable of 1, 0 otherwise;
MW4 = if the observation represents material weaknesses reported in four or more consecutive years, then it is assigned an indicator variable of 1, 0 otherwise;
MKT_CAP = log of the share price at the end of the fiscal year multiplied by the number of common shares outstanding (log(Compustat Data item PRCF_F × Compustat Data item CSHO));
AGE_FIRM = log of the number of years the firm has been reporting within CRSP;
LOSS = sum of earnings before extraordinary items (Compustat Data item IB) in years *t* and (*t*–1). If this sum totals less than 0, then the firm is assigned an indicator variable of 1, 0 otherwise;
FCR = if the firm has a non-zero value associated with the foreign currency translation (Compustat data item FCA) item, then the observation is assigned an indicator variable of 1, 0 otherwise;
SL_GR = decile ranking of the year over year sales growth (Compustat data item SALE) for years *t*–2 through *t*; and
AVGPRED_ER = measured as the average of the predicted forecast bias measured for one- and two-year-ahead predicted forecasts as measured by Hughes et al. (2008), described in Appendix A.

Additional Independent Variables Used in Various Analyses

NW = the number of material weaknesses reported in each observation as identified by Audit Analytics;
Z_SCORE = measured as a decreasing decile ranking of the Altman (1968) Z-score;
LEV = leverage calculated as current liabilities plus long-term debt divided by total assets;

SEG = measured as the number of business segments reported in the Compustat Segments File;
$RESTRUCTURE$ = assigned an indicator variable of 1 if any of the Compustat restructuring items are different from 0, 0 otherwise;
$AVGPRED\_ER_{OSR}$ = measured using methodology of Ogneva et al. (2007); and
$AVGPRED\_ER_{GM}$ = measured using methodology of Gode and Mohanram (2010).

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The Impact of Mandatory IFRS Adoption on Audit Fees: Theory and Evidence

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ABSTRACT: This study examines the impact of International Financial Reporting Standards (IFRS) adoption on audit fees. We first build an analytical audit fee model to analyze the impact on audit fees for the change in both audit complexity and financial reporting quality brought about by IFRS adoption. We then test the model's predictions using audit fee data from European Union countries that mandated IFRS adoption in 2005. We find that mandatory IFRS adoption has led to an increase in audit fees. We also find that the IFRS-related audit fee premium increases with the increase in audit complexity brought about by IFRS adoption, and decreases with the improvement in financial reporting quality arising from IFRS adoption. Finally, we find some evidence that the IFRS-related audit fee premium is lower in countries with stronger legal regimes. Our results are robust to a variety of sensitivity checks.

Keywords: *IFRS adoption; audit fees; audit complexity; reporting quality; legal regime.*

Data availability: *Data are available from public sources identified in the paper.*

I. INTRODUCTION

Starting January 1, 2005, the European Parliament (Regulation No. 1606/2002) required companies listed on organized exchanges in European Union (EU) countries to prepare their consolidated financial statements using International Financial Reporting Standards

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(IFRS).[1] Since then, the accounting profession and the academic literature have paid considerable attention to the economic consequences of IFRS adoption. This study adds to the literature by examining whether and how IFRS adoption impacts audit fees.

A number of empirical studies examine whether the quality of financial reporting improves subsequent to IFRS adoption. Using an international sample from 21 countries, Barth et al. (2008) find that firms voluntarily adopting IFRS generally experience less earnings management, more timely loss recognition, and greater value-relevance than do matched sample firms applying local generally accepted accounting principles (GAAP). Chen et al. (2010) examine mandatory IFRS adoption of firms from 15 EU member states and find less earnings management toward a target, a lower magnitude of absolute discretionary accruals, and higher accruals quality after mandatory IFRS adoption. However, these authors and Ahmed et al. (2009) also suggest that firms engage in more earnings smoothing and recognize large losses in a less timely manner in post-IFRS adoption periods. Several other studies that examine the effects of voluntary or mandatory IFRS adoption on accounting quality within a single country provide mixed evidence (e.g., Eccher and Healy 2003; Bartov et al. 2005; Van Tendeloo and Vanstraelen 2005; Hung and Subramanyam 2007; Jeanjean and Stolowy 2008; Paananen and Lin 2009).

In addition to studies on the effect of IFRS adoption on financial reporting quality, another strand of research examines a broader range of economic consequences of IFRS adoption. Evidence indicates that IFRS adoption leads to lower cost of equity capital (Daske et al. 2009; Li 2010), higher market liquidity (Leuz and Verrecchia 2000; Drake et al. 2010), more investment flows through foreign mutual funds (Covrig et al. 2007), more favorable terms in private debt contracting (Kim et al. 2011), greater analyst coverage (Kim and Shi 2012b), and lower stock return synchronicity (Kim and Shi 2012a). Several studies (e.g., Comprix et al. 2003; Christensen et al. 2008; Pae et al. 2008; Armstrong et al. 2010) seek to address whether the benefits of IFRS adoption outweigh the associated costs by examining market reactions to the events leading to IFRS adoption. These studies yield mixed evidence on whether market participants perceive an overall net benefit from IFRS adoption.

The aforementioned studies shed light on the ongoing debate regarding the merits of adopting IFRS. However, very few studies directly examine the costs associated with IFRS adoption. Our study aims to provide systematic evidence on the cost side of mandate IFRS adoption, with a focus on audit costs. Specifically, we investigate whether the EU decision to mandate adoption of IFRS impacts fees paid to auditors for their financial statement audits (henceforth, audit fees). In addition, we examine the channels through which IFRS adoption leads to changes in audit fees.

Since Simunic (1980), many studies have examined cross-sectional determinants of audit fees within a country. These studies find that audit fees are primarily determined by client size, audit task complexity, and potential legal liability or litigation risk (e.g., Craswell et al. 1995; Simunic and Stein 1996). Our study extends the previous single-country studies to an international setting, where mandatory IFRS adoption by EU countries leads to a shift in disclosure regime. This shift implies not only a change in audit task complexity, but also a change in financial reporting quality across countries with different legal regimes. We take advantage of this unique setting to assess how such changes work together to affect audit fees.

To guide our hypothesis development and interpretation of empirical results, we first develop a simple analytical model in which audit task complexity, financial reporting quality, and legal regime play critical roles in the production and pricing of audit services. We then test the model's predictions concerning the relation between IFRS adoption and audit fees. To further understand the audit fee effect of IFRS adoption, we also test the model's predictions of how the relation between IFRS adoption and audit fees varies with a country's institutional factors. In so doing, we consider three types of institutional factors: (1) the increase in audit complexity arising from IFRS adoption, (2) the change in financial reporting quality brought about by IFRS adoption, and (3) the strength of a country's legal regime.

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[1] The last decade has witnessed a growing trend of IFRS adoption around the world, with more than 100 countries now requiring or permitting IFRS for their domestically listed companies. In 2002, the International Accounting Standards Board and the Financial Accounting Standards Board in the U.S. embarked on a joint program, known as the Norwalk Agreement, to make the U.S. GAAP and IFRS fully compatible.

For our empirical tests, we employ a difference-in-differences design to control for the general trend or changes in the economic environment unrelated to IFRS adoption. Specifically, we use as the control sample non-adopter firms from Organization for Economic Co-operation and Development (OECD) countries in which IFRS had not been mandated up to 2008. Our final sample consists of 2,860 firm-years from 11 EU countries in the treatment sample and 9,052 firm-years from three OECD countries (Japan, Canada, and the U.S.) in the control sample for which we have observations during both the pre-IFRS adoption period (hereafter, the pre-IFRS period) and the post-IFRS adoption period (hereafter, the post-IFRS period). Unless otherwise specified, we use the term *pre-IFRS period* to refer to $t-1$ and *post-IFRS period* to refer to $[t+1, t+3]$, where t is the adoption year.[2]

Our empirical results can be summarized as follows. First, we find that audit fees increase from the pre-IFRS period to the post-IFRS period for both the IFRS adopter firms in the treatment sample (EU countries) and the non-adopter firms in the benchmark control sample (non-EU OECD countries), but this fee increase is larger in magnitude for IFRS adopter firms than for non-adopter firms. More importantly, we find that the incremental fee increase for the adopter firms (over and above the audit fee change for the non-adopter firms) is highly significant at the 1 percent level. The above results, taken together, lead us to conclude that IFRS adoption causes a significant increase in audit fees. Second, we find that this audit fee increase for IFRS adopter firms from the pre-IFRS period to the post-IFRS period, which we refer to as the IFRS-related audit fee premium, is positively related to the increase in audit complexity brought about by IFRS adoption. Third, we find that the IFRS-related audit fee premium decreases with the improvement in financial reporting quality brought about by IFRS adoption. Fourth, we find some evidence that the IFRS-related audit fee premium decreases with the strength of a country's legal regime. The above results hold after controlling for the concurrent reforms in corporate governance and enforcement mechanisms. This suggests that the audit fee premium documented in this paper is primarily driven by IFRS adoption *per se*, rather than by concurrent reforms that may also affect audit fees.

This study makes several contributions to the literature. First, it is one of the few studies that directly examine the cost side of IFRS adoption with a focus on audit fees. A study conducted by the Institute of Chartered Accountants in England and Wales (ICAEW 2007) on behalf of the European Commission suggests that EU companies ranked increased audit costs as one of their largest IFRS-related costs. A small number of concurrent academic studies examine the effects of IFRS adoption on audit fees in single country settings (Degeorge et al. 2008; Griffin et al. 2009; Lin and Yen 2010; Vieru and Schadewitz 2010). These studies provide consistent evidence that audit fees increase in the post-IFRS period. Using audit fee data from 11 EU countries and employing a difference-in-differences approach, we provide large-sample evidence on the average magnitude of the audit fee premium associated with IFRS adoption. Specifically, we find that, on average, the audit fee increase is 5.44 percent greater for the adopter firms, compared to that for the non-adopter firms in our control sample. Moreover, our cross-country study sheds light on how the institutional features of different countries, including legal environments and pre-IFRS local GAAP characteristics, affect the audit fee premium associated with IFRS adoption. The findings of this paper should therefore be of substantial interest to regulators and policy makers, including those in countries that have not yet decided to move toward IFRS.

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[2] Since our focus is on mandatory IFRS adoption, we exclude firms that started IFRS reporting either prior to the mandatory adoption year ("voluntary adopters") or after the mandatory adoption year ("late adopters"). The EU mandated IFRS adoption for each financial year starting on or after January 1, 2005. Adoption year t in this study is 2005 (2006) for firms with a December (non-December) fiscal year-end. Note that the year to which Worldscope assigns financial data is the year in which the company's fiscal year ended. Section III provides a detailed discussion on our choice of pre- and post-IFRS windows.

Second, this study contributes to the audit-pricing literature. To the best of our knowledge, our study is the first to provide systematic evidence on the two channels of audit complexity and financial reporting quality through which accounting standards and their changes affect audit fees. More specifically, IFRS adoption involves not only an upward shift in audit complexity, which increases audit fees, but also a simultaneous improvement in financial reporting quality, which reduces audit fees. Our empirical results suggest that both factors play important roles in explaining cross-sectional variations in audit fee changes brought about by IFRS adoption.

The rest of the paper is organized as follows. Section II develops a theoretical audit fee model and formulates empirical hypotheses based on the model's predictions. Section III specifies empirical models for hypothesis testing. Section IV describes our sample and presents descriptive statistics. Section V reports our main empirical results, while Section VI presents additional sensitivity analyses. Section VII concludes.

II. THEORY AND HYPOTHESIS DEVELOPMENT

To gain insight into the effect of IFRS on audit fees, we build an audit fee model that is similar in spirit to the audit fee models in Liu and Simunic (2005) and Choi et al. (2008, 2009). For any given auditor, his or her objective (decision problem) is to choose the audit effort $e \in (0, 1)$ that minimizes total audit cost, which is the sum of the expected legal liability cost and the auditor's effort cost:

$$\underset{e \in (0,\,1)}{\text{Minimize}}\,\text{Total-Audit-Cost} = (1-q)[c(1-e)][r(1-e)]l + ke^2, \tag{1}$$

where:

- $q \in (0, 1)$ denotes the financial reporting quality conditional on a country's reporting regime (GAAP), which is defined as the probability of financial statements containing no misstatement conditional on the reporting regime;
- $c \in (0, 1)$ refers to the complexity of the audit;
- $e \in (0, 1)$ represents the auditor's effort;
- $r \in (0, 1)$ refers to the strength of a country's legal environment in determining the auditor's legal liability conditional on audit failure;
- $l > 0$ refers to the legal payment the auditor makes to the client if the auditor is found liable in court; and
- $k > 0$ is the auditor's effort cost parameter.

To interpret the economic meaning of the audit fee, first note that audit risk refers to the risk that an auditor fails to discover and report a material misstatement in a company's financial report, and is a function of material misstatement and detection risk (IAASB 2009). Consistent with this concept, audit risk in our model corresponds to $(1-q)[c(1-e)]$, where $(1-q)$ represents the risk of material misstatements, defined as the probability of client misreporting (i.e., failure to comply with GAAP), and $c(1-e)$ represents the detection risk, defined as the probability of the auditor's failure to discover and report misstatements *conditional on* the financial report being misstated. The detection risk is increasing in audit complexity c and decreasing in the auditor's effort e.

Second, note that the auditor's engagement risk represents the overall risk associated with an audit engagement. This engagement risk consists of two components, audit risk and litigation risk, and is formally represented by $[(1-q)c(1-e)][r(1-e)l]$ in our model. The litigation risk component, $r(1-e)$, is the probability of the auditor being held liable in court in the event of audit failure, which is increasing in the strength of the country's legal regime r and decreasing in the auditor's effort e. Hence, $[(1-q)c(1-e)][r(1-e)l]$ as a whole represents the auditor's *expected* legal liability associated with an audit engagement. Finally, ke^2 is the auditor's effort cost, which is increasing and convex in effort e.

The auditor's effort is determined by the first-order condition:

$$e^* = (1-q)crl/[(1-q)crl + k].$$

Specifically, the auditor's effort is a function of the reporting quality q, audit complexity c, and the strength of the legal regime r.

In a competitive audit market, the audit fee equals the total audit cost in equilibrium; that is:

$$f = (1-q)rc(1-e^*)^2 l + ke^{*2}.$$

Since $\partial f/\partial r = (1-q)c(1-e^*)^2 l > 0$, $\partial f/\partial c = (1-q)r(1-e^*)^2 l > 0$, and $\partial f/\partial q = -rc(1-e^*)^2 l < 0$, we obtain the following results:

Observation 1: The audit fee is increasing in the strength of the legal regime r and in audit complexity c, but decreasing in financial reporting quality q.

One implication of Observation 1 is that the audit fee decreases with the quality of financial reporting. The intuition behind this result is clear: given GAAP, the higher the quality of a financial report (which means less likelihood of financial misstatements), the lower the audit risk an auditor faces, which leads to a lower audit fee.

To examine how IFRS adoption changes the audit fee, we note that IFRS adoption has two audit-related effects. First, the adoption of IFRS may improve financial reporting quality, that is, reduce financial misstatements conditional on the reporting regime, thereby reducing audit risks. IFRS is generally considered to be superior to former local accounting standards. Proponents of IFRS claim that IFRS adoption leads to increased and higher quality disclosures that better reflect a firm's economic conditions. They argue that, compared with former local GAAP in most countries, IFRS is more principles-based, emphasizing a "true and fair view" in both letter and spirit, and incorporating into financial statements the effects of economic events on firm performance in a timelier manner (Coopers & Lybrand 1993; Dumontier and Raffournier 1998; Alexander and Archer 2001).[3] More specifically, the adoption of IFRS may reduce financial misstatements by improving management's accounting decisions and measurements. To the extent that IFRS fills voids in the local standards and provides comprehensive guidelines on accounting issues, it can lead to better judgments and, thus, fewer misstatements or better compliance with the fair presentation principle of GAAP to reflect the firm's underlying economics.[4] Moreover, accounting choices and judgment are often allowed for an accounting issue. IFRS provides more practical guidelines to limit opportunistic management discretion in determining accounting amounts, thus leading to fewer misstatements or better compliance with accounting standards that require professional judgment (Barth et al. 2008). In summary, the comprehensiveness and quality of IFRS guidelines have positive effects on reporting quality, as they improve management accounting decisions and reduce judgment errors in complying with GAAP, particularly the fair presentation principle. The improved financial reporting quality tends to reduce audit risk and the audit fee.

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[3] Skeptics, on the other hand, argue that IFRS adoption is unlikely to enhance accounting quality unless it is accompanied by a wide range of complementary changes in infrastructure (Ball 2006). In addition, the inherent flexibility in principles-based standards could provide more room for firms to manage earnings. This effect on client misreporting, however, can be mitigated by extensive practical guidelines to help firms comply with such a standard.

[4] In most countries, compliance with GAAP is not narrowly defined in the accounting framework as compliance only with specific accounting provisions. It also includes compliance with the fair presentation principle of GAAP. Managers/auditors cannot avoid litigation simply by blaming GAAP for lacking specific provisions or practical guidelines. Failure to fairly present the firm's economic conditions when judgment is required constitutes misstatements and violation of GAAP, subject to the court's decision.

Second, IFRS adoption increases the complexity of an audit. Since IFRS is comprehensive, fair-value oriented, and principles-based, using IFRS generally requires accountants and auditors to make more complex estimates and to use greater professional judgment (KPMG 2007; Deloitte 2008).[5] As shown in Observation 1, improved financial reporting quality reduces the audit fee, but increased audit complexity increases it. Therefore, the effect of IFRS adoption on audit fees is determined by which of the above two forces is dominant. Our analysis yields the following observation.

Observation 2: IFRS adoption, which increases both financial reporting quality q and audit complexity c, leads to an increase (decrease) in audit fees if the positive effect of the increase in audit complexity dominates (is dominated by) the negative effect of improved financial reporting quality on the audit fee. Specifically, IFRS adoption increases (decreases) the audit fee if $(1-q)\Delta c > c\Delta q$ (if $(1-q)\Delta c < c\Delta q$),where Δ denotes the change brought about by IFRS adoption.

Proof: See Appendix A.

Observation 2 indicates that IFRS adoption can lead to an increase or decrease in audit fees, depending on whether an increase in audit complexity or an increase in financial reporting quality is the dominant factor in determining the audit fee change.[6] Since we cannot specify *a priori* the direction of the impact of IFRS adoption on audit fees, we formulate the following null hypothesis:

H1: IFRS adoption has no impact on audit fees.

To facilitate the discussion below, we now summarize the subsequent results of empirical tests for H1. As reported later in Table 5, these results show that IFRS adoption leads to an increase in audit fees. According to Observation 2, these empirical results suggest that $(1-q)\Delta c > c\Delta q$ holds; that is, the positive effect of increased audit complexity on audit fees dominates the negative effect of improved reporting quality, leading to an IFRS-related increase in the audit fee or audit fee premium.

We next examine how this audit fee premium associated with IFRS adoption varies with a number of institutional factors. First, we examine the effect of increased audit complexity arising from IFRS adoption. We find that:

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[5] For a small subset of cross-listed clients, the increase in audit complexity associated with IFRS adoption may be partially offset by a benefit derived from reduced reconciliation to foreign countries' GAAP if the cross-listed foreign country has also adopted IFRS. Out of the 833 firms in our final treatment sample, 45 are identified as being cross-listed in foreign countries that also mandated IFRS before 2008. The untabulated results for our test variables remain qualitatively unchanged after excluding these 45 firms.

[6] While our discussion focuses on the situation in which IFRS leads to an improvement in financial reporting quality q, our analytical results (observations) encompass situations in which q can increase or decrease. If IFRS adoption causes deterioration in financial reporting quality, then the condition $(1-q)\Delta c > c\Delta q$ in Observation 2 always holds, which means that the IFRS-related audit fee change will always be positive and is attributable to both reduced financial reporting quality and increased audit complexity. H3 specifically addresses the relation between the audit fee change and the cross-country variation in the change of reporting quality brought about by IFRS adoption.

$$\partial \Delta f / \partial \Delta c = \partial [f(q_{IFRS}, c_i + \Delta c) - f(q_i, c_i)] / \partial \Delta c > 0,$$

where the subscript i refers to country i. This means that, *ceteris paribus*, the greater the increase in audit complexity (i.e., greater Δc), the greater the IFRS-related audit fee premium. Second, we examine how the IFRS-related audit fee premium varies with the change in financial reporting quality brought about by IFRS adoption. We find that:

$$\partial \Delta f / \partial \Delta q = \partial [f(q_i + \Delta q, c_{IFRS}) - f(q_i, c_i)] / \partial \Delta q < 0,$$

which means that, *ceteris paribus*, the greater the improvement in financial reporting quality brought about by IFRS adoption (i.e., greater Δq), the smaller the audit fee premium. We summarize these results as follows.

Observation 3: The audit fee premium associated with IFRS adoption is increasing with the increase in audit complexity arising from IFRS adoption, and decreasing with the improvement in financial reporting quality brought about by IFRS adoption.

Based upon the predictions in Observation 3, we test the following hypotheses.

H2: *Ceteris paribus*, the audit fee premium associated with IFRS adoption is increasing with the increase in audit complexity brought about by IFRS adoption.

H3: *Ceteris paribus*, the audit fee premium associated with IFRS adoption is decreasing with the improvement in financial reporting quality brought about by IFRS adoption.

Finally, we examine how the audit fee premium associated with IFRS adoption varies with a country's legal regime. In this analysis, we assume that $(1 - q)\Delta c > c\Delta q$, which means that the increase in audit complexity dominates any improvement in financial reporting in determining the audit fee change, since we observe an overall positive audit fee change after IFRS adoption (see the discussion following H1). We derive:

$$\partial df / \partial r = (\partial df / \partial r)|_{e=e*} + (\partial df / \partial e)(\partial e / \partial r).$$

The first component, $(\partial df / \partial r)|_{e=e*} > 0$, represents the direct positive effect of a stronger legal regime on the audit fee premium, *with effort level being fixed*. The intuition for the positive sign on this first component is that, when the auditor's effort is fixed, a stronger legal regime experiences a larger IFRS-related audit fee premium because the auditor faces greater legal liability in a stronger legal regime. The second component, $(\partial df / \partial e)(\partial e / \partial r) < 0$, represents the negative effect of greater auditor effort on the audit fee premium. The economic intuition for the negative sign on the second component is that auditors are induced to expend greater effort in a stronger legal regime, which has a negative effect on the auditor's expected legal liability cost and, thus, reduces the IFRS-related audit fee premium. To summarize, the effect of the legal regime on the IFRS-related audit fee premium, i.e., the sign of $\partial df / \partial r$, depends on which of the two forces mentioned above is dominant. Our analysis yields the following results:

Observation 4: Assume that $(1 - q)\Delta c > c\Delta q$ holds. The audit fee premium associated with IFRS adoption decreases (increases) with the strength of a country's legal regime if the incentive effect of a stronger legal regime on auditor effort is sufficiently large (small). Specifically, $\partial df / \partial r < 0 (\partial df / \partial r > 0)$ when the strength of the legal regime exceeds (falls below) a threshold, that is $r > k / cl(1 - q) (r < k / cl(1 - q))$.

Proof: See Appendix A.

Observation 4 shows that if the legal regime, r, is sufficiently strong, then the incentive effect on the auditor's expected liability costs becomes the dominant factor, leading to an overall reduction in the IFRS-related audit fee premium in a stronger legal regime. However, because the condition in Observation 4 is *a priori* unknown, we formulate the following hypothesis in null form:

> **H4:** The audit fee premium associated with IFRS adoption in countries with strong legal regimes is the same as that in countries with weak legal regimes.

III. EMPIRICAL PROCEDURES

Research Design for Testing H1

To assess the overall effect of mandatory IFRS adoption on audit fees (H1) for adopter firms in EU countries, we employ a difference-in-differences design, using non-adopter firms in selective non-EU countries as the benchmark. The difference-in-differences design is used to control for changes unrelated to IFRS adoption in the form of a general trend or concurrent changes in audit fees that may also be experienced by the non-adopter firms in the control sample.

We choose our benchmark control sample of firms from non-EU OECD countries that had not mandated IFRS as of 2008. Non-EU OECD countries have well-developed capital markets and are at a stage of economic development similar to those of EU countries, making them a reasonable control group for our tests. Specifically, we estimate the following regression model:

$$\begin{aligned} AUDFEE = {} & \beta_0 + \beta_1 POST + \beta_2 POST*TREAT + \sum \delta_k FSCONTROL_k + Industry\ Indicators \\ & + Country\ Indicators + \varepsilon, \end{aligned} \tag{2}$$

where:

$AUDFEE$ = natural log of the audit fee in thousands of euros;
$TREAT$ = 1 for firms in the treatment sample (i.e., mandatory adopter firms in EU countries), and 0 for firms in the benchmark control sample (i.e., non-adopter firms in non-EU OECD countries); and
$POST$ = 1 for the post-IFRS period, and 0 for the pre-IFRS period.

For firms in the control sample, we assign the pseudo-adoption year 2005 (2006) for firms with a December (non-December) fiscal year-end (to be consistent with the IFRS adoption pattern in the treatment firms). The term *FSCONTROL* denotes firm-specific control variables. Our model specification includes country-specific indicators, which fully control for *fixed* differences between treatment and control countries (Low 2009, 478; Bertrand and Mullainathan 2003, 1057). We also include industry fixed effects.

In Equation (2), the coefficient β_1 captures the audit fee change for non-adopter firms in the control sample from the pre-(pseudo)-adoption period to the post-(pseudo)-adoption period. The coefficient of interest is β_2, which captures the *incremental* change in audit fees from the pre-IFRS period to the post-IFRS period in the treatment sample relative to the change in the control sample. H1 in *null* form translates to predicting that $\beta_2 = 0$. Appendix B provides detailed definitions for all variables included in Equation (2).

We include 14 firm-specific controls (*LNTA*, *INVREC*, *NBS*, *NGS*, *MB*, *LOSS*, *LEV*, *QUICK*, *SPECIAL_ITEM*, *QUALIFIED*, *MERGE*, *FINANCE*, *BIG4*, and *CROSS*). The variables *LNTA* and *INVREC* are proxies for client size and client complexity, respectively (Simunic 1980; Francis 1984). Because operationally or geographically diversified firms may require more complex audits, we include the number of business segments (*NBS*) and the number of geographical segments

(*NGS*) as additional proxies for client complexity (Simunic 1980; Choi et al. 2008). Following the audit fee literature (Francis 1984; Seetharaman et al. 2002; Hay et al. 2006), we include *LOSS*, *LEV*, *QUICK*, *SPECIAL_ITEM*, and *QUALIFIED* to measure client-specific litigation risks borne by auditors. Similar to Ashbaugh et al. (2003), we include two indicator variables, *MERGE* and *FINANCE*, to capture the demand for additional audit and consulting services associated with business combinations and financing activities. We expect the coefficients for all the above firm-specific control variables except *QUICK* to be positive, since the literature shows that audit fees are positively related to client size, client complexity, and client-specific risk factors. We expect the coefficient of *QUICK* to be negative, since a low *QUICK* ratio is associated with higher financial risk (Francis 1984). The market-to-book ratio (*MB*) is included, but the relation between *MB* and audit fees is unclear, since it is associated with audit complexity and firm risk, as well as firm performance (Ashbaugh et al. 2003).

We include the Big 4 indicator variable (*BIG4*) to capture a Big 4 auditor fee premium (DeFond et al. 2000; Choi et al. 2008). Choi et al. (2009) find that auditors charge higher fees for firms that are cross-listed in countries with stronger legal regimes than they do for non-cross-listed firms. We therefore control for any cross-listing effect by including an indicator variable, *CROSS*, which equals 1 for cross-listed firms, and 0 otherwise.

To estimate Equation (2), we pool the pre- and post-IFRS data. One problem we face is that most of the firm-specific control variables are accounting-based measures. This means that, for IFRS adopter firms in our treatment sample, these variables are measured following local GAAP in the pre-IFRS period and IFRS in the post-IFRS period. This measurement difference may cause the coefficients on the accounting measures to differ between the pre- and post-IFRS periods. Thus, it could be problematic to pool data from pre- and post-IFRS periods. To address this measurement problem, we restrict our pre-IFRS period to year $t-1$ and use restated numbers under IFRS for accounting-based, firm-specific control variables, where year t is the adoption year. IFRS 1 (*First-Time Adoption of International Financial Reporting Standards*) requires firms to apply IFRS retrospectively to the year prior to IFRS adoption to establish comparative financial statements in year t. This regulatory requirement provides us with one year of data in the pre-IFRS era with accounting measures that are recalibrated by firms following IFRS.[7]

Our post-IFRS period covers three years $[t+1, t+3]$, excluding the adoption year t. We exclude the adoption year t in our main analysis to alleviate any potential concern that our results are driven by temporary transitional effects.

Research Design for Testing H2, H3, and H4

To test the impact of institutional factors on the audit fee premium associated with IFRS adoption (H2–H4), we estimate the following model:

$$\begin{aligned} AUDFEE = {} & \beta_0 + \beta_1 POST + \beta_2 POST*TREAT + \beta_3 POST*TREAT*\Delta COMPLEXITY \\ & + \beta_4 POST*TREAT*\Delta QUALITY + \beta_5 POST*TREAT*Legal\ Regime \\ & + \sum \delta_k FSCONTROL_k + Industry\ Indicators + Country\ Indicators + \varepsilon, \end{aligned} \qquad (3)$$

where *TREAT* and *POST* are as defined in Equation (2) and $\Delta COMPLEXITY$ refers to the change (increase) in audit complexity arising from IFRS adoption. To obtain an empirical proxy for

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[7] We thank an anonymous referee for bringing this measurement issue to our attention. Another benefit of starting the sample period from $t-1$ is to avoid the effect of SOX-related reforms on the audit fee change in the control sample. The untabulated results suggest a large increase in audit fees from $t-2$ to $t-1$ in the U.S. sample, which is likely due to SOX Section 404(b), which requires auditors to express opinions on client firms' internal controls for fiscal years ending on or after November 15, 2004.

$\Delta COMPLEXITY$, we first measure the extent to which (pre-IFRS) local GAAP deviates from IFRS. Since IFRS is more comprehensive, principles-based, and fair-value-oriented, it involves more complex planning, tests, and judgment on the part of auditors. As such, a large deviation between IFRS and local GAAP implies that IFRS adoption causes a large increase in audit complexity.

Ding et al. (2007) classify and quantify the difference between local GAAP and IFRS along two dimensions, *Absence* and *Divergence*. The *Absence* score is based on the number of accounting rules regarding particular accounting issues that are missing in (pre-IFRS) local GAAP but that are explicitly stipulated in IFRS. The *Divergence* score is based on the number of accounting rules regarding the same accounting issue that differ between IFRS and (pre-IFRS) local GAAP. In this study, $\Delta COMPLEXITY$ is computed as the natural log of the sum of the *Absence* and *Divergence* scores, since both lead to an increase in audit complexity. H2 states that the audit fee premium associated with IFRS adoption is increasing with the change in audit complexity brought about by IFRS adoption, which translates to a positive coefficient for $POST * TREAT * \Delta COMPLEXITY$ (i.e., $\beta_3 > 0$).

The variable $\Delta QUALITY$ refers to the change in financial reporting quality brought about by IFRS adoption. We use three different measures to empirically capture $\Delta QUALITY$. Our first measure is based on the previously described comparison of IFRS versus pre-IFRS local GAAP. Although IFRS deviates from local GAAP along the two dimensions of *Absence* and *Divergence*, for reasons we describe below, our measure of the change in reporting quality focuses only on the *Absence* dimension. Specifically, we use the natural log of $(1 + Absence)$, denoted $\Delta GAAPQ$, to measure the change (improvement) in GAAP quality brought about by IFRS adoption.[8] Since a high *Absence* score implies a less developed pre-IFRS local GAAP, *ceteris paribus*, a higher level of *Absence* in the pre-IFRS period implies a greater improvement in reporting quality in the post-IFRS period.

We do not include *Divergence* when measuring the change in reporting quality associated with IFRS adoption because *Divergence* does not have a clear implication for whether domestic GAAP is superior or inferior to IFRS (Ding et al. 2007).[9] Given the ambiguity in the relation between *Divergence* and reporting quality, we construct two discretionary accrual measures of the change in reporting quality brought about by IFRS adoption. Prior studies (e.g., Jones et al. 2008) identify discretionary accruals as an important red flag for material misstatement and fraud, which are particularly relevant to the duties of an auditor. The first discretionary accrual measure, denoted *DA1*, is the residual from the cross-sectional Jones (1991) model. The second measure, denoted *DA2*, is the residual from the cross-sectional Dechow and Dichev (2002) model augmented by the fundamental variables from the modified Jones models. More specifically, we measure $\Delta QUALITY$ through $\Delta|DA1|$ and $\Delta|DA2|$, that is, the changes in the country's median values of the absolute discretionary accruals from the pre-IFRS period ($t-1$) to the post-IFRS period ($[t+1, t+3]$). In constructing these two variables, we use only observations from our final audit fee sample. We multiply $\Delta|DA1|$ and $\Delta|DA2|$ by -1 so that higher values indicate greater improvements in financial reporting quality. Appendix C provides more detail on the construction of *DA1* and *DA2*.

In summary, we measure the reporting quality changes, $\Delta QUALITY$ in Equation (3), using three different proxies: $\Delta GAAPQ = ln(1 + Absence)$, $(-1) * \Delta|DA1|$, and $(-1) * \Delta|DA2|$. H3 predicts that the IFRS-related audit fee premium decreases with the improvement in financial reporting quality brought about by IFRS adoption, which translates to a negative coefficient for $POST * TREAT * \Delta QUALITY$ (i.e., $\beta_4 < 0$).

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[8] We use $-ln(1 + Absence)$ to proxy for GAAP quality. Since *Absence* becomes 0 in the post-IFRS period, the change in GAAP quality is captured by $0 - [-ln(1 + Absence)] = ln(1 + Absence)$.

[9] On the one hand, one can argue that the international standard-setting process is intended to design a set of internationally acceptable high-quality financial reporting standards. Hence, IFRS should be generally superior to local standards, and any deviation may be viewed as an indication of low-quality reporting. On the other hand, one can argue that local accounting standards that deviate from IFRS are more customized to the local business practices and legal environment and, hence, provide better information to local stakeholders (Ding et al. 2007).

Finally, we examine the impact of legal regime on the IFRS-related audit fee premium. The strength of legal regime in Equation (3) is first proxied by *WINGATE*, which is the natural log of the Wingate (1997) litigation index. The Wingate index has been widely used in prior audit fee studies (e.g., Choi et al. 2008, 2009), and is based on surveys of partners in auditing firms regarding auditor litigation risk and, hence, captures those dimensions of a legal environment that are most relevant to auditing practices. However, the Wingate index may not capture recent changes in the legal environment. We therefore employ two additional measures of the strength of legal regimes. We include a more recent measure, *ENFORCE*, which refers to the aggregate public enforcement index from La Porta et al. (2006). In addition, La Porta et al. (1998) document that common law countries generally have stronger legal protection for investors than civil law countries. We therefore include an indicator variable, *LAW*, which equals 1 for countries with a common law legal origin, and 0 otherwise. We consider *LAW* and *ENFORCE* together in our regression analyses to comprehensively capture the legal environment in which managers operate their businesses and auditors perform their professional services.

In summary, legal regime in Equation (3) is measured by two proxies: (1) *WINGATE* and (2) both *LAW* and *ENFORCE*. Our null hypothesis, H4, translates to a prediction that $\beta_4 = 0$.

IV. SAMPLE AND DESCRIPTIVE STATISTICS

We initially obtain from Worldscope a sample of 29,206 firm-years from 14 EU countries over the period 2004–2008. To test our hypotheses, we exclude (1) firm-year observations for which information about the chosen accounting standards is missing, ambiguous, or coded as U.S. GAAP; (2) firms that belong to banking, insurance, or other financial industries (with a Worldscope general industry classification of 04, 05, or 06); (3) firms that adopted IFRS prior to the mandatory adoption year (voluntary adopters); (4) firms that adopted IFRS after the mandatory adoption year (late adopters); (5) firm-year observations with missing audit fee data; (6) firm-year observations lacking the data required to compute other firm-specific control variables; (7) firms without sufficient information to clearly identify their IFRS adoption year; and (8) firm-year observations falling outside our sample period of [*t*–1, *t*+3]. Finally, we require each firm to have data in the adoption year *t*, the pre-adoption year *t*–1, and at least one observation in the post-IFRS years [*t*+1, *t*+3] to minimize the impact of changes in sample composition on our results. The final treatment sample consists of 3,693 (2,860) firm-year observations for 11 EU countries if we include (exclude) the adoption year *t*.

Next, to construct our control sample, we begin with firm-year observations from non-EU member countries in the OECD that had not mandated IFRS adoption up to 2008. This requirement leads to an initial sample of 73,114 observations over the period 2004–2008 from seven countries (Canada, Chile, Japan, Mexico, South Korea, Turkey, and the U.S.). We follow sample-selection procedures similar to those used for the treatment sample. The final control sample consists of 11,903 (9,052) firm-year observations from three countries (Japan, Canada, and the U.S.) if we include (exclude) the pseudo-adoption year *t*.[10] Table 1 provides detailed information on our sample selection.

Table 2 reports the pattern of IFRS adoption over the period 2005–2008 across 14 EU countries. These statistics are based on a comprehensive EU sample before we impose the non-missing data requirements for audit fees and firm-specific control variables. We use data field 07536 in Worldscope to identify accounting standards followed by each firm in each year. This data

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[10] Seven OECD countries had not mandated IFRS adoption as of 2008. Audit fee information is mostly unavailable for Chile, Mexico, South Korea, and Turkey, which leaves three countries in the final control sample. The relatively small number of observations for Japan is due to limited audit fee data prior to 2007.

TABLE 1
Sample Selection[a,b,c]

	Treatment Sample (EU Countries)	Control Sample (OECD Countries)
Initial sample (firm-year observations) from Worldscope, 2004–2008	29,206	73,114
Subtract:		
Firm-year observations for which information about the chosen accounting standard is missing or ambiguous	(758)	
Firms that belong to banking, insurance, or other financial industries	(6,244)	(10,584)
Firms that started IFRS reporting prior to the mandatory adoption year (voluntary adopters)	(3,332)	(344)
Firms that started IFRS reporting after the mandatory adoption year (late adopters)	(6,360)	
Firm-year observations with missing audit fee data	(5,645)	(31,661)
Firm-year observations lacking data required to compute firm-specific control variables	(793)	(7,170)
Firms without sufficient information to identify their IFRS adoption year *t*	(488)	
Firm-year observations falling outside our sample period of [$t-1$, $t+3$]	(336)	(1,303)
Firms that do not have data available in both the pre- and post-adoption periods	(1,557)	(10,149)
Final sample including adoption year *t*	3,693	11,903
Final sample excluding adoption year *t*	2,860	9,052

[a] The initial sample consists of 14 EU countries in the treatment group and seven countries in the control group (Canada, Chile, Japan, Mexico, South Korea, Turkey, and the U.S.). The countries in the control group are selected using the following two criteria: (1) the country belongs to the OECD, and (2) the country had not mandated IFRS adoption up to 2008. Further sample-selection procedures result in a final sample of 11 EU member countries and three OECD countries. See Table 3 for details of the final sample composition.

[b] The year to which Worldscope assigns financial data is the year in which the company's fiscal year ended. Thus, the mandatory adoption year for firms with a December (non-December) fiscal year-end is 2005 (2006). We assign the pseudo-adoption year of 2005 (2006) for firms with a December (non-December) fiscal year-end in the control sample.

[c] Since the control sample has not mandated IFRS up to 2008, firms in the control sample are identified as voluntary adopters if they choose IFRS in any year in the sample period.

field includes a total of 23 categories. In the main test, we follow Daske et al. (2009) to use the strict coding for IFRS adoption, which consists of only Worldscope categories 02 ("International Standards") and 23 ("IFRS").[11]

As shown in Table 2, the adoption of IFRS by the end of 2006 is incomplete, with the U.K. having the lowest adoption rate (45 percent), followed by Ireland (63 percent). The reason is that Regulation No. 1606/2002 of the European Parliament allowed the following types of firms to postpone IFRS adoption: (1) firms listed on less-regulated markets (e.g., the Alternative Investment Market in London); (2) firms reporting non-consolidated reports; (3) firms publicly traded in a

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[11] The results are qualitatively similar when we use an alternative coding by Jeanjean and Stolowy (2008) or a comprehensive, less strict coding following Daske et al. (2009, Table 1A, Panel A).

TABLE 2

Distribution of IFRS Adoption 2005–2008

	2005		2006		2007		2008	
Country	**# of Firms**	**% of IFRS**	**# of Firms**	**% of IFRS**	**# of Firms**	**% of IFRS**	**# of Firms**	**% of IFRS**
Austria	19	74%	20	75%	19	84%	17	94%
Belgium	77	70%	78	77%	74	80%	52	88%
Denmark	92	59%	101	81%	103	82%	97	87%
Finland	107	93%	105	100%	101	100%	97	100%
France	651	59%	639	74%	574	78%	461	87%
Germany	376	54%	387	66%	369	70%	289	78%
Greece	250	96%	251	100%	244	100%	219	100%
Ireland	58	43%	59	63%	58	88%	44	100%
Italy	203	97%	224	100%	215	100%	200	100%
The Netherlands	114	93%	114	99%	107	98%	90	99%
Portugal	45	84%	44	86%	40	90%	40	93%
Spain	109	85%	110	93%	105	96%	104	95%
Sweden	318	75%	339	85%	346	85%	329	87%
U.K.	1,540	20%	1,642	45%	1,586	69%	1,326	97%

This table reports the percentage of firms that adopt IFRS during the period of 2005–2008 across 14 EU countries. The initial sample obtained from the Worldscope database consists of 29,206 firm-year observations from 2004 to 2008. We delete (1) firm-year observations with accounting standard information missing, ambiguous, or coded as U.S. GAAP, (2) firms that belong to the banking, insurance, or other financial industries (Worldscope general industry classification of 04, 05, or 06), and (3) firms that started IFRS reporting prior to the mandatory adoption year (voluntary adopters). These selection procedures result in 18,872 firm-year observations for the period 2004–2008, of which 15,378 observations fall in 2005–2008.

non-EU country that uses internationally accepted standards; and (4) firms with only publicly traded debt securities. By the end of 2008, almost all EU publicly listed firms had adopted IFRS.

Table 3 provides descriptive statistics of audit fees and firm characteristics. Panel A of Table 3 presents the mean values of the variables included in Equation (2) for each country and the grand means for the treatment and control samples.[12] We also report the mean for the pre- and post-IFRS periods, separately, for the treatment and control samples. The panel shows that the number of observations varies across countries, with only four observations for Germany and 1,524 observations for the U.K.[13] The sub-period statistics reveal that both the treatment and control

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[12] We use the 2008 data for the following four variables: the number of business segments (*NBS*), the number of geographical segments (*NGS*), auditor identity (*BIG4*), and cross-listing status (*CROSS*). Worldscope provides only the most recent year (i.e., 2008) information on *BIG4* and *CROSS*. As for *NBS* and *NGS*, Worldscope does not have complete time-series data (with a code of N.A. for at least one firm-year) for 38 percent of our sample firms. More importantly, the over-time variation of the number of segments could be due simply to the change in segment reporting regulations during the 2002–2008 period, rather than actual change in segments. For example, the revised International Accounting Standard 14 on segment reporting adopted by the EU in 2003 is based on the so-called risks and rewards approach, whereas IFRS 8 on segment reporting, adopted by the EU in 2007, is based on the so-called management approach.

[13] Audit fee data are mostly missing prior to 2005 for firms in Germany, which explains the small number of observations from Germany in the final sample after we require firms to have audit fee information both before and after IFRS adoption. Austria, Greece, and Italy drop out of the sample due to missing audit fee data.

TABLE 3

Firm-Level Descriptive Statistics

Panel A: Sample Characteristics by Country

Country	n	AUD-FEE	LNTA	INV-REC	NBS	NGS	MB	LOSS	LEV	QUICK	SPECIAL_ITEM	QUAL-IFIED	MERGE	FI-NANCE	BIG4	CROSS
Treatment Sample																
Belgium	12	4.72	12.21	0.47	0.77	1.13	1.56	0.42	0.66	0.72	0.25	0.00	0.00	0.25	0.33	0.00
Denmark	185	6.12	12.29	0.39	0.95	1.03	3.03	0.17	0.53	1.42	0.08	0.00	0.24	0.15	0.85	0.00
Finland	80	6.12	12.79	0.35	1.25	1.42	2.67	0.10	0.54	1.18	0.09	0.00	0.44	0.14	0.88	0.04
France	261	6.99	13.61	0.35	0.76	0.90	2.44	0.16	0.60	1.15	0.12	0.00	0.22	0.26	0.49	0.08
Germany	4	5.10	11.73	0.43	1.61	1.10	0.90	0.00	0.33	1.50	0.00	0.00	0.00	0.00	0.00	0.00
Ireland	95	6.34	12.96	0.27	0.97	0.99	3.56	0.18	0.51	1.55	0.12	0.11	0.21	0.18	0.89	0.79
The Netherlands	24	8.04	14.56	0.28	1.11	1.52	3.34	0.00	0.60	0.80	0.29	0.00	0.50	0.17	1.00	0.17
Portugal	42	6.31	14.30	0.22	1.12	0.91	2.55	0.10	0.66	0.83	0.07	0.05	0.21	0.24	0.83	0.55
Spain	235	6.61	14.31	0.32	1.26	1.16	3.69	0.09	0.61	0.93	0.03	0.01	0.10	0.31	0.94	0.20
Sweden	398	6.43	12.77	0.36	1.14	1.35	3.00	0.18	0.51	1.46	0.08	0.00	0.36	0.19	0.98	0.10
U.K.	1,524	6.57	12.91	0.30	1.10	1.10	3.07	0.17	0.54	1.35	0.14	0.01	0.34	0.19	0.83	0.07
Mean (total)	2,860	6.54	13.06	0.32	1.08	1.12	3.04	0.16	0.55	1.30	0.11	0.01	0.30	0.20	0.83	0.11
Pre [$t-1$]	833	6.33	12.76	0.33	1.06	1.10	3.23	0.17	0.54	1.40	0.10	0.01	0.32	0.16	0.83	0.11
Post [$t+1$, $t+3$]	2,027	6.63	13.18	0.32	1.08	1.13	2.96	0.16	0.55	1.27	0.12	0.01	0.29	0.22	0.83	0.11
Control Sample																
Canada	723	5.74	11.99	0.18	0.88	0.98	3.21	0.45	0.38	3.47	0.09	0.01	0.21	0.35	0.87	0.22
Japan	113	5.72	12.89	0.31	0.76	0.98	2.51	0.04	0.38	2.01	0.01	0.00	0.01	0.18	0.97	0.20
U.S.	8,216	6.45	12.32	0.26	0.93	0.88	3.67	0.33	0.45	2.39	0.13	0.04	0.23	0.21	0.65	0.01
Mean (total)	9,052	6.38	12.30	0.25	0.93	0.89	3.61	0.33	0.44	2.48	0.13	0.03	0.23	0.22	0.68	0.03
Pre [$t-1$]	2,851	6.22	12.03	0.26	0.92	0.89	4.20	0.32	0.43	2.62	0.13	0.04	0.32	0.20	0.67	0.03
Post [$t+1$, $t+3$]	6,201	6.46	12.42	0.25	0.93	0.89	3.35	0.34	0.45	2.41	0.13	0.03	0.19	0.23	0.68	0.03

(continued on next page)

TABLE 3 (continued)

Panel B: Correlations Matrix

	AUD-FEE	*LNTA*	*INV-REC*	*NBS*	*NGS*	*MB*	*LOSS*	*LEV*	*QUICK*	*SPECIAL _ITEM*	*QUAL-IFIED*	*MERGE*	*FI-NANCE*	*BIG4*
LNTA	**0.83**													
INVREC	0.00	**−0.07**												
NBS	**0.28**	**0.31**	**0.06**											
NGS	**0.35**	**0.32**	**0.09**	**0.18**										
MB	**−0.10**	**−0.17**	**−0.09**	**−0.07**	**−0.03**									
LOSS	**−0.27**	**−0.40**	**−0.15**	**−0.16**	**−0.15**	**0.12**								
LEV	**0.36**	**0.42**	**0.17**	**0.24**	**0.15**	**0.14**	**−0.11**							
QUICK	**−0.28**	**−0.30**	**−0.27**	**−0.16**	**−0.14**	**0.04**	**0.23**	**−0.53**						
SPECIAL_ITEM	**0.20**	**0.18**	**−0.04**	**0.12**	**0.05**	**−0.05**	**−0.01**	**0.11**	**−0.07**					
QUALIFIED	**−0.20**	**−0.24**	**−0.08**	**−0.07**	**−0.07**	**0.17**	**0.18**	−0.02	**0.03**	*−0.02*				
MERGE	**0.22**	**0.21**	0.01	**0.12**	**0.13**	**−0.03**	**−0.14**	**0.09**	**−0.11**	**0.04**	**−0.05**			
FINANCE	**−0.12**	**−0.12**	**−0.08**	**−0.05**	**−0.05**	**0.08**	**0.17**	0.02	**0.05**	**−0.05**	**0.09**	**−0.30**		
BIG4	**0.50**	**0.56**	**−0.07**	**0.13**	**0.21**	**−0.07**	**−0.22**	**0.17**	**−0.12**	**0.07**	**−0.18**	**0.12**	**−0.08**	
CROSS	**0.14**	**0.18**	**−0.03**	**0.07**	**0.09**	−0.01	**−0.05**	**0.05**	**−0.02**	0.01	−0.01	0.02	0.01	**0.10**

Panel A reports the sample mean for each country and the grand means for the treatment and control samples. The final sample consists of 2,860 firm-year observations from 11 EU countries in the treatment group and 9,052 firm-year observations from three OECD countries in the control group. Data from the IFRS adoption year *t* in the treatment sample and the pseudo-adoption year in the control sample are excluded. To avoid the undue influence of outliers, we winsorize all variables (except for variables with natural lower or upper bounds) at the 1st and 99th percentiles.

Panel B reports the Pearson correlations. Correlation coefficients are bold (italic) if they are statistically significant at the 1 percent (5 percent) level.

See Appendix B for variable definitions.

samples experience an increase in audit fees during the sample period, indicating the importance of controlling for the general trend or the concurrent changes in audit fees.[14]

Panel B of Table 3 presents the Pearson correlation coefficients for the audit fee and firm-specific control variables used in our regression analysis. Consistent with prior studies, audit fees (*AUDFEE*) are highly correlated with all variables representing client size, client complexity, and client-specific risk, except for a complexity variable, *INVREC*.

Panel A of Table 4 presents descriptive statistics for our key institutional variables. As shown in the panel, $\Delta COMPLEXITY$ and $\Delta GAAPQ$ vary significantly across our sample countries, providing us with a reasonable cross-country setting in which to assess the impact of these country-level variables on audit fees. The two accrual-based measures of $\Delta QUALITY$, namely $\Delta|DA1|$ and $\Delta|DA2|$, further indicate that the change in financial reporting quality brought about by IFRS adoption is not homogeneous across countries. For example, of the nine countries that have sufficient data to construct $\Delta|DA1|$ measures, only five reveal negative signs on this measure. The negative values of $\Delta|DA1|$ and $\Delta|DA2|$ indicate a decrease in the magnitude of discretionary accruals, reflecting an improvement in financial reporting quality. *WINGATE*, *LAW*, and *ENFORCE* also vary across countries, suggesting that the strength of legal regime differs across EU countries in our treatment sample.

Panel B of Table 4 presents Pearson correlation coefficients between key institutional variables. The variable $\Delta GAAPQ$ is significantly positively correlated with $\Delta COMPLEXITY$. This is not surprising, since both measures contain the *Absence* score, which captures the development of a country's pre-IFRS local GAAP. $\Delta GAAPQ$ is negatively correlated with *WINGATE* and *LAW*, consistent with the view that countries with weak legal regimes tend to have less comprehensive (poorer) pre-IFRS local standards. Since $\Delta|DA1|$ inversely measures the improvement in financial reporting quality, the negative correlations between $\Delta GAAPQ$ and $\Delta|DA1|$, although only suggestive of the underlying relation, indicate that IFRS adoption is associated with a more significant improvement in financial reporting quality in countries with poorer pre-IFRS accounting standards. As expected, the two accrual-based reporting quality change measures, $\Delta|DA1|$ and $\Delta|DA2|$, are significantly and positively correlated with each other. Finally, we note that *LAW* and *ENFORCE* are not significantly correlated with each other, suggesting that they capture different aspects of the strength of legal regime.

V. REGRESSION RESULTS

Test of H1

H1 is concerned with the overall impact of IFRS adoption on audit fees. To test H1, we estimate Equation (2) and report the results in Column (1) of Table 5. Throughout this paper, all reported t-statistics in parentheses are adjusted using robust standard errors corrected for firm-level clustering. All regressions are estimated after removing outliers with absolute studentized residuals greater than 3. As a result, the actual sample size varies slightly across regressions.

As shown in Table 5, Column (1), the coefficient of *POST* is positive and weakly significant at the 10 percent level ($\beta_1 = 0.019$; t $= 1.89$). This suggests that non-adopter firms in the control

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[14] Worldscope auditor fee data also include nonaudit fees paid to auditors. We note that the SOX-induced significant declines in the provision of nonaudit services had occurred before IFRS adoption (during 2002–2004), and surveys of U.S. and U.K. audit fees (including the audit fee trend report from Audit Analytics in the U.S. and the Audit Fees Survey from the *Financial Director* magazine in the U.K.) indicate that in both countries the declining trend continued but at a slower pace after 2004, and that nonaudit fees leveled off in 2007 and 2008. Insofar as the decline in nonaudit services is a global trend, our difference-in-differences design mitigates the potential confounding effect of nonaudit fees. Nonetheless, we acknowledge this data limitation.

TABLE 4

Country-Level Descriptive Statistics[a,b]

Panel A: Descriptions of Country-Level Variables

		ΔQUALITY			Legal Regime		
Country	*ΔCOMPLEXITY*	*ΔGAAPQ*	*Δ|DA1|*	*Δ|DA2|*	*WINGATE*	*LAW*	*ENFORCE*
Belgium	3.99	3.14	NA	NA	1.57	0	0.15
Denmark	3.95	3.47	−0.037	−0.024	1.57	0	0.37
Finland	3.97	3.14	−0.005	−0.010	1.28	0	0.32
France	4.01	3.09	−0.007	−0.023	1.83	0	0.77
Germany	4.03	2.94	−0.028	NA	1.83	0	0.22
Ireland	3.53	0.00	0.027	0.030	1.83	1	0.37
The Netherlands	3.56	2.40	0.018	0.011	1.83	0	0.47
Portugal	3.93	3.40	NA	NA	1.28	0	0.58
Spain	4.04	3.37	0.003	0.013	1.57	0	0.33
Sweden	3.58	2.40	−0.004	0.001	1.57	0	0.50
U.K.	3.56	0.00	0.000	−0.001	2.30	1	0.68

Panel B: Correlations Matrix

	ΔCOMPLEXITY	*ΔGAAPQ*	*Δ|DA1|*	*Δ|DA2|*	*WINGATE*	*LAW*
ΔGAAPQ	0.80***					
Δ|DA1|	−0.65*	−0.59*				
Δ|DA2|	−0.59	−0.57	0.87**			
WINGATE	−0.51	−0.72**	0.21	0.19		
LAW	−0.65**	−0.96***	0.49	0.50	0.66**	
ENFORCE	−0.32	−0.26	0.19	−0.36	0.36	0.24

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a] Refer to Appendix B for variable definitions, and Appendix C for details of the construction of *Δ|DA1|* and *Δ|DA2|*.

[b] "NA" indicates a missing value due to insufficient data to construct the measure.

sample experienced an average 1.92 percent increase in audit fees from year *t*–1 to years [*t*+1, *t*+3]. The coefficient of *POST* ∗ *TREAT*, which captures the incremental fee-increasing effect associated with IFRS adoption in the treatment sample, is significantly positive ($\beta_2 = 0.052$; t = 2.61). These results suggest that a representative firm in countries that mandated IFRS adoption experienced an average 7.36 percent increase in audit fees during the post-IFRS period;[15] this fee increase is 5.44 percent greater than the fee increase that an average non-adopter firm in the benchmark control sample experienced over the same period. These results lead us to reject the null hypothesis of no audit fee impact (H1) in favor of a significant increase in audit fees in the post-IFRS period.

The coefficients of firm-specific control variables are mostly significant with expected signs at the 1 percent level (except *LEV*, *QUALIFIED*, and *FINANCE*). These results are consistent with the findings in prior literature that audit fees are positively associated with client size (*LNTA*), client complexity (*INVREC*, *NBS*, and *NGS*), and client-specific risks (*LOSS*, the inverse of *QUICK*, and

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[15] The percentage change in audit fees from the pre-IFRS period to the post-IFRS period in the treatment group (7.36 percent) can be obtained by applying exp $(\beta_1 + \beta_2) - 1$.

TABLE 5

Results of Multivariate Regressions for Testing H1[a,b]

Panel A: Test Variables

Variables (Pred. Sign)	(1) $t-1$ versus $[t+1, t+3]$	(2) $t-1$ versus $[t, t+3]$
POST (β_1)	0.019*	0.019*
	(1.89)	(1.82)
POST $*$ *TREAT* (β_2)	0.052***	0.052***
	(2.61)	(2.63)
ADOPT (β_3)		−0.009
		(−1.06)
ADOPT $*$ *TREAT* (β_4)		0.073***
		(4.34)

Panel B: Firm-Specific Control Variables (*FSCONTROL*)

Variables (Pred. Sign)	(1) $t-1$ versus $[t+1, t+3]$	(2) $t-1$ versus $[t, t+3]$
LNTA (+)	0.563***	0.565***
	(86.89)	(88.58)
INVREC (+)	0.215***	0.220***
	(4.07)	(4.20)
NBS (+)	0.078***	0.074***
	(4.39)	(4.19)
NGS (+)	0.237***	0.238***
	(13.09)	(13.24)
MB (?)	0.011***	0.011***
	(6.27)	(6.62)
LOSS (+)	0.244***	0.242***
	(14.28)	(14.51)
LEV (+)	0.061	0.057
	(1.19)	(1.13)
QUICK (−)	−0.020***	−0.020***
	(−5.89)	(−6.23)
SPECIAL_ITEM (+)	0.164***	0.168***
	(8.31)	(8.97)
QUALIFIED (+)	0.061	0.053
	(1.25)	(1.06)
MERGE (+)	0.085***	0.080***
	(5.58)	(5.74)
FINANCE (+)	−0.018	−0.008
	(−1.16)	(−0.58)
BIG4 (+)	0.266***	0.261***
	(10.82)	(10.72)

(continued on next page)

TABLE 5 (continued)

Variables (Pred. Sign)	**(1)** ***t*–1** **versus** **[*t*+1, *t*+3]**	**(2)** ***t*–1** **versus** **[*t*, *t*+3]**
CROSS (+)	0.335***	0.325***
	(6.27)	(6.11)
Intercept	−2.878***	−2.837***
	(−9.44)	(−9.03)
Country dummies	Included	Included
Industry dummies	Included	Included
F-statistic for testing $\beta_1 + \beta_2 = 0$	16.82	
	($p < 0.01$)	
F-statistic for testing $\beta_2 = \beta_4$		1.78
		($p = 0.18$)
Observations	11,825	15,484
Adjusted R^2	0.85	0.85

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

[a] Observations in year *t* are excluded in Column (1) and included in Column (2), where *t* is the IFRS adoption year for the treatment sample and a pseudo-adoption year for the control sample, and *ADOPT* equals 1 for the adoption year, and 0 otherwise. Refer to Appendix B for the definitions of the other variables.

[b] Observations with absolute studentized residuals greater than 3 are deleted. The reported t-statistics are based on standard errors clustering on the firm dimension.

SPECIAL_ITEM). We find that the market-to-book ratio (*MB*) is positively associated with audit fees, consistent with the notion that firms with high *MB* values are associated with greater audit complexity or high client-specific risk. We also find that *MERGE* is positively associated with audit fees, suggesting that mergers create demand for additional audit services. Finally, there exist audit fee premiums for Big 4 client firms and cross-listed companies, which is consistent with evidence reported in Choi et al. (2008, 2009).

To focus on the permanent effect of IFRS adoption on audit fees, we have excluded the initial IFRS adoption year *t* in the main test, since the initial adoption year may experience a temporary audit fee spike. Specifically, it takes time and effort for auditors to learn about new IFRS rules, but this learning effect is likely to become insignificant after the initial year of IFRS adoption. Besides, in the initial adoption year, firms are required to apply IFRS retrospectively to one year prior to IFRS adoption for the purpose of establishing comparative financial statements (IFRS 1). If auditors are required to review the restated data, then this can also cause a temporary increase in audit costs.

To obtain further insight into the temporary transitional audit fee increase in the initial IFRS adoption year, we include in Column (2) of Table 5 the observations from the adoption year and test whether audit firms charge a significantly higher fee in the year of adoption relative to post-adoption years. Specifically, we add *ADOPT* and *ADOPT* ∗ *TREAT* to Equation (2), where *ADOPT* equals 1 if it is the IFRS (pseudo-) adoption year for the treatment (control) sample, and 0 otherwise. The *ADOPT* ∗ *TREAT* (*POST* ∗ *TREAT*) term captures the incremental audit fee increases in the adoption year (subsequent post-IFRS years) brought about by IFRS adoption in the treatment sample over and beyond the general trend in audit fees. As shown in Column (2) of Table 5, the coefficient of *ADOPT* ∗ *TREAT* (β_4) is 0.073 ($t = 4.34$), which is greater than the coefficient of *POST* ∗ *TREAT* ($\beta_2 = 0.052$; $t = 2.63$). However, as shown in the third to last row of Table 5, the difference in magnitude between β_2 and β_4 is not statistically significant ($F = 1.78$; $p = 0.18$). One possible explanation for this insignificant difference is that audit firms may have spread the temporary transitional costs incurred in the initial year of IFRS adoption to subsequent years by charging higher fees in later periods.

The results reported in Table 5, taken together, suggest that IFRS adoption increases the audit fee to a higher level both in the year of adoption and in subsequent years. Stated another way, IFRS adoption results in a positive audit fee premium. This implies that the audit fee-increasing effect of the increased complexity arising from IFRS adoption dominates the associated audit fee-decreasing effect of the improved financial reporting quality.

Tests of H2, H3, and H4

H2, H3, and H4 are concerned with whether the IFRS-related audit fee premium differs systematically, depending on the increase in audit complexity associated with IFRS adoption (H2), the improvement in reporting quality brought about by IFRS adoption (H3), and the strength of a country's legal regime (H4). To test H2 through H4, we estimate Equation (3) and report the results in Table 6.

In Columns (1) to (3) of Table 6, we measure the strength of legal regime using the natural log of the Wingate index (*WINGATE*), the change in audit complexity ($\Delta COMPLEXITY$) using the natural log of the sum of *Absence* and *Divergence*, and the change in reporting quality ($\Delta QUALITY$) using three alternative measures, that is, $\Delta GAAPQ = ln(1 + Absence)$, $(-1) * \Delta|DA1|$, and $(-1) * \Delta|DA1|$.

As shown in Columns (1) to (3) of Table 6, the coefficient of $POST * TREAT * \Delta COMPLEXITY$ is significantly positive at less than the 5 percent level across all three columns. This is consistent with H2, suggesting that the audit fee premium associated with IFRS adoption is positively associated with the increase in audit complexity brought about by IFRS adoption in a particular country. We also find that the coefficient of $POST * TREAT * \Delta QUALITY$ is significantly negative, at less than the 5 percent level, across all three columns. This result is consistent with H3 and indicates that, *ceteris paribus*, the greater the improvement in financial reporting quality brought about by IFRS adoption, the lower the audit fee premium associated with IFRS adoption.

Finally, we find mixed results with respect to the impact of legal regime on the IFRS-related audit fee premium. In Column (1) of Table 6, where $\Delta QUALITY$ is proxied by $\Delta GAAPQ = ln(1 + Absence)$, the coefficient of $POST * TREAT * WINGATE$ is negative and significant at the 10 percent level. This result indicates that the adoption of IFRS results in a smaller IFRS-related audit fee premium in countries with a strong legal regime than in countries with a weak legal regime. According to Observation 4, this result suggests that the greater audit effort induced by a stronger legal regime has a stronger effect on reducing auditors' expected legal liability cost and, hence, reducing audit fee premium, than does the direct impact of stronger legal regime on increasing audit fee premium.[16] However, in Columns (2) and (3) of Table 6, where $\Delta QUALITY$ is proxied by two alternative measures of discretionary accruals, namely $(-1) * |DA1|$ or $(-1) * |DA2|$, we find that the coefficient of $POST * TREAT * WINGATE$ is insignificant.[17]

In Columns (4) to (6) of Table 6, we repeat the analysis using alternative legal regime measures. Specifically, in lieu of *WINGATE*, we use both *LAW* and *ENFORCE* to capture

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[16] We note that while a stronger legal regime increases the *level* of audit fees (e.g., Choi et al. 2008, 2009), the stronger legal regime does not necessarily translate to a greater *change* in audit fees. For example, Choi et al. (2008) predict and find that as a legal regime becomes stronger, the audit quality and audit fee differences between big and small auditors become smaller or insignificant. In this regard, our finding of a smaller IFRS-related audit fee premium in a stronger legal regime is consistent with their finding of a smaller Big 4 audit fee premium in a stronger legal regime.

[17] Multicollinearity could be a potential reason for the insignificant result for legal regime proxies in the alternative model specifications in Columns (2), (3), (5), (6) of Table 6 because the Variance Inflation Factors for the test variables exceed a threshold value of 10 in the regressions.

TABLE 6

Results of Multivariate Regressions for Testing H2, H3, and H4^{a,b,c}

Panel A: Test Variables

	Dependent Variable = *AUDFEE*					
	Wingate Litigation Index as Legal Regime Proxy (*WINGATE*)			Alternative Legal Regime Proxies (*LAW* and *ENFORCE*)		
Variables (Pred. Sign)	**(1) Δ*GAAPQ***	**(2) Δ*QUALITY* = (−1) ∗ Δ|*DA1*|**	**(3) (−1) ∗ Δ|*DA2*|**	**(4) Δ*GAAPQ***	**(5) Δ*QUALITY* = (−1) ∗ Δ|*DA1*|**	**(6) (−1) ∗ Δ|*DA2*|**
POST	0.019*	0.019*	0.019*	0.019*	0.019*	0.019*
	(1.88)	(1.90)	(1.90)	(1.88)	(1.90)	(1.90)
POST ∗ *TREAT*	−1.304**	−1.060**	−1.417**	−2.688***	−1.109**	−1.429***
	(−2.34)	(−2.10)	(−2.52)	(−2.59)	(−2.29)	(−2.65)
POST ∗ *TREAT* ∗ Δ*COMPLEXITY* (+)	0.533***	0.287**	0.352***	1.057**	0.326***	0.381***
	(2.72)	(2.43)	(2.72)	(2.51)	(2.59)	(2.80)
POST ∗ *TREAT* ∗ Δ*QUALITY* (−)	−0.104**	−4.913***	−4.644***	−0.383**	−4.449***	−4.734**
	(−2.19)	(−3.54)	(−2.88)	(−2.00)	(−3.19)	(−2.54)
POST ∗ *TREAT* ∗ *WINGATE* (?)	−0.238*	0.034	0.091			
	(−1.66)	(0.55)	(1.44)			
POST ∗ *TREAT* ∗ *LAW* (?)				−0.765*	0.068	0.055
				(−1.76)	(1.26)	(0.99)
POST ∗ *TREAT* ∗ *ENFORCE* (?)				−0.387**	−0.120	0.095
				(−2.16)	(−0.89)	(0.57)

(continued on next page)

TABLE 6 (continued)

Panel B: Firm-Specific Control Variables (*FSCONTROL*)

	Dependent Variable = *AUDFEE*					
	Wingate Litigation Index as Legal Regime Proxy (*WINGATE*)			Alternative Legal Regime Proxies (*LAW* and *ENFORCE*)		
Variables (Pred. Sign)	(1) *ΔGAAPQ*	(2) *ΔQUALITY* = (−1) * Δ|*DA1*|	(3) (−1) * Δ|*DA2*|	(4) *ΔGAAPQ*	(5) *ΔQUALITY* = (−1) * Δ|*DA1*|	(6) (−1) * Δ|*DA2*|
LNTA (+)	0.563***	0.563***	0.563***	0.563***	0.563***	0.563***
	(86.89)	(86.75)	(86.76)	(86.90)	(86.75)	(86.75)
INVREC (+)	0.216***	0.216***	0.216***	0.215***	0.216***	0.216***
	(4.07)	(4.07)	(4.07)	(4.07)	(4.07)	(4.07)
NBS (+)	0.078***	0.075***	0.075***	0.078***	0.075***	0.075***
	(4.39)	(4.19)	(4.19)	(4.40)	(4.19)	(4.19)
NGS (+)	0.237***	0.233***	0.233***	0.237***	0.233***	0.233***
	(13.09)	(12.80)	(12.80)	(13.09)	(12.80)	(12.80)
MB (?)	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***
	(6.28)	(6.26)	(6.26)	(6.28)	(6.26)	(6.26)
LOSS (+)	0.245***	0.245***	0.245***	0.245***	0.245***	0.245***
	(14.29)	(14.27)	(14.28)	(14.30)	(14.28)	(14.28)
LEV (+)	0.061	0.062	0.061	0.061	0.062	0.061
	(1.18)	(1.20)	(1.19)	(1.18)	(1.20)	(1.19)
QUICK (−)	−0.020***	−0.020***	−0.020***	−0.020***	−0.020***	−0.020***
	(−5.89)	(−5.95)	(−5.96)	(−5.89)	(−5.95)	(−5.95)
SPECIAL_ITEM (+)	0.165***	0.168***	0.168***	0.164***	0.168***	0.168***
	(8.33)	(8.57)	(8.56)	(8.34)	(8.57)	(8.56)
QUALIFIED (+)	0.061	0.062	0.062	0.060	0.062	0.062
	(1.25)	(1.26)	(1.27)	(1.23)	(1.26)	(1.27)
MERGE (+)	0.084***	0.085***	0.086***	0.084***	0.086***	0.086***
	(5.57)	(5.63)	(5.67)	(5.55)	(5.64)	(5.66)
FINANCE (+)	−0.018	−0.018	−0.018	−0.018	−0.018	−0.018
	(−1.18)	(−1.15)	(−1.16)	(−1.20)	(−1.15)	(−1.16)

(continued on next page)

TABLE 6 (continued)

	Dependent Variable = *AUDFEE*					
	Wingate Litigation Index as Legal Regime Proxy (*WINGATE*)			**Alternative Legal Regime Proxies (*LAW* and *ENFORCE*)**		
Variables (Pred. Sign)	**(1) $\Delta GAAPQ$**	**(2) $\Delta QUALITY = (-1) * \Delta\lvert DA1\rvert$**	**(3) $(-1) * \Delta\lvert DA2\rvert$**	**(4) $\Delta GAAPQ$**	**(5) $\Delta QUALITY = (-1) * \Delta\lvert DA1\rvert$**	**(6) $(-1) * \Delta\lvert DA2\rvert$**
BIG4 (+)	0.266***	0.267***	0.267***	0.266***	0.267***	0.267***
	(10.82)	(10.93)	(10.93)	(10.82)	(10.93)	(10.93)
CROSS (+)	0.335***	0.356***	0.356***	0.335***	0.356***	0.356***
	(6.27)	(6.59)	(6.60)	(6.27)	(6.60)	(6.60)
Intercept	−2.929***	−0.979***	−0.939***	−3.043***	−0.955***	−0.917***
	(−9.50)	(−3.57)	(−3.43)	(−9.26)	(−3.46)	(−3.33)
Country dummies	Included	Included	Included	Included	Included	Included
Industry dummies	Included	Included	Included	Included	Included	Included
Observations	11,825	11,771	11,767	11,825	11,771	11,767
Adjusted R^2	0.85	0.85	0.85	0.85	0.85	0.85

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

[a] The final sample consists of 2,860 firm-year observations from 11 EU countries in the treatment group and 9,052 firm-year observations from three OECD countries in the control group. The final sample includes observations in the pre-IFRS period (i.e., $t-1$), and the post-IFRS period (i.e., $[t+1, t+3]$), where t is the IFRS adoption year for the treatment sample or the assigned pseudo-adoption year for the control sample.

[b] Refer to Appendix B for variable definitions.

[c] Observations with absolute studentized residuals greater than 3 are deleted. The reported t-statistics (in parentheses) are based on standard errors clustering on the firm dimension.

differences in a country's legal environment. The results are qualitatively similar to those reported in Columns (1) to (3) of Table 6,[18] suggesting that our results are robust to the use of alternative proxies for a country's litigation environment.

VI. FURTHER ANALYSIS

Concurrent Reforms in the EU

Along with the introduction of mandatory IFRS reporting, the EU also made concurrent efforts to harmonize accounting and auditing practices and to enhance enforcement and governance mechanisms. For example, in 2003, the Committee of European Securities Regulators released its first standard (Standard No. 1) to develop and implement a common approach to the enforcement of IFRS throughout the EU. In addition, various efforts have been made to harmonize and improve EU auditing practices since 2002.[19] In the areas of company law and corporate governance, on May 25, 2003, the European Commission issued a communication to the European Parliament entitled *Modernizing Company Law and Enhancing Corporate Governance in the European Union—A Plan to Move Forward*, which sets up agendas for changes in various areas of corporate governance, such as the role of independent directors and shareholder rights. In short, the last decade has seen gradual changes in governance and enforcement mechanisms in the EU, along with implementation of IFRS reporting.

These concurrent changes pose an important challenge to our efforts to separate the audit fee effect of mandatory IFRS adoption *per se* from the effect associated with concurrent governance and enforcement reforms, as well as concurrent changes in accounting and auditing practices, which, for convenience, we refer to as *concurrent reforms*. In an attempt to isolate the IFRS adoption effect from the concurrent reform effect, we include in Equation (3) an additional variable, $POST * TREAT * REFORM$. For this additional test, we obtain information about country-level efforts on concurrent reforms from the Annual Executive Opinion Survey conducted by the Institute for Management Development. This survey is designed to quantify issues of competitiveness that are not easily measured, such as management practices, labor relations, and corruption. Among other questions, the survey asks respondents to evaluate the extent to which auditing and accounting practices are implemented in their firms adequately and the extent to which corporate boards supervise company management effectively. We then construct our measure of concurrent reform, denoted *REFORM*, by taking the arithmetic mean of (1) the change in corporate governance effectiveness, measured as the average of corporate governance effectiveness scores between 2006 and 2008 minus the score in 2004, and (2) the change in accounting and audit practices, measured as the average of the accounting and audit practices scores between 2006 and 2008 minus the score in 2005.[20]

We estimate Equation (3) with $POST * TREAT * REFORM$ being added. The untabulated results show that the coefficient of this additional variable is insignificant. More importantly, with the exception that the coefficient of $POST * TREAT * LAW$ becomes insignificant, the results for our test variables remain qualitatively similar to those reported in Table 6, suggesting that our main results are unlikely to be driven by concurrent reforms. Admittedly, our measure of concurrent reform and the difference-in-differences design may not sufficiently control for the effect of concurrent changes on our test results. As in other studies (Barth et al. 2008; Daske et al. 2008), we therefore cannot completely rule out the possibility that the audit fee-increasing effect of IFRS adoption reported in this study reflects the combined effects associated with IFRS adoption and other concurrent reforms.

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[18] The term "qualitatively similar" or "qualitatively unchanged" in this paper means that both the signs and the statistical significance for the variables of interest remain unaltered.

[19] After the Enron scandal in the U.S., a meeting of EU Finance Ministers in May 2002 called on the Commission to adopt Recommendation 2002/590/EC, *Statutory Auditors' Independence: A Set of Fundamental Principles*. In September 2003, the Commission published *Communication on Reinforcing the Statutory Audit in the EU* (2003/236/02). In April 2006, the EU Council adopted the *Statutory Audit Directive* (2006/43/EC), also known as E-SOX, with a deadline of June 2008 for all member states to ensure that all local laws are in compliance with this directive.

[20] We use the 2005 score as an approximation for the pre-IFRS measure because the survey data related to auditing and accounting practices were not available until 2005.

Change Analyses

Our main analyses have thus far used pooled cross-sectional regressions of audit fees on our test variables, firm-specific controls, and country indicators. In an attempt to mitigate concern about potential problems of correlated omitted variables, we also conduct change analyses. We examine whether the audit fee change from the pre-IFRS to the post-IFRS periods, denoted *ΔAUDFEE,* is associated with our test variable *TREAT*, and how this association varies with *ΔCOMPLEXITY*, *ΔQUALITY*, and the strength of legal regime. For this purpose, we estimate the following regression:

$$\begin{aligned}\Delta AUDFEE = {} & \gamma_0 + \gamma_1 TREAT + \gamma_2 TREAT*\Delta COMPLEXITY + \gamma_3 TREAT*\Delta QUALITY \\ & + \gamma_4 TREAT*Legal\ Regime + \gamma_5 TREAT*REFORM + \Sigma\delta_k \Delta FSCONTROL_k + \varepsilon,\end{aligned} \tag{4}$$

where all variables are as defined earlier. In each of the post-IFRS years (t+1, t+2, t+3), we calculate $\Delta FSCONTROL_k$ relative to the pre-IFRS year (t–1). We use the same set of firm-specific controls as in Equation (3), except that we exclude *NBS*, *NGS*, *BIG4*, and *CROSS* because, as explained in footnote 12, all these variables are treated as static variables to be measured as of 2008. To address the change in the *LOSS* indicator in Equation (3), following Ghosh and Pawlewicz (2009), we create two indicator variables: (1) *Noloss-to-Loss* is an indicator variable that equals 1 for firms that report no loss in the pre-IFRS period but report a loss in the post-IFRS period, and 0 otherwise; (2) *Loss-to-Noloss* is an indicator variable that equals 1 for firms that report a loss in the pre-IFRS period but report no loss in the post-IFRS period, and 0 otherwise. For other indicator variables included in Equation (3), that is, *SPECIAL_ITEM*, *QUALIFIED*, *MERGE*, and *FINANCE*, we again create separate indicator variables in a similar manner.

Column (0) of Table 7 reports the results of our baseline change regression, in which the test variable is *TREAT*. Consistent with results in Table 5, we find that the coefficient of *TREAT* is significantly positive (0.042; t = 2.25). This leads us to reject null H1, suggesting that the audit fee increases associated with IFRS adoption are greater for adopter firms in the treatment sample than for non-adopter firms in the control sample.

Columns (1)–(6) of Table 7 report the results of the impact of the institutional factors on the IFRS-related audit fee premium across countries. Similar to our main research design using level-based analyses, in Columns (1) to (3), the strength of the legal regime is proxied by *WINGATE*, while in Columns (4) to (6), it is proxied jointly by *LAW* and *ENFORCE*. We find that, except for the insignificant coefficient of *TREAT* ∗ *LAW*, all of the results for our test variables are consistent with our main finding as reported in Tables 6. The corroborating results alleviate concerns about potential problems of correlated omitted variables.

Cross-Country Variation in Sample Size

As shown in Table 2, our sample size varies across countries. To address concerns that our results may be unduly influenced by the unequal distribution of sample firms across countries, we conduct two additional analyses. First, since U.K. firms represent a large portion of our sample, we

TABLE 7

Change Analysis for Testing H1, H2, H3, and H4[a,b,c]

Panel A: Test Variables

	Dependent Variable = Δ*AUDFEE*						
		Wingate as Legal Regime Proxy (*WINGATE*)			Alternative Legal Regime Proxies (*LAW* and *ENFORCE*)		
Variable (Pred. Sign)	(0)	(1) Δ*GAAPQ*	(2) Δ*QUALITY* = (−1) ∗ Δ|*DA1*|	(3) (−1) ∗ Δ|*DA2*|	(4) Δ*GAAPQ*	(5) Δ*QUALITY* = (−1) ∗ Δ|*DA1*|	(6) (−1) ∗ Δ|*DA2*|
TREAT	0.042**	−2.193***	−0.949*	−1.362**	−2.727***	−1.138*	−1.261**
	(2.25)	(−3.15)	(−1.68)	(−2.25)	(−2.73)	(−1.81)	(−2.03)
TREAT ∗ Δ*COMPLEXITY* (+)		0.864***	0.268**	0.345**	1.012**	0.338**	0.332**
		(3.77)	(2.14)	(2.55)	(2.46)	(2.12)	(2.08)
TREAT ∗ Δ*QUALITY* (−)		−0.175***	−5.551***	−5.838***	−0.320	−4.666***	−7.709***
		(−3.67)	(−3.79)	(−3.52)	(−1.59)	(−2.87)	(−2.84)
TREAT ∗ *WINGATE* (?)		−0.389***	0.021	0.088			
		(−2.86)	(0.32)	(1.34)			
TREAT ∗ *LAW* (?)					−0.583	0.081	−0.011
					(−1.23)	(1.10)	(−0.12)
TREAT ∗ *ENFORCE* (?)					−0.397**	−0.164	0.266
					(−2.27)	(−1.22)	(1.26)

(continued on next page)

TABLE 7 (continued)

Panel B: Concurrent Reform Control Variable

		Dependent Variable = $\Delta AUDFEE$					
		Wingate as Legal Regime Proxy (*WINGATE*)			Alternative Legal Regime Proxies (*LAW* and *ENFORCE*)		
Variable (Pred. Sign)	**(0)**	**(1) $\Delta GAAPQ$**	**(2) $\Delta QUALITY = (-1) * \Delta\lvert DA1\rvert$**	**(3) $(-1) * \Delta\lvert DA2\rvert$**	**(4) $\Delta GAAPQ$**	**(5) $\Delta QUALITY = (-1) * \Delta\lvert DA1\rvert$**	**(6) $(-1) * \Delta\lvert DA2\rvert$**
*TREAT * REFORM* (?)		0.191	−0.094	−0.118	0.061	−0.017	−0.201
		(1.50)	(−0.73)	(−0.93)	(0.42)	(−0.10)	(−0.98)
Observations	8,137	8,136	8,097	8,094	8,136	8,097	8,094
Adjusted R^2	0.25	0.26	0.26	0.26	0.26	0.26	0.26

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

[a] The final sample consists of 2,027 firm-year observations from 11 EU countries in the treatment group and 6,201 firm-year observations from three OECD countries in the control group. The intercept and firm-specific control variables are not reported for parsimony, but the reported coefficients stem from estimating the full model of Equation (4).

[b] The dependent variable is the change in audit fees ($\Delta AUDFEE$). Here Δ is a change operator defined as the difference between the number in each of the post-IFRS years (t+1, t+2, t+3), and that in the pre-IFRS year (t–1). See Appendix B for variable definitions.

[c] Observations with absolute studentized residuals greater than 3 are deleted. The reported t-statistics (in parentheses) are based on standard errors clustering on the firm dimension.

repeat our main analyses after deleting the U.K. observations. Untabulated results show that all our findings remain qualitatively unchanged, except that the negative coefficient of *POST* ∗ *TREAT* ∗ *WINGATE* becomes insignificant. Our explanation is that removing the U.K. significantly reduces the cross-country variation in legal regime. Second, following Daske et al. (2008), we randomly select up to 150 firms in each country and re-estimate all the regressions. The untabulated results for our test variables using this approach remain qualitatively similar to those reported in Tables 5 to 7.

Auditor Switches

As mentioned earlier, Worldscope only provides the auditor identity data for the most recent year, 2008. One concern is that auditor switches during the sample period may confound our empirical analysis of the IFRS-related fee change, as the switch from non-Big 4 to Big 4 auditors may lead to an audit fee increase for the switching clients.

To address this potential confounding effect, we obtained the 2005 version of Worldscope that includes the auditor identity data as of 2004, the first year of our sample period. To identify auditor changes, we compare auditors as of 2004 with auditors as of 2008. Our final treatment sample consists of 833 firms, of which information on auditor change is available for 802 firms. Among these, 34 firms (4.24 percent) switched from non-Big 4 to Big 4 auditors, and 35 firms (4.36 percent) switched from Big 4 to non-Big 4 auditors. This relatively small rate of auditor switches suggests that auditor switches are unlikely to cause systematic biases to our results. Moreover, the untabulated results after excluding firms with auditor changes remain qualitatively unchanged, suggesting that the IFRS-related audit fee premium observed in our main analyses is unlikely to be driven by auditor changes that occurred during our sample period.

VII. SUMMARY AND CONCLUDING REMARKS

We investigate the impact of IFRS on audit fees using audit fee data from EU countries that mandated IFRS adoption in 2005. Our theoretical analysis suggests that IFRS adoption has two opposite effects on audit fees. On the one hand, the increase in audit task complexity arising from IFRS adoption increases audit fees, but on the other hand, the improvement in financial reporting quality leads to a decrease in audit fees. Our empirical tests show that mandatory IFRS adoption leads to an increase in audit fees, which suggests that the increase in audit task complexity is the driving force behind the IFRS-related audit fee increase. Further, we find that the IFRS-related audit fee premium increases with the extent of accounting differences between a country's former local GAAP and IFRS, and decreases with improvements in financial reporting quality brought about by IFRS adoption. We also find some evidence that the IFRS-related audit fee premium decreases with the strength of a country's legal regime.

Our study provides a theory and supporting evidence for understanding the impact of accounting standards, and their changes, on audit pricing. In particular, our results provide useful insights into the channels of audit complexity and financial reporting quality through which the adoption of new accounting standards influences audit pricing. To further understand the impact of IFRS on the auditing profession, we recommend that future research examine the audit fee effect of IFRS adoption on client companies with different characteristics, as well as how IFRS affects the interactions between real activities manipulation and accrual-based earnings management.

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APPENDIX A

Proof of Observation 2

$$\Delta f \approx df = \frac{\partial f}{\partial q}\Delta q + \frac{\partial f}{\partial c}\Delta c = (1 - e^*)^2 rl[(1 - q)\Delta c - c\Delta q].$$

Thus, $df > 0$ ($df < 0$) if $(1 - q)\Delta c > c\Delta q$ (if$(1 - q)\Delta c < c\Delta q$), where Δc and Δq denote the changes in audit complexity and financial reporting quality that are brought about by IFRS adoption. ■

Proof of Observation 4

$$\begin{aligned}\frac{\partial df}{\partial r} &= \frac{\partial df}{\partial r}\Big|_{e=e^*} + \frac{\partial df}{\partial e}\frac{\partial e}{\partial r}\\ &= (1 - e)l[(1 - q)\Delta c - c\Delta q][k - (1 - q)rcl].\end{aligned}$$

Hence, given $(1 - q)\Delta c > c\Delta q$, we know from the above derivation that $\frac{\partial df}{\partial r} < 0 \left(\frac{\partial df}{\partial r} > 0\right)$ when $r > \frac{k}{cl(1-q)} \left(r < \frac{k}{cl(1-q)}\right)$. ■

APPENDIX B

EMPIRICAL DEFINITIONS OF VARIABLES

Variable		Empirical Definition	Data Source
The Dependent Variable and Country-Specific Variables			
AUDFEE	=	natural log of audit fees in thousands of euros;	Worldscope
TREAT	=	1 for firms in the treatment sample (i.e., mandatory adopter firms in EU countries) and 0 for firms in the benchmark control sample (i.e., non-adopter firms in non-EU OECD countries);	Worldscope
POST	=	1 for the post-IFRS period and 0 for the pre-IFRS period. For firms in the control sample, we assign the pseudo-adoption year 2005 (2006) for firms with a December (non-December) fiscal year-end;	Worldscope
Δ*COMPLEXITY*	=	change in audit complexity, measured by the natural log of the sum of the *Absence* score and the *Divergence* score. The *Absence* score captures the number of absent items in local GAAP compared with IFRS. The *Divergence* score captures the number of accounting rules regarding the same accounting issue that differ between IFRS and local GAAP;	Ding et al. (2007)
Δ*GAAPQ*	=	change in accounting standard quality, measured by the natural log of 1 plus the *Absence* score, where the *Absence* score captures the number of absent items in local GAAP compared to IFRS;	Ding et al. (2007)
Δ|*DA1*|	=	country's median value of the absolute value of discretionary accruals in the post-IFRS period (i.e., $[t+1, t+3]$) minus the median value in the pre-IFRS period (i.e., $t-1$), where discretionary accruals are the residuals from the cross-sectional Jones (1991) model. Refer to Appendix C for more details;	Worldscope
Δ|*DA2*|	=	country's median value of the absolute value of discretionary accruals in the post-IFRS period (i.e., $[t+1, t+3]$) minus the median value in the pre-IFRS period (i.e., $t-1$), where discretionary accruals are the residuals from the augmented cross-sectional Dechow-Dichev model. Refer to Appendix C for more details;	Worldscope
WINGATE	=	strength of a country's legal regime, measured by the natural log of the Wingate (1997) litigation index;	Wingate (1997)
LAW	=	1 for countries with a common law legal tradition and 0 for countries with a civil law tradition;	La Porta et al. (1998)
ENFORCE	=	aggregate measure of public enforcement, equal to the arithmetic mean of (1) the supervisor characteristics index, (2) the rule-making power index, (3) the investigative powers index, (4) the orders index, and (5) the criminal index; and	La Porta et al. (2006)

(continued on next page)

APPENDIX B (continued)

Variable		Empirical Definition	Data Source
REFORM	=	aggregate measure of the effect of concurrent reforms, equal to the arithmetic mean of (1) the average score of corporate board effectiveness between 2006 and 2008 minus the score in 2004 and (2) the average score of auditing and accounting practices between 2006 and 2008 minus the score in 2005.	IMD (2010) World Competitiveness Yearbook
Firm-Specific Control Variables (*FSCONTROL*)			
LNTA	=	natural log of year-end total assets in thousands of euros;	Worldscope
INVREC	=	sum of inventories and receivables divided by total assets;	Worldscope
NBS	=	natural log of 1 plus the number of business segments;	Worldscope
NGS	=	natural log of 1 plus the number of geographical segments.	Worldscope
MB	=	year-end market-to-book ratio, defined as firm market value divided by the common shareholder equity;	Worldscope
LOSS	=	1 when a firm reports a net loss, and 0 otherwise;	Worldscope
LEV	=	ratio of year-end total liabilities to total assets;	Worldscope
QUICK	=	quick ratio, equal to quick assets divided by current liabilities;	Worldscope
SPECIAL_ITEM	=	1 if the firm reports special items, and 0 otherwise;	Worldscope
QUALIFIED	=	1 if the firm receives qualified opinions, and 0 otherwise;	Worldscope
MERGE	=	1 if the firm is engaged in a merger or acquisition, and 0 otherwise;	Worldscope
FINANCE	=	1 if *MERGE* is not equal to 1 and either of the following conditions applies: long-term debt increased by 20 percent or more, or the number of shares outstanding increased by 10 percent or more after controlling for stock splits;	Worldscope
BIG4	=	1 when a firm uses one of the Big 4 auditors, and 0 otherwise; and	Worldscope
CROSS	=	1 when a firm is cross-listed in a foreign country, and 0 otherwise.	Worldscope

APPENDIX C
CONSTRUCTION OF DISCRETIONARY ACCRUALS

Following prior literature, we adopt two widely used models to estimate the normal accruals with residuals from the models as measures of discretionary accruals (denoted *DA1* and *DA2*). The first model, the cross-sectional Jones (1991) model, is specified as:

$$TA_t = \beta_0 + \beta_1 \Delta REV_t + \beta_2 PPE_t + \varepsilon_t, \tag{C1}$$

where TA_t is total accruals in year t, that is, income before extraordinary items minus operating cash flows; ΔREV_t is change in net revenues from year $t-1$ to year t; and PPE_t is net property, plant, and equipment. The second model, the augmented cross-sectional Dechow and Dichev (2002) model, follows Francis et al. (2005) and is specified as:

$$TCA_t = \beta_0 + \beta_1 CFO_{t-1} + \beta_2 CFO_t + \beta_3 CFO_{t+1} + \beta_4 \Delta REV_t + \beta_5 PPE_t + \varepsilon_t, \tag{C2}$$

where TCA_t is total current accruals in year t, calculated as ΔCA_t (change in current assets) $-$ ΔCL_t (change in current liability) $-$ $\Delta Cash_t + \Delta STDEBT_t$ (change in debt in current liabilities); CFO_t, CFO_{t-1}, and CFO_{t+1} are cash flow from operations in year t, $t-1$, and $t+1$, respectively; ΔREV_t is change in net revenues from year $t{-}1$ to year t; and PPE_t is net property, plant, and equipment. All variables are scaled by total assets at the beginning of the period, and subscripts for firm i and country j are suppressed.

Both models are estimated cross-sectionally for each industry-year within each country, with industry definition based on the Fama and French (1997) ten-industry classification. Industry-year combinations with fewer than seven observations are deleted. To run the regression, we use all observations with data available for the estimation, except firms that adopted IFRS voluntarily and those in the banking, insurance, and other financial industries. For the pre-IFRS period, the measures are based on data reported under domestic GAAP, and for the post-IFRS period the measures are based on data reported under IFRS. It would be inappropriate here to use restated financial data (following IFRS) in the pre-IFRS period, since the restated financial data would omit the change in financial reporting quality associated with different accounting systems. Note that we use semi-annual data for our analysis instead of annual data. Semi-annual data are not subject to the auditing process and, hence, better capture the pre-audit financial reporting quality in our theoretical model. Since some firms provide quarterly disclosures while others provide semi-annual disclosures, we standardize the data by aggregating quarterly data into semi-annual data.

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Management Forecast Accuracy and CEO Turnover

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ABSTRACT: We investigate whether management forecast accuracy provides a signal regarding CEOs' ability to anticipate and respond to future events by examining the relation between management forecast errors and CEO turnover. We find that the probability of CEO turnover is positively related to the magnitude of absolute forecast errors when firm performance is poor and that this positive relation holds for both positive and negative forecast errors. In addition, we find that the positive relation between CEO turnover and the absolute forecast errors is concentrated in the sample of less entrenched CEOs. Our findings indicate that boards of directors use management forecast accuracy as a signal of CEOs' managerial ability and that managers bear a cost for issuing inaccurate forecasts.

Keywords: *management forecast accuracy; CEO ability; CEO turnover.*

I. INTRODUCTION

Prior literature documents that management forecasts provide a key source of information to the capital market (Patell 1976; Ajinkya and Gift 1984; Waymire 1984; Pownell and Waymire 1989; Kasznik and Lev 1995; Frankel et al. 1995; Coller and Yohn 1997; Lennox and Park 2006). However, there is little empirical evidence regarding the role of management forecasts in the Chief Executive Officer (CEO) labor market.[1] Trueman (1986) suggests that

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[1] In contemporaneous research, Zamora (2009) documents evidence that Chief Financial Officers are rewarded for demonstrating superior forecasting ability.

management earnings forecasts provide a public signal regarding a manager's ability to anticipate future changes in the firm's business environment and to adjust the firm's operations accordingly. Given the importance of these skills to a firm's success in a competitive product market, we predict that the board of directors is more likely to replace their CEO if management forecast outcomes indicate that the CEO lacks those abilities. In this study, we provide empirical evidence as to whether boards of directors consider management forecast accuracy as a signal of the CEO's managerial ability by examining the relation between forecast accuracy and the board's decision to retain or replace the CEO.

The skills necessary to generate an accurate forecast of earnings are closely related to the skills necessary to effectively manage a firm in an uncertain environment. Managers who issue more accurate forecasts are likely to have a better understanding of how changing business conditions affect the firm's product demand and cost structure and be able to more effectively respond to future changes in the economic environment. Thus, managers who have a stronger ability to deal with uncertain future events should be able to issue more accurate forecasts of earnings. If management's earnings forecast accuracy serves as an indicator of the CEO's managerial ability, then the board of directors is likely to incorporate that information into the decision as to whether to retain or replace the CEO. Thus, we predict that the absolute management forecast error, which is inversely related to the forecast accuracy, is positively associated with the probability of CEO turnover.

While the board is likely to consider management forecast accuracy as a valuable signal of the CEO's managerial ability, the board also receives signals regarding overall firm performance. Given that the firm incurs substantial costs from replacing the CEO (e.g., severance payments and the disruption to the firm's operations), the board is more likely to replace the CEO when multiple signals are congruent. Prior literature suggests that financial performance is a strong indicator of CEOs' operating ability (e.g., Warner et al. 1988; Weisbach 1988). Thus, if firm performance is good, then the board is less likely to change leadership based solely on forecast accuracy. However, when firm performance is poor, less accurate forecasts are likely to reinforce the board's concern about the CEO's managerial ability, thereby increasing the probability of CEO turnover. As a result, we predict that the positive relation between the absolute management forecast error and CEO turnover is likely to be stronger when firm performance is poor.

Our first two predictions assume that the board is able to replace the CEO as it revises its beliefs regarding the CEO's ability. However, in some firms, CEOs wield a substantial degree of power within the corporate organization and the board may find it difficult to overcome entrenchment and replace the CEO (Dikolli et al. 2009). We therefore predict that the positive relation between the absolute management forecast error and CEO turnover is more likely to hold when the CEO is less entrenched.

We test our predictions using a sample of 30,481 firm-quarters collected from the intersection of the ExecuComp, Compustat, CRSP, First Call, and Thompson databases. Our sample period consists of quarterly observations for the period from 1996 to 2006. Since managers forecast earnings on a voluntary basis, we jointly estimate the management forecast issuance and CEO turnover decisions to control for potential selection bias in the estimation of the impact of forecast outcomes on CEO turnover.

We find a significantly positive association between the absolute management forecast error and the probability of CEO turnover. This finding is consistent with the board of directors using management forecast accuracy as a signal of the CEO's managerial ability. To gauge the economic significance of our results, we shift the absolute management forecast error from the 25th (10th) percentile to the 75th (90th) percentile and find that the likelihood of CEO turnover increases by 0.46 (1.17) percent. This change represents an increase of 16.6 (42.2) percent from the unconditional average turnover rate of 2.77 percent. The marginal effects are also relatively large compared to other indicators of the CEO's ability and performance such as return on equity, 0.15 (0.38) percent; cumulative abnormal market return, 0.41 (0.91) percent; and analyst forecast error, 0.17 (0.59) percent.

In addition, when we classify firms into a good or poor-performance group based on the signs of quarterly analyst forecast errors, we find that the positive relation between the absolute management forecast error and the probability of CEO turnover remains significant only in the poor-performance group. These findings suggest that when firm performance is poor, an inaccurate management forecast serves as a confirming signal that the CEO needs to be replaced.

To investigate the effect of CEO entrenchment, we use CEO ownership, CEO/Chairman duality, and CEO tenure as measures of entrenchment. We use the measures to construct dummy variables representing low entrenchment and interact each dummy variable with management forecast accuracy. We find a significantly positive coefficient when entrenchment is based on CEO ownership or CEO tenure. These findings are consistent with the prediction that the positive relation between the absolute management forecast error and CEO turnover is stronger when the CEO is less entrenched.

We also examine whether the relation between management forecast accuracy and CEO turnover depends on the sign of management forecast errors and find that the magnitudes of both positive and negative forecast errors are positively related to CEO turnover when firm performance is poor. The finding with positive forecast errors (i.e., reported earnings exceed the forecast) indicates that management forecast accuracy is not a proxy for overall firm performance.

Finally, to further validate our inferences, we apply our management turnover model to a holdout sample using data from 2007–2010. We find that our primary model with the management forecast error and poor performance interaction generally reduces the cost of prediction errors relative to a naive model and relative to the primary model without management forecast error terms.

Our study contributes to the literature on CEO turnover by documenting the importance of management earnings forecasts as a signal of CEOs' managerial ability in CEO replacement decisions. Prior CEO turnover literature has documented performance measures (Warner et al. 1988; Weisbach 1988; Murphy and Zimmerman 1993; Puffer and Weintrop 1991; DeFond and Park 1999; Farrell and Whidbee 2003) and governance structures (Weisbach 1988; Hermalin and Weisbach 1998; Yermack 1996; Goyal and Park 2002) as important determinants in CEO turnover decisions. Our study differs from prior literature in that if overall firm performance is poor, then we find a positive relation between CEO turnover and the absolute management forecast error even when managers beat their own forecasts. Therefore, our results suggest that management forecast accuracy is used as a distinct measure of CEO ability as opposed to an additional measure of overall firm performance.[2]

While our findings complement Zamora's (2009) evidence that strong forecasting ability increases a CFO's labor market value, our study expands the literature in several respects. First, our results suggest that even though the CEO's responsibilities are generally much broader than the CFO's financial or accounting duties, CEOs are also individually held responsible for forecast accuracy when boards deliberate on CEO dismissal. Thus, our study adds to the literature that documents CEO-specific effects on a firm's disclosure, financial reporting, and tax policies (Bamber et al. 2010; Dyreng et al. 2010) by providing evidence that firms use management forecast accuracy to assess the individual CEO's managerial ability. This evidence is consistent with Yang's (2012) evidence that the capital market incorporates the individual CEO's reputation for accuracy in assessing the information content of a management forecast. Second, despite the high costs of CEO replacement, we find that CEO turnover is negatively related to forecast accuracy. This finding suggests that forecasting accuracy is a relatively powerful signal regarding CEO ability. Third, our evidence that the relation between forecast accuracy and CEO turnover is stronger when performance is poor provides insight into how boards use forecast accuracy in conjunction with general performance measures to assess the CEO's managerial ability. Fourth, by documenting that the relation between forecast accuracy and CEO turnover is stronger when the CEO is less entrenched, our study also provides insight into how corporate governance structure affects boards' use of forecasting accuracy in evaluating the CEO. Finally, our study has implications for CEOs' expectations management strategy. If CEOs issue overly pessimistic forecasts to create positive earnings surprises on the earnings announcement date (Cotter et al. 2006), then our evidence suggests that this strategy may backfire because intentionally biasing a forecast reduces its accuracy and increases the probability of CEO turnover.

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[2] For example, Mergenthaler et al. (2011) find that CEO turnover increases when the firm misses the consensus analyst forecast. One interpretation of their finding is that missing the analyst forecast is a measure of poor firm performance. In contrast, our results suggest that CEO turnover increases with absolute management forecast errors even when the firm does not miss the management forecast.

The rest of our paper is organized as follows. We develop our hypotheses in Section II and discuss our research design in Section III. Section IV presents the empirical results and Section V summarizes and concludes the paper.

II. HYPOTHESIS DEVELOPMENT

Prior studies document evidence that management uses voluntary disclosures to reduce information asymmetry between managers and investors (Ajinkya and Gift 1984; Ajinkya et al. 2005; Karamanou and Vafeas 2005; Lennox and Park 2006; Hui et al. 2009), manage earnings expectations (Matsumoto 2002; Cotter et al. 2006), reduce litigation costs (Skinner 1994; Kasznik and Lev 1995; Skinner 1997; Soffer et al. 2000; Field et al. 2005; Rogers and Buskirk 2009), or affect stock-based compensation (Noe 1999; Aboody and Kasznik 2000; Cheng and Lo 2006). All of these studies focus on the disclosure decision and do not recognize differences in managers' forecasting ability.

Trueman (1986) argues that managers' ability to anticipate and respond to future events is valued in the equity market and that managers can signal their ability by issuing a forecast. Trueman's (1986) model shows that a separating equilibrium exists in which only high-ability managers issue forecasts. However, in reality we would expect to observe some low-ability managers issuing forecasts, either because they have overconfidence in their own ability, or they want to mimic managers of high ability. In this case, we expect the accuracy of management's forecast to reflect the CEO's ability. Forecasts are likely to be more accurate when the manager has the ability to develop and maintain a high-quality accounting information system that provides reliable data, and has the ability to accurately anticipate changes in business environment and their impact on earnings. Thus, we expect management forecast accuracy to serve as an observable measure of the CEO's managerial ability to adapt to a changing economic environment.

The evidence documented by Zamora (2009) supports this theory by showing that forecasting ability is associated with a CFO's labor market value. Specifically, Zamora (2009) finds that CFOs who show superior forecasting ability are more likely to have a higher reservation wage or to take a career-advancing opportunity such as an internal promotion or a move to another firm at an equal or higher position. Since CFOs are generally focused on financial management, including communications with capital market participants, the ability to forecast earnings is directly related to the CFO's primary job responsibilities. In contrast, financial management is just one aspect of the CEO's broader set of responsibilities. Thus, it is an empirical question as to whether boards consider management forecast accuracy as an indicator of the CEO's managerial ability and incorporate forecast accuracy in the CEO turnover decision.

If management forecast accuracy signals a CEO's ability to manage the firm, then the board of directors is likely to hold the CEO responsible for the accuracy and likely to incorporate that information into their decision to replace or retain the CEO. Specifically, boards are more likely to retain CEOs who issue accurate forecasts and more likely to replace CEOs who issue inaccurate forecasts. Since management earning forecast accuracy is inversely related to the absolute value of the forecast error, our first hypothesis (stated in alternative form) is:

> **H1:** *Ceteris paribus*, the probability of CEO turnover is positively related to the absolute management forecast error.

H1 predicts that boards are likely to penalize CEOs for issuing inaccurate forecasts. However, prior literature suggests that boards provide CEOs with incentives to issue pessimistic forecasts in order to generate beatable earnings targets (Richardson et al. 2004; Cotter et al. 2006) or with incentives to issue optimistic forecasts to increase analyst coverage or favorable press coverage (McNichols and O'Brien 1997). The extent to which boards reward CEOs who intentionally issue inaccurate forecasts would reduce the power of our tests.

While management forecast accuracy is likely to signal the CEO's managerial ability, the costs associated with removing a CEO, including contractual severance payments and costs from the disruption to the firm's operations, are high and management forecast accuracy is likely to be evaluated in conjunction with the firm's overall performance. If the firm's overall performance is good, then the board is unlikely to change the leadership of the firm and incur the associated costs even if the management forecast is inaccurate. On the other hand, if firm performance is poor, then the board is likely to consider management forecast accuracy as an additional signal to confirm that the poor performance is attributable to the CEO's poor managerial ability. Specifically, when performance is poor, an accurate management earnings forecast is likely to temper the board's concerns regarding the CEO's ability, whereas an inaccurate forecast is likely to reinforce the board's concerns.

Based on the above discussion, we form the second hypothesis:

> **H2:** *Ceteris paribus*, the positive relation between the probability of CEO turnover and the absolute management forecast error is stronger when firm performance is poor than when firm performance is good.

The first two hypotheses assume that boards of directors choose to retain or remove CEOs based on signals regarding the ability of the CEO. However, CEO entrenchment may make it difficult for the board to replace a CEO of poor ability. Specifically, the board may not be able to remove an entrenched CEO who holds relatively large blocks of voting stock or has the power to influence the selection of outside board members, even if the board observes negative signals of the CEO's ability. In contrast, when CEOs are less entrenched, boards are more likely to replace CEOs who are deemed to have poor managerial ability. As a result, we expect less entrenched CEOs to be more vulnerable to dismissal when their forecasts are inaccurate. Thus, our third hypothesis is:

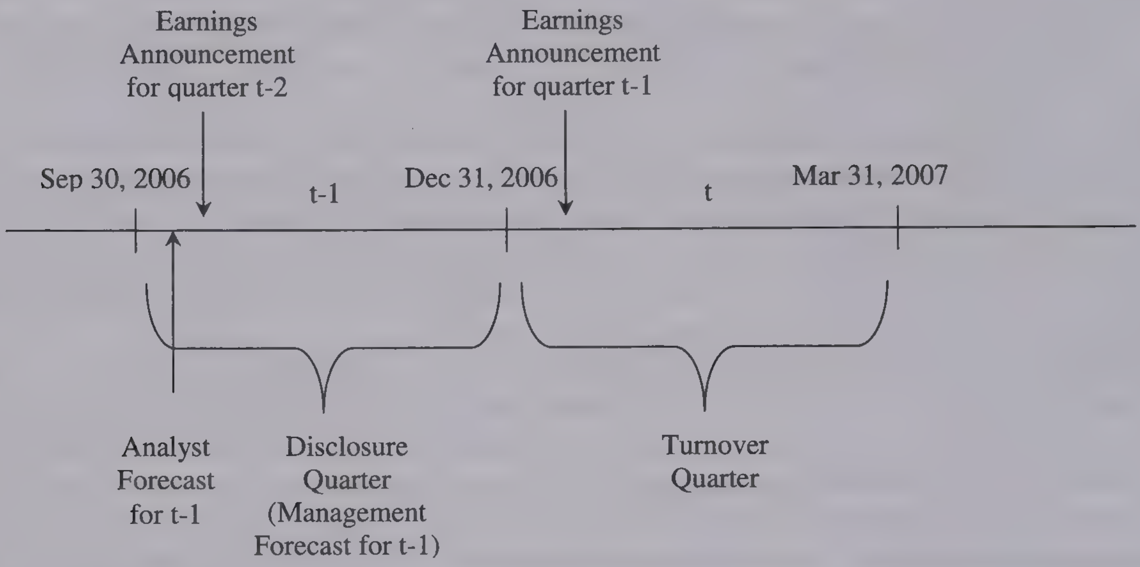
> **H3:** *Ceteris paribus*, the positive relation between the probability of CEO turnover and the absolute management forecast error is more likely to hold when the CEO is less entrenched.

III. RESEARCH DESIGN

Sample Selection and Descriptive Data

We draw our sample from the intersection of the ExecuComp, First Call, Compustat, CRSP, and Thompson databases for the years 1996 to 2006. We use the ExecuComp database to identify CEOs and CEO turnover. We collect management earnings forecasts from the First Call database. Each sample observation must have accounting and stock market data in the Compustat and CRSP databases and institutional ownership data in the Thompson database.

FIGURE 1
Timeline


Earnings Announcement for quarter t-2
Earnings Announcement for quarter t-1
Sep 30, 2006
t-1
Dec 31, 2006
t
Mar 31, 2007
Analyst Forecast for t-1
Disclosure Quarter (Management Forecast for t-1)
Turnover Quarter

This figure illustrates the sequence of events for our study. For each potential turnover quarter *t*, we examine the management forecast for quarter *t*–1 (disclosure quarter). We include a management forecast for quarter *t*–1 in the disclosure decision analysis if the firm issued the forecast during the period from the beginning of quarter *t*–1 to the end of quarter *t*–1. We measure forecast accuracy by the absolute value of the management forecast error that is measured by actual EPS minus the first management forecast EPS in quarter *t*–1, scaled by price at the beginning of the quarter *t*–1.

These procedures yield 30,481 firm-quarter observations, of which 843 (2.8 percent) have CEO turnover.[3]

Figure 1 illustrates a timeline for a firm that chooses to retain or replace the CEO during the first quarter of 2007. We label the first quarter of 2007 as potential "turnover quarter" (quarter *t*) and label the fourth quarter of 2006 as the "disclosure quarter" (quarter *t*–1) during which the manager chooses to issue a forecast for that quarter. We exclude management forecasts that are issued after the end of the fiscal quarter because they often represent preliminary earnings announcements rather than earnings forecasts (e.g., Skinner 1997; Rogers and Stocken 2005). A management earnings forecast can take a form of point, range, or open-ended forecast. We do not include open-ended forecasts because their forecast errors are not well defined. Our sample includes 5,312 (76 percent) range forecasts and 1,636 (24 percent) point forecasts.[4] Among the forecast observations, 18.4 percent issue multiple earnings forecasts in the form of point or range during the quarter. We choose the first forecast if managers issue the multiple forecasts. Our results remain qualitatively unchanged when we choose the last forecast.

We assume that when the board makes a decision on CEO turnover in quarter *t*, the board considers the accuracy of the management earnings forecast for quarter *t*–1 that is issued during quarter $t-1$.[5] This assumption allows us to link the turnover action in quarter t directly to the management forecast error in quarter $t-1$. However, this design assumes a relatively quick reaction by the board to the management forecast signal. To allow for the possibility of the board's delayed reaction to the forecast error, we extend the turnover window to encompass two quarters, quarter t and quarter $t+1$. We find qualitatively similar results. Alternatively, we relax the restriction of relying on the management forecast error of quarter $t-1$ and use the average management forecast error over quarters $t-4$ to $t-1$.[6] The results are robust to this change.

---

[3] The quarterly turnover rate of 2.8 percent is similar to the 3 percent rate in Dikolli et al. (2009) and corresponds to an annual rate of approximately 11.2 percent, which is consistent with the rates documented in prior CEO turnover studies such as Goyal and Park (2002).

[4] In a supplemental untabulated test we compare results for point forecasts to the results for range forecasts and do not find a significant difference.

We measure management earnings forecast accuracy using the absolute value of the forecast error. The smaller the absolute forecast error, the more accurate the forecast. We define the management forecast error for the disclosure quarter $t-1$ as the difference between actual earnings per share for quarter $t-1$ and the management earnings forecast for the quarter, scaled by the share price at the beginning of quarter $t-1$.

Panel A of Table 1 presents the CEO turnover rates based on the management forecast error. We observed a management forecast in 6,948 (22.8 percent) of the 30,481 firms-quarters. For the 23,533 quarters without a management forecast, the unconditional turnover rate of 2.72 percent is close to the overall average of 2.77 percent. When we partition the sample into quintile groups based on the magnitude of the forecast error, we find that the turnover rate is greater for firms in the lowest forecast error quintile (actual earnings are below the forecast) and for firms in the highest forecast error quintile (actual earnings are above the forecast). This is consistent with the prediction that CEO turnover rates are negatively related to management earnings forecast accuracy.

We then split the sample based on firm performance in quarter $t-1$ (the disclosure quarter). As shown in Figure 1, to measure firm performance we use the most recent analyst forecast for quarter $t-1$ issued prior to the earnings announcement date for quarter $t-2$ and define analyst forecast error as the difference between the actual earnings for quarter $t-1$ and the analyst forecast.[7]

Panel B of Table 1 presents the turnover rates for firms missing the analyst forecast (i.e., poor-performance firms) and Panel C of Table 1 presents the turnover rates for firms meeting or beating the analyst forecast (i.e., good-performance firms). Consistent with the findings of Dikolli et al. (2009), the turnover frequencies for poor-performance firms are higher than those for good-performance firms. We also note that for good-performance firms (Panel C) the turnover rates for each quintile group are generally close to the turnover rates for firms that do not issue management forecasts. In addition, we find that the differences between the turnover rate for the most accurate management forecast quintile (Quintile 3) and the extreme management forecast error quintiles (Quintiles 1 and 5) groups are larger for poor-performance firms than for good-performance firms. This result is consistent with the prediction that management forecast accuracy is a more useful signal regarding a CEO's managerial ability when firm performance is poor than when firm performance is good.

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[5] We assume that the board is aware of the actual earnings for quarter $t-1$ at the time they make the CEO retention decision. Although the public earnings announcement is likely to occur 10 to 45 days after the end of quarter $t-1$, the board is likely to have preliminary numbers before they are released to the public.

[6] We find that the correlation between the management forecast error over quarters $t-4$ to $t-2$ and quarter $t-1$ is 0.421. The persistence of forecast accuracy is consistent with CEOs with higher forecasting talent consistently producing more accurate forecasts.

[7] There is the possibility that our measure of firm performance, analyst forecast error, is contaminated by the management forecast. To address this issue, we conduct several sensitivity checks. First, we drop 254 observations where the management forecast is issued before the prior earnings announcement date. Second, we drop 178 observations where the management forecast is issued before the analyst forecast. Third, we use the market return or the earnings change measured over the prior 12 months as alternative performance measures. The results are robust to each of these sensitivity checks.

TABLE 1

Turnover Rates and Management Forecast Outcomes

Panel A: Turnover Frequencies and Management Forecast Error

	Lowest MFE Quintile 1	Quintile 2	Most Accurate Quintile 3	Quintile 4	Highest MFE Quintile 5	No Forecast	Total
TURN = 1	49	41	37	34	42	640	843
TURN = 0	1,340	1,349	1,524	1,185	1,347	22,893	29,638
Total	1,389	1,390	1,561	1,219	1,389	23,533	30,481
Turnover Ratio	3.53%	2.95%	2.37%	2.79%	3.02%	2.72%	2.77%

Panel B: Turnover Frequencies for Poor Performance

	Lowest MFE Quintile 1	Quintile 2	Most Accurate Quintile 3	Quintile 4	Highest MFE Quintile 5	No Forecast	Total
TURN = 1	29	20	23	14	22	310	418
TURN = 0	543	552	720	387	550	9,592	12,344
Total	572	572	743	401	572	9,902	12,762
Turnover Ratio	5.07%	3.50%	3.10%	3.49%	3.85%	3.13%	3.28%

Panel C: Turnover Frequencies for Good Performance

	Lowest MFE Quintile 1	Quintile 2	Most Accurate Quintile 3	Quintile 4	Highest MFE Quintile 5	No Forecast	Total
TURN = 1	20	21	14	20	20	330	425
TURN = 0	797	797	804	798	797	13,301	17,294
Total	817	818	818	818	817	13,631	17,719
Turnover Ratio	2.45%	2.57%	1.71%	2.44%	2.45%	2.42%	2.40%

This table provides data on the unconditional quarterly CEO turnover rates based on the quintiles of management forecast errors. The lowest quintile indicates that actual earnings for the quarter are less than management forecasts. The highest quintile indicates that actual earnings for the quarter are greater than management forecasts. The middle quintile indicates the most accurate management forecast group. A timeline for the measurement of management forecasts is illustrated in Figure 1. *TURN* is 1 if the CEO leaves the firm during the turnover quarter, and 0 otherwise. The classification of good or poor performance is based on the analyst forecast error that is measured as the difference between actual earnings per share and the most recent analyst forecast of earnings per share for the disclosure quarter issued prior to the earnings announcement date for the prior quarter. Performance is good (poor) if actual earnings meet or beat (miss) analyst earnings forecasts.

Research Design

The impact of forecast outcomes on CEO turnover is conditional on management's decision to forecast earnings. Thus, we incorporate the forecast decision into our research design to control for a potential selection bias. Specifically, we follow Pandit et al. (2012) and estimate a seemingly unrelated bivariate probit with a stacked model.[8] In this design, we model both the management forecast and turnover decisions, include the disclosure dependent variable from the first model as an independent variable in the second model, and jointly estimate the two regressions. This procedure adjusts for the endogeneity of the forecast issuance decision and for the fact that forecast errors are conditional on the issuance of a forecast. Because the dependent variable of the forecast decision model is an independent variable in the turnover decision, it may be more appropriate to use a recursive simultaneous equations model. However, Greene (2000, 852–853; Greene 2002, 32, see E17) indicates that the recursive simultaneous equations model can be estimated using the seemingly unrelated bivariate probit model because their likelihood functions are identical (Maddala 1983, 123).[9]

Our empirical model for the issuance of a management forecast is based on Lennox and Park (2006) who focus on management's incentive to issue forecasts to reduce information asymmetry. We augment their model by adding controls for CEO tenure, corporate governance, and ownership structure. Specifically, we estimate the following regression:

$$\begin{aligned} MF_{it-1} = {} & \alpha_0 + \alpha_1 Prev\_Forecast_{it-1} + \alpha_2 Abs\_CXR_{it-1} + \alpha_3 LnMV_{it-1} + \alpha_4 Num\_Analysts_{it-1} \\ & + \alpha_5 MB_{it-1} + \alpha_6 Earn\_Vol_{it-1} + \alpha_7 Ret\_Vol_{it-1} + \alpha_8 High\_Tech_{it-1} + \alpha_9 REG_{it-1} \\ & + \alpha_{10} Tenure_{it-1} + \alpha_{11} Inst\_Own_{it-1} + \alpha_{12} CEO\_Own_{it-1} + \alpha_{13} Chair\_Dual_{it-1} + e_1, \end{aligned} \tag{1}$$

where:

MF = 1 if the firm issues an earnings forecast during quarter *t*–1, and 0 otherwise;
Prev_Forecast = number of quarters for which the firm issued a forecast over quarters *t*–4 through *t*–2;
Abs_CXR = absolute value of cumulative market adjusted stock returns over the 62-day window that ends on the day after the earnings announcement for quarter *t*–1;
LnMV = natural log of the market value of common equity at the beginning of quarter *t*–1;
Num_Analysts = number of analysts following the firm prior to the earnings announcement date for quarter *t*–1;
MB = market-to-book value of common equity at the beginning of quarter *t*–1;
Earn_Vol = variance of changes in quarterly earnings per share (EPS), scaled by assets per share at the beginning of quarter *t*–1. We measure the change in quarterly EPS as the quarter *t*–1 EPS minus the EPS of the same quarter for the prior year, and use the 16 most recent quarters that have data for earnings changes;
Ret_Vol = variance in daily raw stock returns over the 250 trading days prior to the beginning of quarter *t*–1. We require a minimum of 100 daily stock return observations to compute the variance;
High_Tech = 1 if the firm reports Compustat SIC codes 2833–2836 (Drugs), 8731–8734 (R&D services), 7371–7379 (Programming), 3570–3577 (Computers), or 3600–3674 (Electronics), and 0 otherwise;

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[8] Pandit et al. (2012) examine the relation between analyst cash flow forecasts and analyst turnover. In the stacked model we adopt, when the dummy variable for the issuance of a management forecast (*MF*) has the value of 1, the turnover equation includes management forecast outcomes and our other independent variables. In contrast, when *MF* is equal to 0, the turnover equation does not include management forecast outcomes. A Heckman-style selection model is not appropriate because it assumes that turnover in the second stage is observed only for the selected case in which *MF* is equal to 1.

[9] Greene (2000) points out that simultaneity can be ignored in a bivariate probit regression. In our setting his point means that we can ignore the endogenous nature of the management forecast variable in formulating the log-likelihood of the turnover equation.

REG = 1 if the firm reports Compustat SIC codes 4812–4813 (Telephone), 4833 (TV), 4841 (Cable), 4811–4899 (Communications), 4922–4924 (Gas), 4931 (Electricity), 4941 (Water), or 6021–6023, 6035–6036, 6141, 6311, 6321, 6331 (Financial firms), and 0 otherwise;

Tenure = number of years that the CEO has held the position of chief executive officer as of the beginning of the fiscal year containing quarter *t*–1;

Inst_Own = percentage of outstanding shares owned by institutions at the beginning of quarter *t*–1; if missing, we assume institutional ownership is not reported because of minimal institutional holdings, and we set it to 0;

CEO_Own = ownership of the CEO as of the beginning of the fiscal year containing quarter *t*–1; if missing, we assume CEO ownership is not reported because of minimal CEO holdings, and we set it to 0; and

Chair_Dual = 1 if the CEO has the dual positions of chairman at the beginning of the fiscal year containing quarter *t*–1, and 0 otherwise.

This first-stage regression serves as an empirical model of the probability that firm *i* issues a forecast for quarter *t*–1 during quarter *t*–1. We include the *Prev_Forecast* variable to control for the firm's overall disclosure policy. We expect managers who have issued forecasts in the past to be more likely to issue a forecast in the current quarter. Thus, we predict the coefficient on *Prev_Forecast* to be positive.

Prior research finds that the management forecast decision is associated with the magnitude of earnings surprises and future earnings revisions, earnings informativeness, information environment, demand for information, risk, litigation, corporate governance, and ownership structure (Kasznik and Lev 1995; Frankel et al. 1995; Skinner 1994; Lennox and Park 2006; Ajinkya et al. 2005; Karamanou and Vafeas 2005). We include *Abs_CXR* to simultaneously control for earnings surprises and revisions in future earnings forecasts, and earnings informativeness.[10] We include *LnMV* to control for the information environment, *Num_Analysts*, *MB*, and *REG* to control for the information demand, *Earn_Vol* and *Ret_Vol* to control for risk, and *High_Tech* to control for litigation. Additionally, we include *Tenure* and *Chair_Dual* to capture the CEO's power in the governance structure, *Inst_Own* to measure the extent of external monitoring, and *CEO_Own* to control for management's incentives to disclose information (Ali et al. 2007; Chen et al. 2008).[11]

The second-stage regression models CEO turnover as follows:

$$\begin{aligned} Turnover_{it} = {} & \beta_0 + \beta_1 Abs(MFE)_{it-1}{*}MF_{it-1} + \beta_2 MF_{it-1} + \beta_3 AFE_{it-1} + \beta_4 ROE_{it-1} \\ & + \beta_5 CAR_{it-1} + \beta_6 Earn\_Vol_{it-1} + \beta_7 Ret\_Vol_{it-1} + \beta_8 LnSALES_{it-1} + \beta_9 AGE_{it-1} \\ & + \beta_{10} AGE65_{it-1} + \beta_{11} Tenure_{it-1} + \beta_{12} Inst\_Own_{it-1} + \beta_{13} CEO\_Own_{it-1} \\ & + \beta_{14} Chair\_Dual_{it-1} + e_2, \end{aligned} \tag{2}$$

where:

Turnover = 1 if the CEO leaves the firm during quarter *t*, and 0 otherwise;

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[10] A control for earnings informativeness measured by the earnings response coefficient requires extensive time-series of earnings data and drastically reduces the sample observations available for our tests. Thus, we choose *Abs_CXR* as a composite measure of earnings surprises, revisions in future earnings forecasts, and earnings informativeness by relying on the prior capital market literature that documents that a firm's abnormal stock returns are a function of those three variables (e.g., Collins and Kothari 1989; Cornell and Landsman 1989).

[11] The results are qualitatively similar when we include the G-index (Gompers et al. 2003) in the management forecast and CEO turnover equations as an additional measure of corporate governance quality.

Abs(*MFE*) = absolute value of the management forecast error that is measured as actual earnings per share minus the first management forecast of earnings per share during the disclosure quarter (quarter *t*–1), scaled by the share price at the beginning of quarter *t*–1;[12]

MF = 1 if the firm issued an earnings forecast for quarter *t*–1, and 0 otherwise,

AFE = analyst forecast error that is measured as the difference between actual earnings per share for quarter *t*–1 and the most recent analyst forecast of earnings per share for quarter *t*–1 issued prior to the earnings announcement date for quarter *t*–2, scaled by the share price at the beginning of quarter *t*–1;

ROE = firm's ROE minus industry ROE over the four quarters prior to quarter *t*. The firm's ROE is calculated as the sum of earnings for quarters *t*–5 through *t*–1, scaled by common equity at the beginning of quarter *t*–4. Industry ROE is calculated as the median ROE of firms in the same industry (based on two-digit SIC codes) for the same period;

CAR = the cumulative market-adjusted stock returns over 12 months prior to the turnover quarter;

LnSALES = natural log of the sum of sales for quarters *t*–5 through *t*–1;

AGE = age of the CEO at the beginning of the fiscal year including quarter *t*; and

AGE65 = 1 if the age of the CEO = 64, 65, or 66, and 0 otherwise.

All other variables are as previously defined in Equation (1).

H1 states that the probability of CEO turnover is positively related to the absolute management forecast error and predicts that β_1, the coefficient for $Abs(MFE) * MF$, is positive. We include *MF*, the dependent variable in the first-stage regression, as a control variable in the second stage to adjust for the endogenous decision to issue a forecast and for the fact that the test variable is only observed when the firm issues a forecast.

We use analyst forecast errors, industry-adjusted earnings, and market-adjusted returns to control for firm performance and *Earn_Vol* and *Ret_Vol* to control for risk (Coughlan and Schmidt 1985; Warner et al. 1988; Murphy and Zimmerman 1993; DeFond and Park 1999; Engel et al. 2003; Farrell and Whidbee 2003). We also include CEO age and tenure as control variables along with a dummy variable indicating whether CEO age is 64, 65 or 66 to capture normal retirement ages near 65 (Weisbach 1988; Murphy and Zimmerman 1993; DeFond and Park 1999).[13] The log of sales is included to control for firm size (Farrell and Whidbee 2003). Finally, we include *Tenure*, *Chair_Dual*, and *CEO_Own* to control for CEO power, and *Inst_Own* to control for the strength of external monitoring. To reduce the influence of extreme observations we winsorize each continuous variable at the 1 percent and 99 percent values of its distribution. We cluster the standard errors by firm to reduce the potential understatement of standard errors that could result from the use of multiple observations for the same firm.[14]

Given that we use a simultaneous equations system, the identification of Equation (2) requires that the number of exogenous variables excluded from Equation (2) must be at least as large as the number of endogenous variables included in Equation (2) (Greene 2000, 670). Equation (2) has two endogenous variables, *MF* and $Abs(MFE) * MF$, and Equation (1) has seven exogenous variables that are excluded from Equation (2).[15] Thus, Equation (2) is identified. In our stacked bivariate system, Equation (1) explicitly models the decision to issue a forecast. If management believes that an inaccurate forecast will increase the probability of turnover, then it is important to incorporate the expected accuracy of forecasts in the decision model. Earnings are more likely to be difficult to predict as earnings or stock returns are more volatile. We therefore consider the risk variables, *Earn_Vol* and *Ret_Vol*, as proxies for the expected forecast error.[16]

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[12] As a sensitivity check we also deflate by total assets per share instead of share price. The results are qualitatively similar.

[13] The reasons for CEO changes stated in press releases are often not reliable. Thus, consistent with prior studies (Weisbach 1988; Murphy and Zimmerman 1993; DeFond and Park 1999), we include all turnover firms in our test. While the inclusion of routine CEO turnover reduces the power of tests, the use of age and age dummy variables mitigate this concern. We also conduct our regressions dropping all CEOs aged 63 or older from the sample. Our results are robust.

[14] This procedure controls for serial correlation in all our analyses. Correcting cross-sectional correlation provides qualitatively the same results.

To test H2, we expand the CEO turnover regression by adding a variable that interacts a dummy variable for poor performance (*Dummy_Poor*) with *Abs*(*MFE*). We also include *Dummy_Poor* to control for its main effect on CEO turnover. Specifically, we estimate the following second-stage regression:

$$\begin{aligned} Turnover_{it} = {} & \beta_0 + \beta_1 Abs(MFE)_{it-1}{*}MF_{it-1} + \beta_2 Abs(MFE)_{it-1}{*}MF_{it-1}{*}Dummy\_Poor_{it-1} \\ & + \beta_3 MF_{it-1} + \beta_4 Dummy\_Poor_{it-1} + \beta_5 AFE_{it-1} + \beta_6 ROE_{it-1} + \beta_7 CAR_{it-1} \\ & + \beta_8 EarnVol_{it-1} + \beta_9 Ret\_Vol_{it-1} + \beta_{10} LnSALES_{it-1} + \beta_{11} AGE_{it-1} \\ & + \beta_{12} AGE65_{it-1} + \beta_{13} Tenure_{it-1} + \beta_{14} Inst\_Own_{it-1} + \beta_{15} CEO\_Own_{it-1} \\ & + \beta_{16} Chair\_Dual_{it-1} + e_3, \end{aligned} \tag{3}$$

where:

Dummy_Poor = 1 if AFE_{it-1} is negative (i.e., the firm reports earnings for quarter *t*–1 below the analyst forecast), and 0 otherwise.

All the other variables are defined as in Equations (1) and (2).

H2 states that the positive relation between the probability of CEO turnover and the absolute management forecast error is stronger when firm performance is poor than when firm performance is good. In Equation (3), β_2, the coefficient on the interaction of the absolute management forecast error with poor performance, captures the *incremental* effect of management forecast accuracy on CEO turnover for poor performing firms relative to good performing firms. We also report the *full* effect of management forecast accuracy on CEO turnover for poorly performing firms by adding β_1 and β_2. We expect both β_2 and $\beta_1 + \beta_2$ to be positive.

H3 states that the positive relation between the probability of CEO turnover and the absolute management forecast error is more likely to hold when the CEO is less entrenched. To test H3, we use CEO ownership percentage, CEO/Chairman duality, and CEO tenure to measure CEO entrenchment. Using each measure of CEO entrenchment, we estimate the following second-stage regression separately:

$$\begin{aligned} Turnover_{it} = {} & \gamma_0 + \gamma_1 Abs(MFE)_{it-1}{*}MF_{it-1} + \gamma_2 Abs(MFE)_{it-1}{*}MF_{it-1}{*}Less\_Entrenchment_{it-1} \\ & + \gamma_3 MF_{it-1} + \gamma_4 Less\_Entrenchment_{it-1} + \gamma_5 AFE_{it-1} + \gamma_6 ROE_{it-1} + \gamma_7 CAR_{it-1} \\ & + \gamma_8 Earn\_Vol_{it-1} + \gamma_9 Ret\_Vol_{it-1} + \gamma_{10} LnSALES_{it-1} + \gamma_{11} AGE_{it-1} \\ & + \gamma_{12} AGE65_{it-1} + \gamma_{13} Tenure_{it-1} + \gamma_{14} Inst\_Own_{it-1} + \gamma_{15} CEO\_Own_{it-1} \\ & + \gamma_{16} Chair\_Dual_{it-1} + e_4, \end{aligned} \tag{4}$$

where:

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[15] The seven variables are *Prev_Forecast*, *Abs_CXR*, *LnMV*, *Num_Analysts*, *MB*, *High_Tech*, and *REG*. We find that none of the correlations between *Prev_Forecast* (*REG*) and the other exogenous variables in Equations (1) and (2) exceeds 0.18 (0.15) in absolute value. Thus, at minimum, the *Prev_Forecast* and *REG* variables that are in Equation (1) but excluded from Equation (2) are likely to serve as relatively efficient instruments for estimating the turnover equation.

[16] The pairwise correlations between each of the expected forecast error proxies and *ex post* forecast error are significantly positive. Given the high correlation (0.421) between management forecast error over quarters *t*–4 to *t*–2 and quarter *t*–1, we include the previous forecast accuracy over quarters *t*–4 to *t*–2 as an alternative proxy for expected accuracy. The results are robust to this sensitivity test.

Less_Entrenchment = 1 if CEO is less entrenched (i.e., CEO ownership percentage falls within the sample bottom quartile, or the CEO does not have the dual title, or CEO tenure falls within the sample bottom quartile), and 0 otherwise.[17]

All the other variables are defined as in Equations (1) and (2).

H3 predicts that both γ_2 and $\gamma_1 + \gamma_2$ in Equation (4) are significantly positive.

IV. EMPIRICAL RESULTS

Main Tests

Table 2 provides data on the distributions of the variables used in our empirical tests. Panel A of Table 2 includes the variables for the management forecast issuance model and Panel B of Table 2 includes the additional variables for the CEO turnover model. The mean value for *MF* of 0.228 indicates that sample firms issue a forecast in 22.8 percent of the quarters. Approximately 68.6 percent of our sample quarters includes a forecast for at least one of the previous three quarters. Among other variables, we note that the mean (median) tenure for CEOs in our sample is 8.3 (6.0) years, and the average age of the CEOs is approximately 55.5 years.

We present the results from the joint estimation of the bivariate probit regressions in Table 3. The results from the management forecast issuance model are generally consistent with our predictions. The significantly positive coefficient for *Prev_Forecast* indicates that firms are more likely to issue a forecast for a given quarter if they had issued forecasts in the prior quarters. As expected, firms that have a greater analyst following and higher institutional ownership are more likely to issue a forecast. This result is consistent with firms providing forecasts to fulfill a greater demand for information.

We use the second-stage turnover regressions (i.e., Equations (2), (3) and (4)) to test our hypotheses. The results in Table 3 support H1, which predicts that the probability of CEO turnover is positively related to the absolute value of management's forecast errors. The coefficient on the absolute management forecast error is significantly positive at the 1 percent level. This result is consistent with the board using management forecast errors as a signal of the CEO's ability to anticipate and adapt to changes in the business environment. The result also suggests that the labor market penalizes managers who issue inaccurate forecasts. The insignificant coefficient for *MF* indicates that the issuance of a forecast, in itself, does not have a significant effect on CEO turnover.[18] We also note that the coefficient for each of our measures of firm performance is significantly negative, consistent with prior studies that document an inverse relation between CEO turnover and firm performance (Coughlan and Schmidt 1985; Weisbach 1988; Puffer and Weintrop 1991; DeFond and Park 1999).

H2 predicts that the positive relation between CEO turnover and the absolute management forecast error is more pronounced when firm performance is poor. Table 4 presents the results from the management forecast issuance model and two CEO turnover regression models. The first CEO turnover model includes a dummy variable for negative analyst forecast error (*Dummy_Poor*) and excludes the continuous analyst forecast error. The second CEO turnover model includes both the negative forecast error dummy and continuous forecast error (*AFE*). In each model, the coefficient for $Abs(MFE) * MF$ is not significant but the coefficient for the interaction of $Abs(MFE) * MF$ and *Dummy_Poor*, which captures the incremental effect, is significantly positive at the 5 percent level.

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[17] When we use CEO ownership percentage (tenure) as an entrenchment variable, we do not include *CEO_Own* (*Tenure*) in the regression.

[18] We conduct a self-selection bias test with Equations (1) and (2) and find that the bias is not significant (Chi-square = 0.619, p = 0.431). This result is consistent with the insignificant coefficient for *MF*.

TABLE 2

Distributions of Variables

Panel A: Management Forecast Issuance Model

Variable	n	Mean	Median	Std. Dev.	Q1	Q3
MF	30,481	0.228	0.000	0.420	0.000	0.000
Prev_Forecast	30,481	0.686	0.000	1.058	0.000	1.000
Abs_CXR	30,481	0.128	0.094	0.115	0.043	0.176
MV ($ billions)	30,481	8.370	2.050	19.919	0.796	6.238
Num_Analysts	30,481	9.852	9.000	5.886	5.000	13.000
MB	30,481	3.182	2.338	2.689	1.616	3.705
Earn_Vol	30,481	0.023	0.002	0.070	0.001	0.011
Ret_Vol	30,481	0.077	0.051	0.077	0.028	0.093
High_Tech	30,481	0.166	0.000	0.372	0.000	0.000
REG	30,481	0.080	0.000	0.271	0.000	0.000
Tenure	30,481	8.301	6.000	7.515	3.000	11.000
Inst_Own	30,481	60.131	65.909	25.877	49.412	78.715
CEO_Own	30,481	1.934	0.280	4.787	0.084	1.090
Chair_Dual	30,481	0.637	1.000	0.481	0.000	1.000

Panel B: CEO Turnover Model

Variable	n	Mean	Median	Std. Dev.	Q1	Q3
Turnover	30,481	0.028	0.000	0.164	0.000	0.000
Abs(MFE)	30,481	0.015	0.000	0.107	0.000	0.000
AFE	30,481	−0.106	0.000	0.717	−0.181	0.127
ROE	30,481	0.010	0.003	0.152	−0.046	0.063
CAR	30,481	−0.086	−0.096	0.590	−0.324	0.129
SALES ($ billions)	30,481	5.269	1.634	9.972	0.681	4.786
AGE	30,481	55.509	56.000	7.020	51.000	60.000
AGE65	30,481	0.056	0.000	0.231	0.000	0.000

Variable Definitions:

MF = 1 if the firm issues an earnings forecast during quarter *t*–1, and 0 otherwise;

Prev_Forecast = number of quarters for which the firm issued a forecast over quarters *t*–4 through *t*–2;

Abs_CXR = absolute value of cumulative market-adjusted stock returns over the 62-day window that ends on the day following the earnings announcement for quarter *t*–1;

MV = market value of common equity at the beginning of quarter *t*–1;

LnMV = natural log of *MV*;

Num_Analysts = number of analysts following the firm prior to the earnings announcement date for quarter *t*–1;

MB = market-to-book value of common equity at the beginning of quarter *t*–1;

Earn_Vol = variance of changes in quarterly earnings per share (EPS), scaled by assets per share at the beginning of quarter *t*–1. We measure the change in quarterly EPS as the quarter *t*–1 EPS minus the EPS of the same quarter for the prior year, and use the 16 most recent quarters that have data for earnings changes;

Ret_Vol = variance in daily raw stock returns over the 250 trading days prior to the beginning of quarter *t*–1. We require a minimum of 100 daily stock return observations to compute the variance;

High_Tech = 1 if the firm reports Compustat SIC codes 2833–2836 (Drugs), 8731–8734 (R&D services), 7371–7379 (Programming), 3570–3577 (Computers), or 3600–3674 (Electronics), and 0 otherwise;

REG = 1 if the firm reports Compustat SIC codes 4812–4813 (Telephone), 4833 (TV), 4841 (Cable), 4811–4899 (Communications), 4922–4924 (Gas), 4931 (Electricity), 4941 (Water), or 6021–6023, 6035–6036, 6141, 6311, 6321, 6331 (Financial Firms), and 0 otherwise;

Tenure = number of years that the CEO has held the position of chief executive officer as of the beginning of the fiscal year containing quarter *t*–1;

(continued on next page)

TABLE 2 (continued)

Inst_Own = percentage of outstanding shares owned by institutions at the beginning of quarter *t*–1; if missing, we assume institutional ownership is not reported because of minimal institutional holdings, and we set it to 0;
CEO_Own = ownership of the CEO as of the beginning of the fiscal year containing quarter *t*–1; if missing, we assume CEO ownership is not reported because of minimal CEO holdings, and we set it to 0;
Chair_Dual = 1 if the CEO has the dual positions of chairman at the beginning of the fiscal year containing quarter *t*–1, and 0 otherwise;
Turnover = 1 if the CEO leaves the firm during quarter *t*, and 0 otherwise;
Abs(*MFE*) = absolute value of the management forecast error that is measured as actual earnings per share minus the first management forecast of earnings per share during the disclosure quarter (quarter *t*–1), scaled by the share price at the beginning of quarter *t*–1;
AFE = analyst forecast error that is measured as the difference between actual earnings per share for quarter *t*–1 and the most recent analyst forecast of earnings per share for quarter *t*–1 issued prior to the earnings announcement date for quarter *t*–2, scaled by the share price at the beginning of quarter *t*–1;
ROE = firm's ROE minus industry ROE over the four quarters prior to quarter *t*. The firm's ROE is calculated as the sum of earnings for quarter *t*–5 through *t*–1, scaled by common equity at the beginning of quarter *t*–4. Industry ROE is calculated as the median ROE of firms in the same industry (based on two-digit SIC code) for the same period;
CAR = cumulative market-adjusted stock returns over 12 months prior to the turnover quarter;
SALES = sum of sales for quarter *t*–5 through *t*–1;
LnSALES = natural log of *SALES*;
AGE = age of the CEO at the beginning of the fiscal year including quarter *t*;
AGE65 = 1 if the age of the CEO = 64, 65, or 66, and 0 otherwise; and
All other variables are as previously defined.

In addition, the sum of the coefficient for *Abs*(*MFE*) and the coefficient for the interaction variable, which captures the full effect, is also significantly positive at the 1 percent level.[19] These results indicate that the management forecast accuracy is most relevant to boards' decisions on CEO turnover when overall firm performance is poor and suggest that the forecast accuracy is used as a confirming signal that the firm's poor performance is attributable to the ability of the CEO.[20]

H3 states that the positive relation between the probability of CEO turnover and the absolute management forecast error holds when the CEO is less entrenched. Since there is no consensus measure for CEO entrenchment, we use three separate measures as proxies for CEO entrenchment. The first measure is the percentage of outstanding shares owned by the CEO. As CEO ownership decreases, the CEO exercises less power over board members and it becomes easier for the board to replace the CEO. We define *CEO_Own_Low* as 1 if CEO ownership percentage falls within the sample bottom quartile, and 0 otherwise. The results, presented in Panel A of Table 5 show that, consistent with H3, both the coefficient for the interaction of *Abs*(*MFE*) $*$ *MF* with *CEO_Own_Low*, γ_2, which captures the incremental effect of less entrenchment, and the sum of the coefficient for *Abs*(*MFE*) $*$ *MF* and the coefficient for the interaction, $\gamma_1 + \gamma_2$, which captures the full effect, are significantly positive.

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[19] As noted by Ai and Norton (2003), the interpretation of interaction effects in logit and probit models can be problematic. To address this issue, we employ the Stata procedure recommended by Norton et al. (2004). Our results are robust to the alternative procedure.

[20] Richardson et al. (2004) find that analysts tend to issue more optimistic earnings forecasts at the beginning of the forecasting period than later in the period. Thus, some firms may be misclassified as poorly performing because of the analysts' bias. Our evidence indicates that boards are not likely to dismiss CEOs who issue inaccurate forecasts if the firm performance is good. Thus, if good-performance firms are misclassified as poorly performing, then we are less likely to find the positive relation between CEO turnover and the absolute management forecast error in the poor-performance group. In addition, when we use alternative 12-month or 24-month performance measures, such as market returns and earnings changes to classify firms as poorly performing, our results remain robust.

TABLE 3

Bivariate Probit Analysis
Estimated Coefficients [z-statistic]

Management Forecast Issuance Model: Dependent Variable = 1 if the firm issues an earnings forecast for quarter $t-1$ (0 otherwise)		**Turnover Model: Dependent Variable = 1 if the CEO is replaced in quarter t (0 otherwise)**	
Prev_Forecast	0.855***	*Abs(MFE) ∗ MF*	0.440***
	[73.34]		[2.37]
Abs_CXR	0.576***	*MF*	0.017
	[6.27]		[0.28]
LnMV	−0.035***	*AFE*	−0.102***
	[2.72]		[4.99]
Num_Analysts	0.176***	*ROE*	−0.207**
	[6.68]		[1.88]
MB	0.008**	*CAR*	−0.161***
	[1.90]		[4.60]
Earn_Vol	−0.604***	*Earn_Vol*	0.259
	[2.68]		[1.09]
Ret_Vol	−0.025	*Ret_Vol*	0.618***
	[0.17]		[2.71]
High_Tech	0.086***	*LnSALES*	0.039***
	[2.60]		[3.41]
REG	−0.397***	*AGE*	0.033***
	[5.28]		[11.05]
Tenure	−0.003**	*AGE65*	0.370***
	[1.79]		[6.98]
Inst_Own	0.003***	*Tenure*	−0.001
	[6.96]		[0.49]
CEO_Own	−0.002	*Inst_Own*	−0.001**
	[0.72]		[1.97]
Chair_Dual	−0.011	*CEO_Own*	−0.022***
	[0.46]		[4.45]
		Chair_Dual	−0.052*
			[1.50]
n	30,481		30,481
Pseudo R^2	0.39		0.06

*, **, *** $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively (based on one-tailed levels of significance).

This table presents the results of bivariate probit regressions estimated simultaneously. The dependent variable for the first-stage regression, *MF*, is included as an independent variable in the second-stage (CEO turnover) regression. For parsimony, the intercepts are not reported in the table. Variable definitions are in Table 2. We adjust Z-statistics and p-values for time-series correlation.

Our second measure of entrenchment is CEO duality, indicating that the CEO also serves as the chairman of the board. CEOs who do not hold the dual positions are likely to wield a weaker influence over the board's decision on CEO replacement. We therefore define *Chair_Non_Dual* as equal to 1 if the CEO does not serve as board chair. The results in Panel B of Table 5 show that the incremental effect captured by γ_2 is insignificant even though the full effect captured by $\gamma_1 + \gamma_2$ is significantly positive when the CEO does not serve as chairman.

TABLE 4

Bivariate Probit Analysis Conditioning on Firm Performance
Estimated Coefficients [z-statistic]

Management Forecast Issuance Model: Dependent Variable = 1 if the firm issues an earnings forecast for quarter *t*−1 (0 otherwise)		**Turnover Model: Dependent Variable = 1 if the CEO is replaced in quarter *t* (0 otherwise)**		
Prev_Forecast	0.855***	$Abs(MFE) * MF$	0.109	0.160
	[73.34]		[0.46]	[0.67]
Abs_CXR	0.577***	$Abs(MFE) * MF * Dummy\_Poor$	0.725***	0.588**
	[6.27]		[2.52]	[2.00]
LnMV	−0.035***	*MF*	0.019	0.024
	[2.72]		[0.32]	[0.39]
Num_Analysts	0.176***	*Dummy_Poor*	0.072**	0.008
	[6.68]		[2.17]	[0.21]
MB	0.008**	*AFE*		−0.088***
	[1.90]			[3.59]
Earn_Vol	−0.605***	*ROE*	−0.280***	−0.212**
	[2.68]		[2.54]	[1.92]
Ret_Vol	−0.026	*CAR*	−0.170***	−0.157***
	[0.17]		[4.88]	[4.46]
High_Tech	0.086***	*Earn_Vol*	0.279	0.273
	[2.60]		[1.18]	[1.15]
REG	−0.397***	*Ret_Vol*	0.693***	0.624***
	[5.28]		[3.11]	[2.74]
Tenure	−0.003**	*LnSALES*	0.039***	0.039***
	[1.79]		[3.42]	[3.39]
Inst_Own	0.003***	*AGE*	0.033***	0.033***
	[6.96]		[11.17]	[11.09]
CEO_Own	−0.002	*AGE65*	0.368***	0.369***
	[0.73]		[6.96]	[6.96]
Chair_Dual	−0.011	*Tenure*	−0.001	−0.001
	[0.46]		[0.48]	[0.46]
		Inst_Own	−0.001**	−0.001**
			[2.12]	[1.96]
		CEO_Own	−0.022***	−0.022***
			[4.49]	[4.47]
		Chair_Dual	−0.052*	−0.053*
			[1.50]	[1.52]
n	30,481		30,481	30,481
Pseudo R^2	0.39		0.06	0.06
$Turnover_{it} = \beta_0 + \beta_1 Abs(MFE)_{it} * MF_{it} + \beta_2 Abs(MFE)_{it} * MF_{it} * Dummy\_Poor_{it} + \sum \rho_k Z_{kit} + \varepsilon_{it}.$				
Test: $\beta_1 + \beta_2 = 0$			0.834***	0.748***
			[3.48]	[3.09]

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*, **, *** $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively (based on one-tailed levels of significance).

This table presents the results of bivariate probit regressions estimated simultaneously. The dependent variable for the first-stage regression, *MF*, is included as an independent variable in the second-stage (CEO turnover) regression. For parsimony, intercepts are not reported in the table. *Dummy_Poor* = 1 if AFE is negative (i.e., the firm reports earnings below the analyst forecast), and 0 otherwise. Definitions of other variables are in Table 2. We adjust Z-statistics and p-values for time-series correlation.

TABLE 5

Bivariate Probit Analysis for CEO Entrenchment Estimated Coefficients [z-statistic]

Panel A: CEO Ownership

Management Forecast Issuance Model: Dependent Variable = 1 if the firm issues an earnings forecast for quarter $t-1$ (0 otherwise)		**Turnover Model: Dependent Variable = 1 if the CEO is replaced in quarter t (0 otherwise)**	
Prev_Forecast	0.855***	*Abs(MFE) ∗ MF*	0.158
	[73.34]		[0.67]
Abs_CXR	0.577***	*Abs(MFE) ∗ MF ∗ CEO_Own_Low*	0.795***
	[6.27]		[2.64]
LnMV	−0.035***	*MF*	0.033
	[2.73]		[0.55]
Num_Analysts	0.176***	*CEO_Own_Low*	0.393***
	[6.68]		[10.35]
MB	0.008**	*AFE*	−0.107***
	[1.90]		[5.27]
Earn_Vol	−0.605***	*ROE*	−0.245**
	[2.68]		[2.19]
Ret_Vol	−0.025	*CAR*	−0.152***
	[0.17]		[4.30]
High_Tech	0.086***	*Earn_Vol*	0.119
	[2.59]		[0.47]
REG	−0.397***	*Ret_Vol*	0.512**
	[5.28]		[2.24]
Tenure	−0.003**	*LnSALES*	0.015
	[1.78]		[1.28]
Inst_Own	0.003***	*AGE*	0.035***
	[6.96]		[11.60]
CEO_Own	−0.002	*AGE65*	0.394***
	[0.75]		[7.43]
Chair_Dual	−0.011	*Tenure*	−0.001
	[0.45]		[0.57]
		Inst_Own	−0.001
			[0.84]
		Chair_Dual	−0.039
			[1.12]
n	30,481		30,481
Pseudo R^2	0.39		0.06
$Turnover_{it} = \gamma_0 + \gamma_1 Abs(MFE)_{it} * MF_{it} + \gamma_2 Abs(MFE)_{it} * MF_{it} * CEO\_Own\_Low_{it} + \sum \rho_k Z_{kit} + \varepsilon_{it}$.			
Test: $\gamma_1 + \gamma_2 = 0$			0.953***
			[3.83]

(continued on next page)

Our third measure of CEO entrenchment is CEO tenure. CEOs with shorter tenure have had less time to influence the selection of board members and are therefore more vulnerable to being replaced based on signals of poor performance. We therefore define *Tenure_Low* as equal to 1 if the

TABLE 5 (continued)

Panel B: CEO/Chairman Dual Positions

Management Forecast Issuance Model: Dependent Variable = 1 if the firm issues an earnings forecast for quarter *t*−1 (0 otherwise)		**Turnover Model: Dependent Variable = 1 if the CEO is replaced in quarter *t* (0 otherwise)**	
Prev_Forecast	0.855***	*Abs(MFE)* ∗ *MF*	0.373*
	[73.34]		[1.72]
Abs_CXR	0.576***	*Abs(MFE)* ∗ *MF* ∗ *Chair_Non_Dual*	0.171
	[6.27]		[0.60]
LnMV	−0.035***	*MF*	0.017
	[2.72]		[0.28]
Num_Analysts	0.176***	*Chair_Non_Dual*	−0.045
	[6.68]		[1.20]
MB	0.008**	*AFE*	−0.102***
	[1.90]		[4.99]
Earn_Vol	−0.604***	*ROE*	−0.207**
	[2.68]		[1.88]
Ret_Vol	−0.025	*CAR*	−0.161***
	[0.17]		[4.59]
High_Tech	0.086***	*Earn_Vol*	0.263
	[2.60]		[1.10]
REG	−0.397***	*Ret_Vol*	0.616***
	[5.28]		[2.70]
Tenure	−0.003**	*LnSALES*	0.039***
	[1.79]		[3.41]
Inst_Own	0.003***	*AGE*	0.033***
	[6.96]		[11.04]
CEO_Own	−0.002	*AGE65*	0.370***
	[0.72]		[6.98]
Chair_Dual	−0.011	*Tenure*	−0.001
	[0.46]		[0.49]
		Inst_Own	−0.001**
			[1.95]
		CEO_Own	−0.022***
			[4.44]
n	30,481		30,481
Pseudo R^2	0.39		0.06
$Turnover_{it} = \gamma_0 + \gamma_1 Abs(MFE)_{it} * MF_{it} + \gamma_2 Abs(MFE)_{it} * MF_{it} * Chair\_Non\_Dual_{it} + \sum \rho_k Z_{kit} + \varepsilon_{it}.$			
Test: $\gamma_1 + \gamma_2 = 0$			0.544**
			[2.14]

(continued on next page)

CEO's tenure falls within the sample bottom quartile and predict that the interaction of *Tenure_Low* with *Abs(MFE)* is positive. Panel C of Table 5 presents the results. Consistent with H3, γ_2 and $\gamma_1 + \gamma_2$ are significantly positive when CEOs have shorter tenure.

Overall, the results in Table 5 generally support the prediction that the impact of management forecast accuracy on CEO turnover is stronger when CEO entrenchment is relatively low. In

TABLE 5 (continued)

Panel C: CEO Tenure

Management Forecast Issuance Model: Dependent Variable = 1 if the firm issues an earnings forecast for quarter *t*−1 (0 otherwise)		**Turnover Model: Dependent Variable = 1 if the CEO is replaced in quarter *t* (0 otherwise)**	
Prev_Forecast	0.855***	*Abs(MFE) ∗ MF*	0.229
	[73.34]		[1.04]
Abs_CXR	0.576***	*Abs(MFE) ∗ MF ∗ Tenure_Low*	0.710***
	[6.27]		[2.39]
LnMV	−0.035***	*MF*	0.02
	[2.72]		[0.33]
Num_Analysts	0.176***	*Tenure_Low*	−0.161***
	[6.68]		[3.81]
MB	0.008**	*AFE*	−0.104***
	[1.90]		[5.02]
Earn_Vol	−0.604***	*ROE*	−0.212**
	[2.68]		[1.89]
Ret_Vol	−0.025	*CAR*	−0.155***
	[0.17]		[4.41]
High_Tech	0.086***	*Earn_Vol*	0.268
	[2.60]		[1.11]
REG	−0.397***	*Ret_Vol*	0.630***
	[5.28]		[2.73]
Tenure	−0.003**	*LnSALES*	0.041***
	[1.80]		[3.50]
Inst_Own	0.003***	*AGE*	0.031***
	[6.96]		[10.89]
CEO_Own	−0.002	*AGE65*	0.366***
	[0.72]		[6.92]
Chair_Dual	−0.011	*Inst_Own*	−0.001**
	[0.46]		[2.12]
		CEO_Own	−0.024***
			[5.18]
		Chair_Dual	−0.070**
			[1.99]
n	30,481		30,481
Pseudo R^2	0.39		0.06
Test: $\gamma_1 + \gamma_2 = 0$			0.939***
			[3.70]

$$Turnover_{it} = \gamma_0 + \gamma_1 Abs(MFE)_{it} * MF_{it} + \gamma_2 Abs(MFE)_{it} * MF_{it} * Tenure\_Low_{it} + \sum \rho_k Z_{kit} + \varepsilon_{it}.$$

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*, **, *** $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively (based on one-tailed levels of significance).

This table presents the results of bivariate probit regressions estimated simultaneously. The dependent variable for the first-stage regression, *MF*, is included as an independent variable in the second-stage (CEO turnover) regression. For parsimony, the intercepts are not reported in the table. Definitions of other variables are in Table 2. We adjust Z-statistics and p-values for time-series correlation.

Variable Definitions:
CEO_Own_Low = 1 if CEO ownership percentage falls within the sample bottom quartile, and 0 otherwise;
Chair_Non_Dual = 1 if the CEO does not have the Chairman title, and 0 otherwise; and
Tenure_Low = 1 if CEO tenure falls within the sample bottom quartile, and 0 otherwise.

untabulated tests we conduct sensitivity tests using three alternative measures of CEO entrenchment that aggregate CEO ownership, CEO duality, and CEO tenure. Specifically, we (1) use the continuous values of CEO ownership and CEO tenure variables, and the CEO duality indicator variable to construct a common factor through principle components analysis; (2) construct a new variable by summing the three indicator variables, *CEO_Own_Low*, *Chair_Non_Dual*, and *Tenure_Low*; and (3) create a new indicator variable equal to 1 if any of the three individual indicator variables is equal to 1. We rerun our tests using these alternative measures and find the results remain robust. Our results are also robust to using industry-adjusted CEO ownership and tenure.

Additional Tests

This section investigates four additional issues related to the impact of management forecast accuracy on CEO turnover: (1) whether the impact depends on the sign of the management forecast error, (2) whether the impact is conditional on the underlying uncertainty about future earnings, (3) whether the impact reflects managers' ability to manage earnings rather than their ability to forecast earnings, and (4) whether management forecast accuracy as a measure of CEO ability predicts future CEO turnover.

First, we investigate whether the impact of management forecast accuracy on CEO turnover depends on the sign of management forecast error. Although we include a broad range of controls for overall performance, it is conceivable that a negative management forecast error (actual earnings below a management forecast) proxies for poor performance. According to this logic, the magnitude of positive management forecast error should not be positively associated with the probability of CEO turnover. To test the validity of this argument, we modify Equations (2) and (3) to capture the signs of management forecast errors. In Equation (2), we add the interaction of $Abs(MFE) * MF$ with a dummy variable that is 1 if the management forecast error is negative (*MFE_Negative*) and add the interaction of $Abs(MFE) * MF$ with a dummy variable that is 1 if the management forecast error is positive (*MFE_Positive*). Panel A of Table 6 shows that the magnitudes of both positive and negative forecast errors are significantly related to CEO turnover. In addition, the difference between the coefficients on the positive and negative forecast errors is not significant. These results indicate that the positive association between the absolute management forecast error and the probability of CEO turnover continues to hold when actual earnings exceed the management forecast; i.e., when the forecast error is positive.

To further examine whether our results are driven by negative management forecast errors, in Panel B of Table 6 we present results from the estimation of a regression that augments Equation (3) by interacting the positive and negative management forecast errors with the good and poor performance dummy variables. This allows us to assess whether our results hold for both positive and negative forecast errors in each of good- and poor-performance groups. We find that the coefficients for the positive and negative forecast error interacted with the good performance dummy variable (*Dummy_Good*) are not significant, and not significantly different from each other. This suggests that when performance is good, management forecast error, either positive or negative, does not have a significant impact on CEO turnover. On the other hand, the coefficients for positive and negative forecast errors are significant when interacted with the poor performance dummy variable (*Dummy_Poor*), and the difference between the coefficients for the positive and negative forecast errors is not significant. These results suggest that when performance is poor, the magnitude of forecast error is significantly associated with CEO turnover, and that this applies to both positive and negative forecast error.

Our findings in Table 6 have implications for management's use of earnings forecasts to lower the market's expectation of earnings. Prior literature documents that CEOs have an

TABLE 6

Bivariate Probit Signed Management Forecast Errors Estimated Coefficients [z-statistics]

Panel A: Signed Management Forecast Errors

Management Forecast Issuance Model: Dependent Variable = 1 if the firm issues an earnings forecast for quarter $t-1$ (0 otherwise)		**Turnover Model: Dependent Variable = 1 if the CEO is replaced in quarter t (0 otherwise)**	
Prev_Forecast	0.855***	*Abs(MFE) * MF * MFE_Positive*	0.332*
	[73.34]		[1.48]
Abs_CXR	0.576***	*Abs(MFE) * MF * MFE_Negative*	0.621**
	[6.27]		[2.53]
LnMV	−0.035***	*MF*	0.02
	[2.72]		[0.34]
Num_Analysts	0.176***	*AFE*	−0.097***
	[6.68]		[4.56]
MB	0.008**	*ROE*	−0.211**
	[1.90]		[1.91]
Earn_Vol	−0.604***	*CAR*	−0.160***
	[2.68]		[4.56]
Ret_Vol	−0.025	*Earn_Vol*	0.266
	[0.17]		[1.12]
High_Tech	0.086***	*Ret_Vol*	0.619***
	[2.60]		[2.72]
REG	−0.397***	*LnSALES*	0.039***
	[5.28]		[3.40]
Tenure	−0.003**	*AGE*	0.033***
	[1.79]		[11.08]
Inst_Own	0.003***	*AGE65*	0.369***
	[6.96]		[6.96]
CEO_Own	−0.002	*Tenure*	−0.001
	[0.72]		[0.49]
Chair_Dual	−0.011	*Inst_Own*	−0.001**
	[0.46]		[1.96]
		CEO_Own	−0.022***
			[4.46]
		Chair_Dual	−0.053*
			[1.51]
n	30,481		30,481
Pseudo R^2	0.39		0.06
$Turnover_{it} = \beta_0 + \beta_1 Abs(MFE)_{it} * MF_{it} * MFE\_Positive + \beta_2 Abs(MFE)_{it} * MF_{it} * MFE\_Negative + \sum \rho_k Z_{kit} + \varepsilon_{it}$.			
Test: $\beta_2 - \beta_1 = 0$			0.289
			[1.03]

incentive to opportunistically issue overly pessimistic earnings forecasts to dampen investor expectations and to increase the probability of positive earnings surprises (Matsumoto 2002; Cotter et al. 2006). Our study suggests a potential cost to this strategy. If the firm issues a forecast to guide down the analyst forecast, then investors might suspect that the CEO is attempting to

(continued on next page)

TABLE 6 (continued)

Panel B: Signed Management Forecast Errors with Performance Dummy

Management Forecast Issuance Model: Dependent Variable = 1 if the firm issues an earnings forecast for quarter $t-1$ (0 otherwise)		**Turnover Model: Dependent Variable = 1 if the CEO is replaced in quarter t (0 otherwise)**	
Prev_Forecast	0.855***	*Abs(MFE) ∗ MF ∗ MFE_Positive ∗ Dummy_Good*	0.132
	[93.05]		[0.56]
Abs_CXR	0.577***	*Abs(MFE) ∗ MF ∗ MFE_Negative ∗ Dummy_Good*	0.803
	[6.43]		[1.04]
LnMV	−0.035***	*Abs(MFE) ∗ MF ∗ MFE_Positive ∗ Dummy_Poor*	1.064***
	[3.29]		[2.98]
Num_Analysts	0.176***	*Abs(MFE) ∗ MF ∗ MFE_Negative ∗ Dummy_Poor*	0.632***
	[7.74]		[2.51]
MB	0.008**	*MF*	0.023
	[2.01]		[0.36]
Earn_Vol	−0.605***	*Dummy_Poor*	0.007
	[3.74]		[0.19]
Ret_Vol	−0.026	*AFE*	−0.088***
	[0.17]		[3.88]
High_Tech	0.086***	*ROE*	−0.210**
	[3.06]		[1.93]
REG	−0.397***	*CAR*	−0.158***
	[7.71]		[4.41]
Tenure	−0.003**	*Earn_Vol*	0.274
	[2.18]		[1.19]
Inst_Own	0.003***	*Ret_Vol*	0.621***
	[8.11]		[2.79]
CEO_Own	−0.002	*LnSALES*	0.039***
	[0.81]		[3.27]
Chair_Dual	−0.011	*AGE*	0.033***
	[0.51]		[12.10]
		AGE65	0.370***
			[7.40]
		Tenure	−0.001
			[0.48]
		Inst_Own	−0.001**
			[1.91]
		CEO_Own	−0.022***
			[4.98]
		Chair_Dual	−0.053*
			[1.56]
n	30,481		30,481
Pseudo R^2	0.39		0.06

$$Turnover_{it} = \beta_0 + \beta_1 Abs(MFE) * MF * MFE\_Positive * Dummy\_Good + \beta_2 Abs(MFE) * MF * MFE\_Negative * Dummy\_Good + \beta_3 Abs(MFE) * MF * MFE\_Positive * Dummy\_Poor + \beta_4 Abs(MFE) * MF * MFE\_Negative * Dummy\_Poor + \sum \rho_k Z_{kit} + \varepsilon_{it}.$$

Good-performance group: Test: $\beta_2 - \beta_1 = 0$; results: $\beta_2 - \beta_1 = 0.671$ [0.86].

Poor-performance group: Test: $\beta_4 - \beta_3 = 0$; results: $\beta_4 - \beta_3 = -0.432$ [−1.11].

(continued on next page)

TABLE 6 (continued)

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*, **, *** $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively (based on one-tailed levels of significance).
This table presents the results of bivariate probit regressions estimated simultaneously. The dependent variable for the first-stage regression, *MF*, is included as an independent variable in the second-stage (CEO turnover) regression. For parsimony, the intercepts are not reported in the table. Definitions of other variables are in Table 2. We adjust Z-statistics and p-values for time-series correlation.

Variable Definitions:
MFE_Positive = 1 if the management forecast error is positive, and 0 otherwise;
MFE_Negative = 1 if the management forecast error is negative, and 0 otherwise;
Dummy_Good = 1 if *AFE* is not negative (i.e., the firm reports earnings above the analyst forecast), and 0 otherwise; and
Dummy_Poor = 1 if *AFE* is negative (i.e., the firm reports earnings below the analyst forecast), and 0 otherwise.

mislead the market or lacks the ability to make accurate earnings forecasts. This suspicion is likely reinforced if firms report overall poor performance, and, as a result, the board is more likely to replace the CEO.

We also investigate the impact of earnings uncertainty on the relation between management forecast accuracy and CEO turnover. Earnings uncertainty could affect the relation between management forecast accuracy and CEO turnover if the board adjusts management forecast accuracy for the underlying difficulty in accurate forecasting. To investigate this issue, we expand Equation (2) by adding an interaction between $Abs(MFE) * MF$ and a high earnings volatility dummy that is 1 if earnings volatility (*Earn_Vol*) is above the sample median. In untabulated results, the coefficient for $Abs(MFE) * MF$ is 0.421 and significantly positive ($p < 0.05$), but the coefficient for the interaction variable is 0.075 and insignificant. These results suggest that the relation between CEO turnover and management forecast accuracy does not depend on earnings volatility.

Management forecast error could reflect the ability to manage earnings rather than management's ability to form an accurate forecast. To explore this alternative explanation for our results, we follow Barton and Simko (2002) and assume that firms with relatively low net operating assets have the greatest ability to manage earnings. We find that $Abs(MFE) * MF$ is not significant for firms with low net operating assets (high earnings management potential), but remains significantly positive when net operating assets are high (earnings management is constrained). Thus, we conclude that the observed relation between management forecast accuracy and CEO turnover is more likely to reflect management's ability to forecast earnings than management's ability to manage earnings toward the forecast.

Our primary results are based on CEO turnover from 1996 to 2006. To investigate whether the results also apply to a more recent time period, we follow Beneish (1999) and use the parameter estimates from Equation (3) to predict turnover during a holdout period (2007–2010).[21] We then compare the results to a naive model and to the prediction model without the management forecast error variables.[22] Because a prediction model generates Type 1 (classifying an observation as no

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[21] We also estimate the model using an expanded sample period that covers 1996–2010. We find that the primary results reported in Table 4 continue to hold. However, we observe some changes for control variables in the turnover model. Specifically, *Earn_Vol* becomes significantly positive and *Ret_Vol* becomes weakly significant. In addition, *Inst_Own* and *Chair_Dual* become insignificant.

[22] A naive model classifies CEOs as turnover or as non-turnover depending on the relative costs of Type 1 and Type 2 errors. The best naïve strategy is to classify all CEOs as no turnover if the relative costs of Type 1 and Type 2 are less than 40:1. The naïve strategy switches to classify all CEOs as turnover at 40:1.

TABLE 7
Application of Turnover Prediction Model to Holdout Sample

Relative Costs of Type 1 and Type 2 Errors (1)	Cutoff Probability (2)	Expected Cost of Type 1 Error (3)	Expected Cost of Type 2 Error (4)	Cost of Model Errors Relative to Naive Model (5)	Cost of Model Errors Relative to Model without Management Forecast Error (6)
1:1	1.0000	1.0000	0.0000	1.0000	1.0000
10:1	0.9753	0.8482	0.0373	0.9974	1.0161
20:1	0.8347	0.5741	0.1502	0.9108	0.9653
30:1	0.7195	0.3796	0.2920	0.7645	0.9663
40:1	0.7276	0.3796	0.2920	0.7668	1.0189
60:1	0.8722	0.1969	0.5453	0.8756	0.9891
100:1	0.9720	0.0356	0.8654	0.9687	0.9882

We use the parameter estimates for the turnover prediction model in Table 4 based on the data for the period 1996–2006 to predict turnover during a holdout period 2007–2010. Column (1) presents the assumed relative cost of Type 1 (predicting no turnover when the CEO is replaced) and Type 2 errors (predicting turnover when the CEO is retained). Column (2) presents the cutoff probability of CEO turnover from the model that minimizes the cost of model errors. Columns (3) and (4) present the expected costs of Type 1 and Type 2 errors, respectively, for the model given the cut-off probabilities in Column (2) and the relative costs of errors in Column (1). Column (5) presents the cost of model errors relative to a naive model whereby the CEO is retained each quarter. Numbers less than 1 represent improvements over the naive model. Column (6) presents the cost of model errors relative to the model from Table 4 after removing the management forecast error terms. Numbers less than 1 indicate that including management forecast error improves the prediction model.

turnover when the CEO is dismissed) and Type 2 (classifying an observation as turnover when the CEO is retained) errors, it is necessary to specify the relative importance of each type of error in order to evaluate the overall performance of the prediction model. We follow Beneish (1999) and provide the results assuming different relative costs of Type 1 and Type 2 errors.[23]

We report the results in Table 7. Column (1) presents the relative costs of Type 1 and Type 2 errors used to evaluate the model. Column (2) provides the cut-off probability of turnover from the prediction model that minimizes the cost of the errors. Columns (3) and (4) provide the expected costs of Type 1 and Type 2 errors for each cut-off. The results in Column (5) show that the relative costs of errors using the prediction model relative to a naive model are consistently lower than 1. This provides evidence that the model, as a whole, reduces the cost of prediction error. The results in Column (6) show that the relative costs of errors are below 1 in four out of the six cases, where the cost of Type 1 error is greater than the cost of Type 2 error.[24] This is consistent with the prediction model with management forecast error tending to reduce the cost of prediction errors. It should be noted that the holdout sample period (2007–2010) corresponds to the economic downturn associated with the financial crisis, and the extent to which the crisis altered the underlying CEO replacement decision would add noise to the test.

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[23] Since we do not know the true economic costs associated with a Type 1 or Type 2 error, we follow Beneish (1999) and allow for a wide range of relative costs for Type 1 and Type 2 errors. We note that our objective is to identify the economic factors that lead to CEO turnover, as opposed to developing a model that could be used to predict CEO turnover. Because it is difficult to assess a prediction model outside of a specific decision-making context, our results for the model prediction error should be interpreted with caution.

[24] Given that CEO turnover is an infrequent event (occurs in approximately 3 percent of our sample quarters), for all practical purposes a prediction model has value only if the cost of a Type 1 error is relatively high. Otherwise, it would be difficult to beat a naive model that predicts no turnover, which would be correct 97 percent of the time.

V. SUMMARY AND CONCLUSIONS

This paper investigates the empirical relation between management forecast accuracy and CEO turnover. We contend that the accuracy of the forecast issued by the firm reflects the managerial ability of the CEO. Accordingly, we predict that boards of directors use management forecast accuracy to update their beliefs regarding CEOs' managerial ability and incorporate the forecast accuracy into the decision to retain or replace the CEO. Further, we predict that this effect is more pronounced when the firm's financial performance is poor and when the CEO is relatively less entrenched. Our results generally support our predictions. We find a significantly positive relation between the probability of CEO turnover and the absolute value of management forecast error and that the relation is stronger when overall firm performance is poor, i.e., the firm misses the analyst forecast. We also find that the positive relation between CEO turnover and the absolute management forecast error is stronger when the CEO is less entrenched. The positive relation between CEO turnover and the absolute management forecast error for poorly performing firms also appears to hold when earnings exceed the management forecast and when earnings are below the forecast. Overall, our results suggest that the boards use management forecast accuracy as an indicator of the CEOs' managerial ability and incorporate the forecast accuracy in the decision to retain or replace the CEO when firm performance is poor.

Our study adds new insight into management's incentives to issue accurate forecasts by documenting a labor market consequence of inaccurate management forecasts. Our results suggest that the labor market penalizes managers for inaccurate forecasts and that managers with low ability bear a cost from issuing inaccurate forecasts. In addition, our study suggests that the board holds the CEO responsible for the accuracy of the firm's earnings forecasts.

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The Effect of Financial Analysts' Strategic Behavior on Analysts' Forecast Dispersion

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ABSTRACT: Financial analysts' forecast dispersion has been used in a variety of contexts in accounting and finance studies. In this study, we provide large sample evidence on the cross-sectional determinants of forecast dispersion and examine to what extent analysts' strategic behavior biases the observed dispersion from the dispersion of unmanaged forecasts. We propose a method to estimate the dispersion bias for each sample observation. We find that observed dispersion, on average, understates dispersion of unmanaged forecasts by 53.4 percent and this downward bias varies considerably across firms. We further discuss the implications of the significant cross-sectional variation in the bias in observed dispersion for studies that rely on dispersion to estimate constructs such as consensus and information quality as well as those that use dispersion directly in research design.

Keywords: *financial analysts; earnings forecasts; dispersion.*

Data Availability: *The data are publicly available for the sources indicated.*

I. INTRODUCTION

A variety of empirical applications in accounting and finance have used measures of the dispersion of financial analysts' earnings forecasts. Despite the widespread use of forecast dispersion in accounting and finance research, very little evidence exists on the cross-sectional determinants of this important empirical measure (an exception is Diether et al. [2002]). Observed forecasts data are not only influenced by characteristics such as public and private information precision, but also by analysts' strategic behavior. Prior research has mainly examined two different types of analysts' strategic actions, namely, herding and self-selection. Herding (Trueman 1994; Hong et al. 2000; Clement and Tse 2005) occurs when analysts

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strategically process proprietary and public information to generate forecasts that are closer to recent forecasts of other analysts. Self-selection, on the other hand, arises when a subset of analysts whose information processing results in low forecasted earnings choose not to release their forecasts (McNichols and O'Brien 1997; Hayes 1998; Hayes and Levine 2000). In contrast to prior research that has investigated the effect of the above types of analysts' strategic behavior on forecast accuracy, we examine the impact of analysts' strategic behavior on forecast dispersion in the cross-section.

We first discuss the implications of different types of analysts' strategic actions for their effect on dispersion and show that the bias induced in observed dispersion may be positive or negative depending on the context. For example, if analysts strategically herd toward a consensus of unmanaged forecasts, then it may lead to an estimate of dispersion derived from observed forecasts data that is lower than the dispersion based on unmanaged forecasts.[1] On the other hand, if some of the analysts strategically follow a "leader" analyst who is making a bold forecast away from the consensus, then observed dispersion will be higher than the dispersion of unmanaged forecasts. Finally, if self-selection by analysts is the strategic behavior of choice, then it may lead to observed dispersion being lower than the dispersion of unmanaged forecasts.

We then estimate forecast dispersion for 89,473 observations (treating dispersion for the pre- and post-interim earnings announcement periods as two separate sample observations) by using a primary sample of analysts' annual earnings forecasts over the period 1985–2004. Our pooled cross-sectional analysis reveals that forecast dispersion is positively associated with book-to-market ratio, CAPM beta, earnings variation, reporting a loss, research and development expense, the standard deviation of residuals from the market model, number of analysts, the exchange-adjusted turnover in the firm's shares, and debt-to-equity ratio. We also find that forecast dispersion is negatively associated with market value of the firm, momentum, current period earnings change, and sales turnover ratio. These results are consistent with prior literature. We check for the existence of the bias in observed dispersion measures by modeling the unexplained portion of forecast dispersion as arising due to both a two-sided random noise component and a one-sided random component and find support for the argument that observed forecast dispersion suffers, on average, from a *downward* bias.

We also calculate this downward bias for each sample observation using the conditional mean estimator of the one-sided component of the error. By adjusting the observed dispersion for this downward bias, we derive a modified dispersion measure. On average, the bias understates the dispersion of unmanaged forecasts by 53.4 percent, indicating that the adjustment we propose is both economically and statistically significant. In addition, we also show that a set of firm and analyst characteristics that have been identified in prior research as being associated with analysts' herding and self-selection behavior can explain cross-sectional variation in this dispersion bias.

More importantly, we demonstrate that using our modified dispersion measure instead of the observed dispersion measure leads to significantly different inferences in a number of research settings. Specifically, we show that inferences about the relative magnitude of the change in public and private information quality around quarterly earnings announcements reverse by using our modified dispersion measure. We also document that the consensus or commonality measure derived from observed dispersion is significantly higher than the corresponding measure derived from our dispersion measure. We further predict and find support for the hypothesis that this upward bias in consensus is significantly larger for those firms that have higher herding tendency, higher variation in analyst capabilities, higher levels of financial distress, and higher levels of forecast optimism. We also show that use of our measure leads to significantly different conclusions about the association between cost of equity capital and public and private information quality compared to results based on observed dispersion. Finally, we also discuss why it is critical for studies that develop and test competing hypotheses about the predicted association between a specific variable of interest and dispersion to perform a robustness check of their results with our modified dispersion measure.

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[1] For the rest of the paper, the term "unmanaged forecast" refers to a forecast made by a rational analyst who is an efficient information processor and does not indulge in strategic behavior.

We believe that our study makes the following contributions to the literature on analysts' forecast dispersion. First, it provides large-sample evidence on the determinants of forecast dispersion and confirms that analysts' strategic behavior induces, on average, a downward bias in the dispersion measure. Second, it develops an adjustment procedure that can help correct the bias in observed dispersion. Third, it illustrates the importance of our modified dispersion measure for a variety of studies that employ proxies of analysts' information environment derived from dispersion, as well as those that use dispersion directly in their research design.

Section II provides a review of the relevant literature on forecast dispersion and analysts' strategic behavior, and discusses the research questions. Section III discusses the research methodology used in the paper. We discuss sample selection and the empirical results in section IV. Section V provides empirical demonstrations of settings where our modified measure of dispersion yields different inferences and discusses the implications of these empirical findings for accounting and finance research. We provide a summary and conclude in section VI.

II. LITERATURE REVIEW AND RESEARCH QUESTION

This section first reviews and summarizes the relevant literature in accounting and finance that has used analysts' forecast dispersion as part of the research design. We then provide background information on analysts' strategic behavior and discuss its effect on dispersion in the cross-section.

Use of Dispersion in Accounting and Finance Research

The widespread use of analysts' forecast dispersion in accounting and finance empirical research is primarily due to the fact that dispersion, as a theoretical construct, can be expressed as a product of the overall level of uncertainty and divergence in analysts' beliefs or lack of consensus (Barry and Jennings 1992; Abarbanell et al. 1995; Barron et al. 1998). Therefore, various empirical studies have used observed forecast dispersion as a proxy for uncertainty as well as a proxy for the divergence in analysts' beliefs.

Earlier empirical studies that have used forecast dispersion include Daley et al. (1988), who examine the association between forecast dispersion and *ex post* magnitude of unexpected earnings, *ex post* returns variance around earnings announcement dates, and the average variance of return to maturity of options. Likewise, Stickel (1989) analyzes the association between pre-earnings announcement forecast dispersion and post-announcement analysts' revision activity, and Ajinkya et al. (1991) use forecast dispersion as a proxy for divergence in beliefs and conclude that a significant positive association exists between the dispersion of analysts' forecasts of annual EPS and trading volume.[2]

An influential study by Barron et al. (1998; hereafter, BKLS) has led to an increased research focus on the roles of dispersion and the squared error in mean forecast. Assuming that all analysts following a particular firm are endowed with private information of the same quality, the BKLS approach can be used to develop proxies for private and public information quality at various points in time. Recent studies that have used the BKLS approach include Barron et al. (2002), who find that the precision of public information contained in analysts' forecasts increases more than the precision of private information around earnings announcements, Bowen et al. (2002), who document that conference calls can improve analysts' private information precision, Liang (2003) who concludes that post-earnings announcement drift is mainly driven by investors' irrationally assigning too much weight to their private information, and Botosan et al. (2004), who investigate the association between implied cost of capital and the quality of public and private information.

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[2] Other accounting studies such as Swaminathan (1991), Imhoff and Lobo (1992), Lustgarten and Mande (1998) and Barron and Stuerke (1998) also use dispersion prominently in their research design.

Studies in finance that rely on the dispersion measure include Diether et al. (2002), who find that stocks with higher dispersion in analysts' earnings forecasts earn lower future returns and interpret their evidence as suggesting that a high level of disagreement can result in overvaluation. However, Johnson (2004) argues that Diether et al.'s (2002) evidence may be due to dispersion proxying for idiosyncratic rather than systematic risk. Thomas (2002) investigates the relation between an asset-based Herfindahl Index and forecast dispersion and concludes that greater business diversification is not associated with increased asymmetric information. Zhang (2006) includes forecast dispersion in his research design to quantify information uncertainty while examining the role of information uncertainty in price continuation anomalies and cross-sectional variations in stock returns.

Despite the popularity and widespread use of analysts' forecast dispersion in accounting and finance research, few studies, except for Diether et al. (2002), have examined the cross-sectional determinants of dispersion. Diether et al. (2002) attempt to disentangle the effect of dispersion in analysts' forecasts from stock fundamentals and additional measures of risk by performing a series of cross-sectional regressions of dispersion on various lagged firm characteristics. They conduct their analysis at the annual level and use only a small fraction of all available analyst forecasts for a particular firm. Their study also does not consider the effect of factors other than overall level of uncertainty and divergence of opinion on dispersion.

Impact of Analysts' Strategic Behavior on Observed Dispersion

An important factor that may affect the use of dispersion based on observed analysts' forecasts is possible strategic behavior by some of the analysts issuing the earnings forecasts. Strategic behavior from individual analysts may manifest itself in a number of ways, with different implications for the dispersion among observed forecasts. For example, consider a single analyst who completely ignores her private information when issuing a revised forecast and simply issues a revised forecast equal to the consensus based on unmanaged forecasts of other analysts. It is intuitive that this type of strategic behavior, i.e., reporting the unbiased consensus forecast, will always lead to observed dispersion being strictly lower than the dispersion of unmanaged forecasts.

On the other hand, if the marginal analyst decides to follow a leader by copying the leader's forecast, then the observed dispersion may be lower or higher than the dispersion based on unmanaged forecasts. Given that very little guidance exists in prior literature on how strategic behavior of "follower" analysts affects observed dispersion, we examine stylized situations characterized by the presence of two or three analysts to provide further insight into this issue. The intuitive case of two analysts, where one analyst simply adjusts her forecast to be closer to the leader's forecast, leads to a strict reduction in dispersion. However, with three analysts, the situation becomes more complex; Appendix A presents nine different three-analyst scenarios in which one or more analysts follow a leader analyst. In seven out of these nine scenarios, observed dispersion is smaller than the dispersion of unmanaged forecasts. These scenarios include situations where the follower analysts have initial extreme forecasts as well as when two of three analysts follow the third who is the leader. The only situations where observed dispersion exceeds dispersion of unmanaged forecasts occur when a strategic analyst in the middle of the pack follows one of the extreme analysts.

It is also possible that a downward bias is induced in observed dispersion if a subset of analysts choose not to revise their forecasts when endowed with bad news. This self-selection behavior (McNichols and O'Brien 1997; Hayes 1998; Hayes and Levine 2000) will not only produce a mean forecast that is higher than the mean of the unmanaged forecasts, but also result in a lower level of observed dispersion relative to the dispersion of unmanaged forecasts.

The discussion in this section suggests that analysts' strategic behavior generally leads to convergence of their forecasts and, on average, *downward* bias in the dispersion of analysts' forecasts. The next section proposes a method for evaluating whether observed dispersion exhibits a downward bias and develops a modified measure of forecast dispersion that captures only the overall level of uncertainty and divergence of opinion.

III. METHODOLOGY

Consider at time t, observations on $i = 1,\ldots,I$ firms, each observation comprising a vector of observed individual analysts' forecasts $Y_{it} \equiv (y_{1it},\ldots,y_{Nit})$ and a vector of firm-characteristics $X_{it} \equiv (x_{1it},\ldots,x_{Kit})$. We denote the dispersion in observed forecasts as $\tilde{D}_{it}$, and the dispersion of the unobserved analysts' forecasts in the absence of any strategic action as D_{it}, which is made up of a random zero mean noise term v_{it} and a systematic component that can be explained by the firm characteristics. Specifically:

$$D_{it} = \beta_0 + \sum_{k=1}^{K} \beta_k x_{kit} + v_{it}. \tag{1}$$

Assuming that the observed dispersion $\tilde{D}_{it}$will be less than D_{it}, the dispersion based on unmanaged forecasts, we represent the positive difference between the dispersion of unmanaged forecasts and the observed dispersion as the one-sided random error term $u_{it} \geq 0$, i.e.:

$$\tilde{D}_{it} = D_{it} - u_{it} = \beta_0 + \sum_{k=1}^{K} \beta_k x_{kit} + v_{it} - u_{it}. \tag{2}$$

We assume that the error terms v_{it} and u_{it} are independent and identically distributed (i.i.d.) random variables across t and i, independent of each other and the various firm characteristics. For a major part of the rest of the discussion, we suppress the firm and time subscripts on the random error terms v_{it} and u_{it} and denote the total error $v - u$ as ε.[3]

It is straightforward to verify that the probability density function (pdf) of the total error, ε, is given by:

$$f_\varepsilon(\varepsilon) = \int_{\varepsilon}^{\infty} f_v(v) f_u(v - \varepsilon)\, dv, \tag{3}$$

where $f_v(v)$ and $f_u(u)$ are the pdf's of v and u, respectively. The unknown parameters in Equation (2) can be estimated through Maximum Likelihood (ML) estimation if a specific parametric form is assumed for the pdf of ε.[4] Assume that v is distributed as $N(0, \sigma_v^2)$and u as half-Normal i.e., as

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[3] If prior beliefs dictate that observed dispersion is greater than the dispersion of unmanaged forecasts, i.e., analysts' strategic behavior creates additional divergence, then the specification in Equation (2) can be modified by adding rather than subtracting the one-sided error term u_{it}. The total error ε, in this case, will be $v + u$.

[4] Ordinary Least Square (OLS) method can be used to estimate β_k ($k = 1$ to K) since ε has a finite variance, although it may not have a zero mean (Schmidt 1976). The OLS estimators of β_k ($k = 1$ to K) are consistent. However, the OLS intercept is an inconsistent estimator of β_0. The OLS intercept has a negative bias because it forces the error term to have a mean of zero despite the fact that the error term has a non-zero negative mean. If we denote the OLS intercept as δ_0, then plim $\hat{\delta}_0 = \beta_0 - E(u)$ where $E(u)$ is the expected value of u.

$N^{+}(0, \sigma_u^2)$ as in Aigner et al. (1977) or Meeusen and van den Broeck (1977).[5] Then the domain of the total error ε is (–∞, ∞), and the pdf of ε is:

$$f(\varepsilon) = \frac{2}{\sigma}\phi\left(\frac{\varepsilon}{\sigma}\right)\Phi\left(-\frac{\varepsilon\lambda}{\sigma}\right), \tag{4}$$

where $\sigma = (\sigma_u^2 + \sigma_v^2)^{1/2}$, $\lambda = \frac{\sigma_u}{\sigma_v}$, and $\Phi(.)$ and $\varphi(.)$ are the standard normal cumulative distribution and density functions.[6]

Maximum Likelihood (ML) estimation of Equation (2) using the pdf of ε in Equation (4) provides estimators of β_0, β_k ($k = 1$ to K), σ_u and σ_v that are consistent, asymptotically normally distributed and efficient.[7] The maintained assumption of reduction in dispersion due to analysts' strategic behavior can be evaluated by (a) examining the t-statistic associated with σ_u, and (b) comparing whether ML estimation of Equation (2) generates a significantly higher likelihood than an Ordinary Least Squares (OLS) estimation of Equation (2).

The estimated value of the observation-specific total error term ε_{it} and the estimated values of the parameters σ_u and σ_v can be combined to generate a point estimator of u_{it}. Specifically, the conditional density of u given ε i.e.: $f(u|\varepsilon)$ can be derived as:

$$f(u|\varepsilon) = \exp\left(-\frac{(u-\tilde{\mu})^2}{2s^2}\right) \Big/ \sqrt{2\pi}s\left[1 - \Phi\left(-\frac{\tilde{\mu}}{s}\right)\right], \tag{5}$$

where $\tilde{\mu} = -\frac{\varepsilon\sigma_u^2}{\sigma^2}$, $s^2 = \frac{\sigma_u^2\sigma_v^2}{\sigma^2}$, and $\sigma^2 = (\sigma_u^2 + \sigma_v^2)$. Since $f(u|\varepsilon)$ is distributed as $N^{+}(\tilde{\mu}, s^2)$, one can use the mean of the distribution in Equation (5) as a point estimator for u (Jondrow et al. 1982). Specifically, the conditional mean can be evaluated as:

$$E(u_{it}|\varepsilon_{it}) = s\left[\frac{\phi\left(\frac{\varepsilon_{it}\lambda}{\sigma}\right)}{1 - \Phi\left(\frac{\varepsilon_{it}\lambda}{\sigma}\right)} - \frac{\varepsilon_{it}\lambda}{\sigma}\right]. \tag{6}$$

We add $E(u_{it}|\varepsilon_{it})$, the conditional mean of u (which is strictly positive), to the observed dispersion to generate the observation-specific "corrected" dispersion measure.

We next discuss the sample selection, ML estimation of the cross-sectional determinants of observed dispersion, the empirical distribution of the dispersion bias, and an exploratory analysis of the cross-sectional determinants of the dispersion bias.

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[5] The original Aigner et al. (1977) study suggests normal distribution for v and either the half-normal distribution or the exponential distribution for u. Besides yielding simple closed-form pdf for the total error ε (see Equation (4)) these basic pdf's also provide closed-form pdf for the conditional density of u given ε. The advantage of using half-normal or exponential pdf's for u is that they are both one-parameter distributions. As a result, the pdf for the total error ε (which is also commonly referred to as composed error in the empirical production economics literature) is characterized by just two parameters i.e., σ_u and σ_v. Interestingly, the Aigner et al. (1978) study, while more than 30 years old, continues to provide a robust platform and influence empirical production economics research. A recent Google Scholar search for this study revealed that it has been cited more than 3,200 times.

[6] For the case of observed dispersion being greater than the dispersion of unmanaged forecasts, $\varepsilon = v + u$ and the pdf of the composed error term is $f(\varepsilon) = \frac{2}{\sigma}\phi(\frac{\varepsilon}{\sigma})\Phi(\frac{\varepsilon\lambda}{\sigma})$.

[7] The error, ε, can be expressed as $\tilde{D}_{it} - \beta_0 - \sum_{k=1}^{K}\beta_k x_{kit}$ from Equation (2) and substituted in the pdf in Equation (4) to evaluate the likelihood for every observation. ML estimation involves maximizing the joint likelihood, which is the product of the individual likelihood values by choosing appropriate values for β_0, β_k ($k=1$ to K), σ_u and σ_v. ML estimation routines are available in statistical packages such as STATA, Gauss, R, and SAS. We used Proc NLP in SAS to carry out our maximum likelihood estimation.

IV. EMPIRICAL ANALYSIS

We first describe the criteria we used to select our sample. We start with the full Institutional Broker Estimate System (I/B/E/S) database and use the I/B/E/S Unadjusted Detail Earnings Estimate History File to obtain analysts' annual earnings forecasts, actual earnings, and actual stock price before any stock-split for the period from 1985 to 2004.[8] We focus on only those annual earnings forecasts issued within a 45-day window prior to quarterly earnings announcement or within a 30-day post-announcement window in the analysis. We therefore retain only those forecasts for which the interim quarterly earnings announcement date information is available in quarterly Compustat primary, supplementary, or tertiary files. If a particular analyst made multiple forecasts within a window, then we retain only the one closest to the announcement. If an analyst does not revise her forecast after the quarterly earnings announcement, then we assume she does not change her forecast. We also keep only those windows that had at least two analysts' forecasts for the firm. Using the forecasts within a particular pre- or post-announcement window, we calculate the mean forecast (*MEAN*) and the sample standard deviation (*STD*) of the forecasts.

The above procedure leads to a maximum of six observations (pre or post windows corresponding to each of the three quarterly announcement dates) per firm per year and 116,354 observations in the whole sample. Of these, we lose 26,097 observations due to non-availability of data for one or more of the various explanatory or control variables, leaving 90,257 observations with complete data. We further trim the data set by deleting observations that had extreme analysts' forecasts, which we define as those with forecast error scaled by price in the top or bottom half percentile in each fiscal year, leaving a final sample of 89,473 observations, ranging from 2,167 observations in 1985 to 7,375 observations in 2004.

Table 1 provides the descriptive statistics for some of the primary variables used in subsequent analysis. The average mean forecast is \$1.21 per share and the standard deviation is \$0.08 per share. The empirical distribution of standard deviation is left-skewed and 3,390 of the 89,473 observations (4 percent) have a sample standard deviation of zero. The median market value of equity is approximately \$935 million and median book-to-market ratio is 0.47. The median firm had a following of four analysts and 27 percent of the firms had non-zero R&D expenditure. Overall, the sample firms appear similar to those in prior large-sample studies based on I/B/E/S analysts forecast data.

Cross-Sectional Determinants of Dispersion

To examine whether observed dispersion is downward biased, we employ the following specification:[9]

$$\begin{aligned} Log(STD_{it}) = {} & \beta_0 + \beta_1 SIZE_{it} + \beta_2 BM_{it} + \beta_3 BETA_{it} + \beta_4 MOMENT_{it} + \beta_5 VAREPS_{it} \\ & + \beta_6 \Delta EPS_{it} + \beta_7 LOSS_{it} + \beta_8 RD_{it} + \beta_9 SDRES_{it} + \beta_{10} LOGNANALYST_{it} \\ & + \beta_{11} TURN_{it} + \beta_{12} DE_{it} + \beta_{13} SA_{it} + \beta_{14} LOG(ABSM)_{it} + \beta_{15} LOGP_{it} + v_{it} \\ & - u_{it}. \end{aligned} \tag{7}$$

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[8] Using I/B/E/S detail file with already split-adjusted data does not change our results.

[9] The idea behind the log transformation is to make the data approximately normal. Since some of our observations have a standard deviation equal to zero, we add a small constant to all the standard deviation values before taking the logarithm. Berry (1987) suggests the choice of a constant that minimizes the sum of the absolute values of skewness and excess kurtosis. We fitted a 3-parameter log-normal distribution to the full dispersion data (including the zero dispersion observations) and chose the negative of the threshold value as the constant such that the resulting distribution of *log*(*STD* + *constant*) is approximately normal. The sum of the absolute values of skewness and excess kurtosis is very small at 0.09.

TABLE 1

Descriptive Statistics

	Mean	Standard Deviation	Lower Quartile	Median	Upper Quartile
MEAN	1.207	1.458	0.468	0.988	1.700
STD	0.076	0.147	0.014	0.035	0.082
SIZE	6.945	1.628	5.768	6.841	8.006
BM	0.523	0.316	0.299	0.469	0.681
BETA	1.103	1.102	0.572	1.015	1.481
MOMENT	0.196	0.550	−0.082	0.118	0.348
VAREPS	0.345	0.496	0.096	0.189	0.384
ΔEPS	−0.002	0.058	−0.017	−0.001	0.014
LOSS	0.129	0.336	0.000	0.000	0.000
RD	0.270	0.444	0.000	0.000	1.000
SDRES	0.024	0.012	0.015	0.021	0.029
LOGNANALYST	1.605	0.715	1.099	1.609	2.079
TURN	1.195	1.079	0.550	0.872	1.445
DE	3.046	4.582	0.651	1.321	2.694
SA	0.241	0.190	0.098	0.216	0.328

The sample consists of 89,473 observations for the period 1985–2004. For each firm-quarter, there has to be at least two analysts' forecasts of annual earnings within 45 days before the quarterly earnings announcement, and within 30 days after the announcement. Pre- and post-announcement are considered two different observations. If an analyst does not revise her forecast after the quarterly earnings announcement, then we assume she does not change her forecast. If an analyst makes multiple forecasts within a measurement window, then the one closest to the announcement date is used.

Variable Definitions:
MEAN = mean of individual analysts' earnings forecasts;
STD = standard deviation of individual analysts' earnings forecasts;
SIZE = natural log of market value;
BM = book-to-market ratio;
BETA = beta calculated using a market model, based on monthly returns over the past 36 to 60 months before the quarterly earnings announcement;
MOMENT = cumulative monthly return calculated over the (−12,−2) window before the quarterly earnings announcement;
VAREPS = variance of the past eight quarters' earnings per share calculated in a rolling window;
ΔEPS = change in annual earnings per share, scaled by last fiscal year's closing price;
LOSS = 0 if quarterly earnings per share are positive or 0, 1 otherwise;
RD = 1 if the company reports research and development expense, 0 otherwise;
SDRES = standard deviation of the residual from the market model that estimated *BETA*;
LOGNANALYST = natural log of number of analysts who release forecasts in the measurement window;
TURN = mean daily turnover measured over the past 250 days, lagged one month, before the quarterly earnings announcement, adjusted for the average exchange daily turnover. Turnover is calculated as volume divided by shares outstanding;
DE = debt to equity; and
SA = sales to total assets.

We use explanatory variables from prior studies to capture cross-sectional variation in dispersion induced by firm-level variables proxying for characteristics such as fundamental risk, uncertainty, and difference in opinion. The fundamental risk variables we include are *SIZE* (measured as the natural log of market value of equity), CAPM *BETA*, book-to-market ratio (*BM*), and the cumulative return over an 11-month period ending two months before the earnings announcement (*MOMENT*). Based on the empirical evidence in Diether et al. (2002), we expect dispersion to be positively associated with *BETA* and *BM* but negatively associated with *SIZE* and *MOMENT*. Similar to Diether et al. (2002), we use turnover in the firm's shares (*TURN*) and the natural logarithm of the number of analysts (*LOGNANALYSTS*) as proxies for differences in opinion and expect them to be positively associated with dispersion.

We include past earnings variability (*VAREPS*), change in annual EPS from prior year (ΔEPS), an indicator variable for firms reporting a loss (*LOSS*), an indicator variable for firms that invest in R&D (*RD*), and standard deviation of market model residuals (*SDRES*) to capture the uncertainty in the analysts' information environment. We also include debt-to-equity ratio (*DE*) and sales turnover ratio (*SA*) as additional control variables. To control for the fact that firms with a high level of earnings may mechanically have a high level of dispersion, we also include the log of the absolute mean forecast, *LOG*(*ABSM*). In addition, we include the log of stock price (*LOGP*) as an additional control variable to account for any other correlated, omitted determinants of dispersion.[10] Brown and Han (1992) show that forecast dispersion decreases after earnings announcements and Barron et al. (2002) document that forecast dispersion decreases over time when one goes from the first quarter to the third quarter as analysts obtain more information. We include indicator variables that specify whether the observation belongs to Q1pre-announcement, Q1post-announcement, Q2pre-announcement, Q2post-announcement, or Q3pre-announcement windows to capture these effects. Finally, we also incorporate industry and year fixed-effects in the estimation.

The results in the first two columns of Table 2 of estimating Equation (7) using OLS are consistent with the evidence shown in prior studies on the determinants of forecast dispersion. All the explanatory variables have the predicted signs with the absolute values of many of the associated t-statistics being greater than 20. The model has good explanatory power with an adjusted R^2 of 0.392.

Next, using ML estimation based on the error term in Equation (5) yields estimated coefficients on the explanatory variables in the last two columns of Table 2 that are similar to those under OLS. The intercept term, however, shows a significant increase, consistent with the idea that the one-sided error term makes a significant contribution to the composed error structure. More specifically, the estimated value of σ_u, the parameter associated with the one-sided error term, is 0.962 with an associated t-statistic of 81.69. This provides support for the argument that the observed dispersion of analysts' forecasts is, on average, *less* than the dispersion based on the unmanaged forecasts. Additional evidence on the superiority of the composed error specification is provided by the likelihood ratio test. The Chi-square statistic with one degree of freedom for the likelihood ratio test has a value of 947, which is well above the 99 percent critical value of 6.63. Finally, the Schwartz index associated with the comparison of the ML specification with that of a benchmark OLS specification has a value of 468, which is significantly greater than the critical value of 10, once again providing evidence on the superiority of the composed error-model.[11]

Dispersion Bias Factor (DBF) and its Empirical Characteristics

Using the ML parameter estimates of the coefficients on the explanatory variables, we next calculate the residual error for every observation in the sample. We then use the residual error with the estimates of σ_u and σ_v to generate observation-specific, strictly positive, correction using the

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[10] Prior studies have typically used a scaled measure of dispersion with absolute mean forecast or price as the deflator. Our primary interest in this paper is to develop a method to provide a correction to the observed dispersion measure. Therefore, instead of scaling dispersion by absolute mean forecast or price, we include both as control variables.

[11] The Schwartz index is calculated as $\{ML\ loglikelihood - OLS\ loglikelihood - 0.5log\ (n)\}$ where n is the total number of observations. For large samples, values of this index exceeding 10 indicate that the composed error model is favored over the OLS model.

TABLE 2

Cross-Sectional Determinants of Analyst Forecast Dispersion
(n = 89,473)

		OLS		ML	
	Predicted Sign	Estimated Coefficient	p-value	Estimated Coefficient	p-value
Intercept		−5.362	< 0.001	−4.582	< 0.001
Fundamental Risk					
SIZE	−	−0.102	< 0.001	−0.101	< 0.001
BM	+	0.803	< 0.001	0.811	< 0.001
BETA	+	0.006	0.056	0.663	0.027
MOMENT	−	−0.079	< 0.001	−0.083	< 0.001
Uncertainty					
VAREPS	+	0.325	< 0.001	0.348	< 0.001
ΔEPS	−	−0.720	< 0.001	−0.742	< 0.001
LOSS	+	0.700	< 0.001	0.722	< 0.001
RD	+	0.035	0.001	0.029	0.003
SDRES	+	19.123	< 0.001	19.162	< 0.001
Difference in Opinion					
LOGNANALYST	+	0.436	< 0.001	0.378	< 0.001
TURN	+	0.023	< 0.001	0.028	< 0.001
Control Variables					
DE	+	0.012	< 0.001	0.012	< 0.001
SA	−	−0.372	< 0.001	−0.380	< 0.001
LOG(ABSM)	+	0.242	< 0.001	0.237	< 0.001
LOGP	?	0.440	< 0.001	0.453	< 0.001
Q1-PRE	+	0.596	< 0.001	0.585	< 0.001
Q1-POST	+	0.460	< 0.001	0.455	< 0.001
Q2-PRE	+	0.472	< 0.001	0.454	< 0.001
Q2-POST	+	0.272	< 0.001	0.266	< 0.001
Q3-PRE	+	0.275	< 0.001	0.256	< 0.001
		Adjusted $R^2 = 0.392$		$\sigma_u = 0.962$ (t-statistic = 81.69) $\sigma_v = 0.800$ (t-statistics = 62.33)	

Tests Comparing ML estimators with OLS estimators:

Likelihood Ratio Test: Chi-square = 947 (p-value < 0.01).

Schwartz index: 468 (Greater than critical value 10).

This table presents the pooled-regression results for OLS and ML estimation procedures respectively for the specification including year and industry fixed effects:

$$\begin{aligned} Log(STD_{it}) = {} & \beta_0 + \beta_1 SIZE_{it} + \beta_2 BM_{it} + \beta_3 BETA_{it} + \beta_4 MOMENT_{it} + \beta_5 VAREPS_{it} + \beta_6 \Delta EPS_{it} + \beta_7 LOSS_{it} \\ & + \beta_8 RD_{it} + \beta_9 SDRES_{it} + \beta_{10} LOGNANALYST_{it} + \beta_{11} TURN_{it} + \beta_{12} DE_{it} + \beta_{13} SA_{it} \\ & + \beta_{14} LOG(ABSM)_{it} + \beta_{15} LOGP_{it} + v_{it} - u_{it}. \end{aligned}$$

The sample consists of 89,473 observations for the period 1985–2004. For each firm-quarter, there has to be at least two analysts' forecasts of annual earnings within 45 days before the quarterly earnings announcement, and within 30 days after the announcement. Pre- and post-announcement are considered two different observations. If an analyst does not revise her forecast after the quarterly earnings announcement, then we assume she does not change her forecast. If an analyst makes multiple forecasts within a measurement window, then the one closest to the announcement date is used.

Variable Definitions:

STD = standard deviation of individual analysts' earnings forecasts;

(continued on next page)

TABLE 2 (continued)

SIZE = natural log of market value;
BM = book-to-market ratio;
BETA = beta calculated using a market model, based on monthly returns over the past 36 to 60 months before the quarterly earnings announcement;
MOMENT = cumulative monthly return calculated over the (−12,−2) window before the quarterly earnings announcement;
VAREPS = variance of the past eight quarters' earnings per share calculated in a rolling window;
ΔEPS = change in annual earnings per share, scaled by last fiscal year's closing price;
LOSS = 0 if quarterly earnings per share are positive or 0, 1 otherwise;
RD = 1 if the company reports research and development expense, 0 otherwise;
SDRES = standard deviation of the residual from the market model that estimated *BETA*;
LOGNANALYST = natural log of number of analysts who release forecasts in the measurement window;
TURN = mean daily turnover measured over the past 250 days, lagged one month, before the quarterly earnings announcement, adjusted for the average exchange daily turnover. Turnover is calculated as volume divided by shares outstanding;
DE = debt to equity;
SA = sales to total assets;
LOG(*ABSM*) = natural log of the absolute value of the mean forecasts;
LOGP = natural log of stock price;
Q1-PRE = 1 if the observation belongs to the (−45,−1) day window around earnings announcement of quarter 1, 0 otherwise;
Q1-POST = 1 if the observation belongs to the (0,29) day window around earnings announcement of quarter 1, 0 otherwise;
Q2-PRE = 1 if the observation belongs to the (−45,−1) day window around earnings announcement of quarter 2, 0 otherwise;
Q2-POST = 1 if the observation belongs to the (0,29) day window around earnings announcement of quarter 2, 0 otherwise; and
Q3-PRE = 1 if the observation belongs to the (−45,−1) day window around earnings announcement of quarter 3, 0 otherwise.

expression for the conditional mean in Equation (6).[12] Combining this correction with our dependent variable yields the corrected standard deviation (*STDMOD*).[13] For the rest of the paper, we define the fractional reduction in dispersion induced by analysts' strategic behavior as the dispersion bias factor, i.e., $DBF \equiv (STDMOD - STD)/STDMOD$[14] By construction, this factor takes values from 0 to 1 with large values denoting a greater level of downward bias in observed dispersion.

Panel A of Table 3 provides details on the empirical distribution of the observed dispersion (*STD*), the modified dispersion (*STDMOD*), and the dispersion bias factor (*DBF*). While the average observed dispersion is 0.076, the average modified dispersion is 0.133. Likewise, observed dispersion has a smaller standard deviation of 0.147 versus 0.220 for modified dispersion. On

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[12] We examined the pair-wise correlations between the estimated conditional mean of u and each of the 15 independent variables. The average of these 15 correlations is −0.004, with ten of them being less than 0.01. This provides some support for the assumption that the one-sided error term u is independent of the firm characteristics.

[13] Given that the dependent variable is expressed in logarithmic form with a constant added to it (see footnote 9), we add the correction to the dependent variable, exponentiate it, and then subtract the constant to arrive at the corrected standard deviation. Since the correction is strictly positive, by construction, this procedure always generates a modified dispersion measure that is strictly greater than the observed dispersion measure.

[14] Instead of conditional mean $E(u_{it}|\varepsilon_{it})$, we also estimate conditional *mode* $M(u_{it}|\varepsilon_{it})$ to correct the dispersion bias, where the conditional mode is set equal to $-(\varepsilon_{it}\sigma_u^2)/\sigma^2$if $\varepsilon_{it} \leq 0$, and 0 otherwise (Jondrow et al. 1982). The resulting *DBF* values are highly correlated (Pearson and Spearman correlations exceeding 0.95) with the *DBF* values derived using the conditional mean estimator. We also use the alternative point estimator discussed in Battese and Coelli (1988) to estimate *DBF*. The resulting *DBF* measure is again found to be highly correlated with *DBF* calculated using the conditional mean estimator.

TABLE 3

Descriptive Statistics for Standard Deviation of Analysts' Earnings Forecasts, Modified Standard Deviation, and Dispersion Bias Factor (*DBF*)

Panel A: Overall Sample (n = 89,473)

	Mean	Standard Deviation	Lower Quartile	Median	Upper Quartile
STD	0.076	0.147	0.014	0.035	0.082
STDMOD	0.133	0.220	0.035	0.071	0.149
DBF	0.534	0.165	0.418	0.518	0.629

Panel B: Annual Trend (n = 89,473)

Year	Number of Observations	Mean (Standard Deviation) *STD*	Mean (Standard Deviation) *STDMOD*	Mean (Standard Deviation) *DBF*
1985	2167	0.103 (0.171)	0.182 (0.264)	0.524 (0.147)
1986	2416	0.115 (0.189)	0.205 (0.290)	0.531 (0.158)
1987	2675	0.123 (0.287)	0.218 (0.454)	0.529 (0.159)
1988	2796	0.099 (0.165)	0.178 (0.257)	0.527 (0.151)
1989	3132	0.102 (0.261)	0.180 (0.403)	0.530 (0.159)
1990	3727	0.104 (0.184)	0.184 (0.287)	0.529 (0.161)
1991	3912	0.094 (0.150)	0.174 (0.266)	0.529 (0.156)
1992	3975	0.084 (0.142)	0.149 (0.213)	0.533 (0.162)
1993	4119	0.077 (0.124)	0.138 (0.188)	0.530 (0.158)
1994	4227	0.066 (0.100)	0.117 (0.150)	0.534 (0.165)
1995	4492	0.067 (0.107)	0.117 (0.153)	0.536 (0.169)
1996	4952	0.072 (0.127)	0.122 (0.173)	0.537 (0.169)
1997	5433	0.063 (0.104)	0.108 (0.144)	0.540 (0.175)
1998	5554	0.068 (0.120)	0.116 (0.166)	0.540 (0.175)
1999	5315	0.064 (0.119)	0.111 (0.164)	0.543 (0.179)
2000	5209	0.070 (0.157)	0.118 (0.208)	0.543 (0.180)

(continued on next page)

TABLE 3 (continued)

Year	**Number of Observations**	**Mean (Standard Deviation)** ***STD***	**Mean (Standard Deviation)** ***STDMOD***	**Mean (Standard Deviation)** ***DBF***
2001	5573	0.071	0.124	0.534
		(0.147)	(0.206)	(0.166)
2002	5892	0.056	0.098	0.537
		(0.101)	(0.144)	(0.164)
2003	6532	0.059	0.105	0.534
		(0.116)	(0.163)	(0.158)
2004	7375	0.067	0.117	0.532
		(0.121)	(0.164)	(0.157)

The sample consists of 89,473 observations for the period 1985–2004. For each firm-quarter, there has to be at least two analysts' forecasts of annual earnings within 45 days before the quarterly earnings announcement, and within 30 days after the announcement. Pre- and post-announcement are considered two different observations. If an analyst does not revise her forecast after the quarterly earnings announcement, then we assume she does not change her forecast. If an analyst makes multiple forecasts within a measurement window, then the one closest to the announcement date is used.

Variable Definitions:
STD = standard deviation of individual analysts' earnings forecasts;
STDMOD = standard deviation adjusted for the downward bias in *STD*, where the bias is calculated for every sample observation based on the ML estimation of the following model:

$$\begin{aligned} Log(STD_{it}) = {} & \beta_0 + \beta_1 SIZE_{it} + \beta_2 BM_{it} + \beta_3 BETA_{it} + \beta_4 MOMENT_{it} + \beta_5 VAREPS_{it} + \beta_6 \Delta EPS_{it} + \beta_7 LOSS_{it} \\ & + \beta_8 RD_{it} + \beta_9 SDRES_{it} + \beta_{10} LOGNANALYST_{i,t} + \beta_{11} TURN_{it} + \beta_{12} DE_{it} + \beta_{13} SA_{it} \\ & + \beta_{14} LOG(ABSM)_{it} + \beta_{15} LOGP_{it} + v_{it} - u_{it};\ \text{and} \end{aligned}$$

DBF = Dispersion Bias Factor: $\frac{STDMOD-STD}{STDMOD}$.

average, the observed dispersion understates the dispersion based on unmanaged forecasts by 53.4 percent.[15] For a median representative firm, this understatement is 51.8 percent. Thus, our measure of dispersion bias appears to be both statistically and economically significant.

Panel B of Table 3 provides details on the year-wise distribution of average *DBF*. Interestingly, while the mean observed dispersion has steadily declined over time, the average *DBF* displays a more stable pattern. The trend over time in *DBF* seems to be slightly positive, providing some preliminary evidence that the downward bias may have increased a little bit over time.

Next, we analyze the factors that influence the variation in *DBF* in the cross-section. We focus on a set of eight firm and analyst characteristics that have been identified as being associated with analysts' herding and self-selection behavior. First, following Clement and Tse (2005), we identify forecasts as herding forecasts whenever a revised forecast falls between that analyst's prior forecast and the existing consensus forecast. We calculate *HERDING INTENSITY* on a firm-year basis as the number of herding forecasts divided by the total number of forecasts revisions. We predict a positive association between dispersion bias and *HERDING INTENSITY*.

We next introduce five firm-level measures of normalized variation in analysts' experience (*CV*(*EXPERIENCE*)), prior accuracy (*CV*(*ACCURACY*)), brokerage firm size (*CV*(*B_SIZE*)), forecasts' timeliness (*CV*(*TIMELINESS*)), and forecast frequency (*CV*(*FREQUENCY*)).[16] Hong et al. (2000) find that inexperienced analysts deviate less from consensus forecasts. Therefore, we predict that dispersion bias will be higher in firms with a high level of variation in the experience level of the analysts following the firm i.e., in firms with high *CV*(*EXPERIENCE*). It is also more likely that less capable analysts copy the star analysts' forecasts in those firms where there is wide variation in the capability of analysts. Therefore, we predict that dispersion bias will be increasing in *CV*(*ACCURACY*). High variability in the size of brokerage firms may lead to a greater level of dispersion bias, if one assumes that smaller brokerage firms employ lower-ability analysts who herd with the more capable analysts employed by bigger brokerage firms. We, therefore, predict a positive association between *CV*(*B_SIZE*) and dispersion bias. The possibility that the forecasts are clustered in time is positively related to a low level of variability in forecasts' timelines and frequency and since clustering is likely to be positively related to analysts' strategic behavior, we predict a negative association between dispersion bias and both *CV*(*TIMELINESS*) and *CV*(*FREQUENCY*).

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[15] Using I/B/E/S Detail file with split-adjusted data yields an average *DBF* of 53.1 percent.

To account for the possibility that some of the dispersion bias may be due to self-selection behavior by analysts who abstain from releasing a forecast when they have bad news, we include two additional variables in the cross-sectional analysis. The results in McNichols and O'Brien (1997) and Hayes and Levine (2000) suggest a positive association between forecast optimism and self-selection. Our measure of analysts' optimism, denoted as *OPTIMISM*, is calculated as the difference between analysts' mean forecast and actual earnings, scaled by stock price. Suppression of bad news and the incidence of self-selection are likely to be relatively high in firms with a high level of financial distress, so we also include the Zmijewski (1984) distress score as an additional explanatory variable and predict a positive association between dispersion bias and *DISTRESS*.

The comparisons in Panel A of Table 4 reveal significant differences in the *DBF* between high and low groups of firms across the eight firm characteristics noted earlier. The average *DBF* is significantly smaller for the low group when compared to the high group in the case of *HERDING INTENSITY*, *CV*(*EXPERIENCE*), *CV*(*ACCURACY*), *DISTRESS*, and *OPTIMISM*. In contrast, the average *DBF* for the high group is significantly lower for *CV*(*B_SIZE*), *CV*(*TIMELINESS*), and *CV*(*FREQUENCY*).

Panel B of Table 4 provides results for a pooled cross-sectional regression of the *DBF* on the fractional rank of *HERDING INTENSITY*, the five firm-level analysts' heterogeneity measures (*CV*(*EXPERIENCE*), *CV*(*ACCURACY*), *CV*(*B_SIZE*), *CV*(*TIMELINESS*), and *CV*(*FREQUENCY*)), *DISTRESS*, and *OPTIMISM*. The regression is estimated at the firm-quarter level using all observations for which data on all eight explanatory variables are available. Year dummies as well as indicator variables for the points in time during the year when dispersion bias factor is estimated are included in the regression specification. All eight variables are found to be significantly associated with the dispersion bias. As predicted, we observe a positive association of dispersion bias with herding intensity, variation in analyst experience, variation in prior accuracy, financial distress, and analyst optimism. We also find that dispersion bias is negatively associated with variation in forecast timeliness and frequency. Contrary to our prediction, we observe a negative association between dispersion bias and variation in broker size. Analysts from smaller brokerage firms may try to differentiate themselves from their counterparts in bigger firms by communicating their true beliefs. This could be a possible explanation for this unexpected result.

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[16] *CV*(.) denotes the coefficient of variation of a particular attribute and is measured as the standard deviation of that attribute divided by its mean.

TABLE 4

Empirical Characteristics of Dispersion Bias Factor (*DBF*)

Panel A: Comparing Dispersion Bias Factor (*DBF*) in Different Subsamples (n = 68,497)

	Subsample with Smaller Than or Equal to Median Criterion			Subsample with Greater Than Median Criterion			
Criteria to Divide the Sample	**Number of Observations**	**Mean *DBF* (μ_1)**	**Standard Deviation of *DBF***	**Number of Observations**	**Mean *DBF* (μ_2)**	**Standard Deviation of *DBF***	**t-statistic for H_0: $\mu_1 = \mu_2$**
HERDING INTENSITY	34182	0.518	0.155	34315	0.547	0.164	23.65*
CV(EXPERIENCE)	34238	0.529	0.163	34259	0.535	0.159	5.22*
CV(ACCURACY)	34252	0.510	0.163	34245	0.554	0.155	36.60*
CV(B_SIZE)	34243	0.537	0.165	34254	0.527	0.156	−8.58**
CV(TIMELINESS)	34241	0.531	0.166	34256	0.528	0.155	−2.87**
CV(FREQUENCY)	34211	0.537	0.164	34286	0.527	0.157	−8.16**
DISTRESS	34243	0.507	0.158	34254	0.558	0.159	42.05*
OPTIMISM	34250	0.511	0.156	34247	0.553	0.162	35.06*

Panel B: Regression Analysis of Dispersion Bias Factor (*DBF*) (n = 68,497)

	Predicted Sign	**Estimated Coefficient**	**t-statistic**	**p-value**
Intercept		0.364	61.78	< 0.001
HERDING INTENSITY	+	0.113	26.32	< 0.001
CV(EXPERIENCE)	+	0.010	4.31	< 0.001
CV(ACCURACY)	+	0.070	32.92	< 0.001
CV(B_SIZE)	+	−0.028	−13.16	< 0.001
CV(TIMELINESS)	−	−0.005	−2.24	0.025
CV(FREQUENCY)	−	−0.017	−7.74	< 0.001
DISTRESS	+	0.216	17.30	< 0.001
OPTIMISM	+	0.019	33.63	< 0.001
Year and Year-Time Control		Yes		
Adjusted R^2		0.063		

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*, ** The results indicate that $\mu_1 < \mu_2$ and $\mu_1 > \mu_2$, respectively.

Panel A presents the results of comparing *DBF* in different subsamples.

Panel B shows the results of pooled-regression for the following model:

$$DBF_{i,t} = \gamma_0 + \gamma_1 HERDING\ INTENSITY_{i,t} + \gamma_2 CV(EXPERIENCE)_{i,t} + \gamma_3 CV(ACCURACY)_{i,t} + \gamma_4 CV(TIMELINESS)_{i,t} + \gamma_5 CV(B\_SIZE)_{i,t} + \gamma_6 CV(FREQUENCY)_{i,t} + \gamma_7 DISTRESS_{i,t} + \gamma_8 OPTIMISM_{i,t} + error_{i,t}.$$

We use the fractional rank for all the independent variables. We also include year dummy and year-time dummy (*Q1-PRE, Q1-POST, Q2-PRE, Q2-POST*, and *Q3-PRE*) in the regression.

The sample consists of 68,497 observations for the period 1985–2004. For each firm-quarter, there has to be at least two analysts' forecasts of annual earnings within 45 days before the quarterly earnings announcement, and within 30 days after the announcement. Pre- and post-announcement are considered two different observations. If an analyst does not revise her forecast after the quarterly earnings announcement, then we assume she does not change her forecast. If an analyst makes multiple forecasts within a measurement window, then the one closest to the announcement date is used.

(continued on next page)

TABLE 4 (continued)

Variable Definitions:

DBF = Dispersion Bias Factor $\frac{STDMOD-STD}{STDMOD}$, where *STD* is standard deviation of analysts' forecast, *STDMOD* is standard deviation adjusted for the downward bias in *STD*, and the bias is calculated for every sample observation based on the ML estimation of the following model:

$$Log(STD_{it}) = \beta_0 + \beta_1 SIZE_{it} + \beta_2 BM_{it} + \beta_3 BETA_{it} + \beta_4 MOMENT_{it} + \beta_5 VAREPS_{it} + \beta_6 \Delta EPS_{it} + \beta_7 LOSS_{it} + \beta_8 RD_{it} + \beta_9 SDRES_{it} + \beta_{10} LOGNANALYST_{it} + \beta_{11} TURN_{it} + \beta_{12} DE_{it} + \beta_{13} SA_{it} + \beta_{14} LOG(ABSM)_{it} + \beta_{15} LOGP_{it} + v_{it} - u_{it};$$

HERDING INTENSITY = number of herding forecasts divided by the total number of forecasts for each firm-year. A herding forecast is a revised forecast that is between the existing consensus and the analyst's previous forecast;

CV(B_SIZE) = standard deviation of the size of the brokerage houses that employ analysts who release annual forecasts for a certain firm, divided by the mean size of the brokerage houses. Size of a brokerage house is measured by the number of analysts the brokerage house employed in a certain quarter;

CV(EXPERIENCE) = standard deviation of the analysts' tenure divided by the mean tenure of the analysts who release annual forecasts for a certain firm. An analyst's tenure is measured as how many days this analyst has been releasing annual earnings forecasts in the I/B/E/S database;

CV(TIMELINESS) = standard deviation of analysts' forecast timing, divided by the mean timing. Forecast timing is measured as the number of days between a forecast's release date and the quarterly earnings announcement date;

CV(ACCURACY) = standard deviation of forecast accuracy deciles, divided by the mean forecast accuracy deciles. An analyst's forecast accuracy is calculated based on her last year's average absolute forecast errors within an industry. The bigger the average forecast errors are, the lower the deciles she belongs to;

CV(FREQUENCY) = standard deviation of forecast frequency of the analysts who release annual forecasts for a certain firm, divided by the mean forecast frequency. An analyst's forecast frequency is calculated as the number of annual forecasts she released for a certain firm in a given year;

OPTIMISM = analysts' mean forecast minus earnings, scaled by stock price; and

DISTRESS = Zmijewski's (1984) Financial Distress Score.

Subsamples Where Observed Dispersion is Greater than Unmanaged Dispersion

As pointed out in the leader-follower analyst setup described in Appendix A, there could be situations in which analysts' strategic behavior may lead to observed dispersion exceeding the dispersion of unmanaged forecasts.[17] If this is the case, then it should be possible to fit the error model $\varepsilon = v + u$, rather than the model in Equation (2) which specifies $\varepsilon = v - u$, to parts of the sample where we expect observed dispersion to exceed dispersion of unmanaged forecasts.

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[17] Bernhardt et al. (2006) examine the location of individual analyst forecasts relative to existing consensus and actual earnings and suggest that there is a 60 percent chance of an analyst at any particular point in time moving away from existing consensus. This result and our finding that, on average, dispersion of unmanaged forecasts is greater than observed dispersion are not necessarily contradictory. This is because of the following reasons: (1) observed dispersion is affected by not only whether analysts bias their forecasts away from or toward consensus, but also by how much; and (2) herding/anti-herding behavior is defined relative to *existing* consensus (which may itself be based on some biased past forecasts) in Bernhardt et al. (2006). This is different from the consensus based on unmanaged forecasts. In fact, it is easy to construct a number of examples where a majority of the analysts forecast away from existing consensus but the final observed dispersion is less than the dispersion of unmanaged forecasts. Consider for example a group of five analysts (A, B, C, D, and E) following a firm with unmanaged forecasts of 0.21, 0.22, 0.25, 0.29, and 0.3. Let E release her forecast first. Looking at the consensus of 0.3, analyst D moves her unbiased forecast away to, say, 0.26. Now the existing consensus is 0.28 ((0.3 + 0.26)/2). Let us say that B then issues her unbiased forecast of 0.22. The revised existing consensus is (0.3 + 0.26 + 0.22)/3 = 0.26. Let A then release her anti-herding forecast of 0.20 making the consensus (0.3 + 0.26 + 0.22 + 0.2)/4 = 0.245. Finally, C anti-herds and releases a forecast of 0.26 resulting in an observed forecasts vector of (0.2, 0.22, 0.26, 0.26, and 0.3). We have a case where three out of four follower analysts anti-herd and one analyst releases an unbiased forecast. But observed dispersion based on the managed forecasts is 0.039, whereas the dispersion based on the unmanaged forecasts is 0.040.

The cross-sectional distribution of the various determinants of the dispersion bias factor provides us with some guidance on the subsamples where upward dispersion bias is likely to be present. If a particular determinant has a positive (negative) association with the dispersion bias factor, then the bias is expected to be lower for relatively low (high) values of this variable. Therefore, subsamples characterized by low (high) values of this determinant are likely to have observed dispersion that is greater than dispersion of unmanaged forecasts. For example, given the significant positive association between dispersion bias and *HERDING INTENSITY*, we chose subsamples from the bottom 10, 25, and 50 percent of the overall sample when sorted by *HERDING INTENSITY* to check whether observed dispersion is greater than dispersion of unmanaged forecasts i.e., $\varepsilon = v + u$, in these samples. We found that in the 25 percent and 50 percent subsamples, the ML estimation yielded coefficients that were identical when compared to OLS coefficients and the estimated value of σ_u was indistinguishable from zero. In contrast, we were able to fit the $\varepsilon = v - u$ model for these two subsamples and the resulting σ_us were significant, confirming an on average downward dispersion bias.

However, in the subsample characterized by the bottom 10 percent of observations, when sorted based on *HERDING INTENSITY*, the ML procedure confirmed an, on average, *negative* dispersion bias factor. For this subsample, by construction, observed dispersion is greater than the estimated modified dispersion. For this 10 percent sample, the average value of the observed dispersion, modified dispersion and *DBF* respectively, were 0.069, 0.029 and −1.125. Not surprisingly, ML estimation for the subsample containing the remaining 90 percent of the observations provided results that were consistent with observed dispersion being less than the dispersion of unmanaged forecasts, with average value of the observed dispersion, modified dispersion and *DBF* at the level of 0.074, 0.142, and 0.561, respectively. We merged the two subsamples and compared the empirical distribution of the modified dispersion of the merged full sample with the corresponding distribution in Table 3, Panel A. The mean and the standard deviation of modified dispersion for the merged sample were 0.129 and 0.219 and these are very close to the original values of 0.133 and 0.220. The Pearson and Spearman correlations between the two distributions were also very high, 0.973 and 0.918, respectively, providing some evidence that the assumption of downward bias in observed dispersion for the full sample is a reasonable one despite the possibility that upward bias in observed dispersion may be present in parts of the sample.[18]

V. EMPIRICAL IMPLICATIONS

The preceding analysis provides empirical evidence that analysts' strategic behavior creates a downward bias in forecast dispersion. We have also demonstrated that observed dispersion can be adjusted upward on an observation-specific basis to generate a modified measure of dispersion that ostensibly estimates the dispersion that would have resulted if the analysts had not strategically managed their forecasts. In this section, we discuss specific research settings and empirically demonstrate where and how researchers can benefit from using our modified measure of dispersion.

Following Barron et al. (1998) a number of studies have used observed values of dispersion, variance of analysts' forecasts, (D), squared error in the mean forecast (SE), and the number of analysts (N) to estimate proxies for consensus, and public and private information precision, i.e.,:

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[18] The ML procedure confirmed an on average, negative, dispersion bias factor in only one other subsample, characterized by the top 10 percent of observations when sorted by *CV(TIMELINESS)*. The dispersion bias factor was, on average, significantly positive for the remaining 90 percent of the full sample. Here again, the Pearson and Spearman correlations between the empirical distributions of modified dispersion were very high, 0.978 and 0.925, respectively.

$$CONSENSUS = \frac{SE - \frac{D}{N}}{\left(SE - \frac{D}{N}\right) + D};\ PUBLIC = \frac{SE - \frac{D}{N}}{\left[\left(SE - \frac{D}{N}\right) + D\right]^2};$$

$$\text{and } PRIVATE = \frac{D}{\left[\left(SE - \frac{D}{N}\right) + D\right]^2}.$$

Some studies (e.g., Barron et al. 2002) use these proxies, often referred to as BKLS measures, to examine the relative changes in public information quality and private information quality around events such as earnings announcements, while other studies (e.g., Botosan et al. 2004) exploit these measures to investigate the relative impact of the two different information sources (public versus private) on variables of interest such as cost of capital.

For given values of squared error in mean forecast (*SE*) and the number of analysts (*N*), it is straightforward to verify that both *CONSENSUS* and *PUBLIC* decrease when dispersion (*D*) increases. It is also the case that the partial derivative of *PRIVATE* with respect to *D* is positive whenever the squared error in mean forecast is greater than dispersion. Given the significant downward dispersion bias that we have documented earlier, these observations further suggest that the importance of public information relative to private information may have been overstated in prior studies.

Table 5 provides estimates of BKLS measures i.e., *CONSENSUS*, *PUBLIC*, and *PRIVATE* for a sample of 1,828 firms for which these variables can be calculated before and after the first, second, and third quarter earnings announcements.[19] Panel A shows the estimates when the observed dispersion measure is used and Panel B shows the estimates calculated with our bias-adjusted dispersion. Consistent with the above intuition we find significant reduction in the estimates of consensus, public information precision, and a significant increase in private information precision when our modified dispersion measure is used to replace the observed dispersion measure. Consider, for example, the forecast window following the third quarter earnings announcement (*Q3-POST*). The median *CONSENSUS*, *PUBLIC*, and *PRIVATE* measures in Panel A are 0.760, 50.168, and 17.430, respectively. The first two measures have significantly lower median values in Panel B, i.e., 0.412 and 15.211, and *PRIVATE* shows a significant increase to 25.654. A similar pattern is observed for all the other five forecast windows.

In addition, a number of striking differences emerge when Panels A and B of Table 5 results are compared. One of the main results in Barron et al. (2002), which is also confirmed by our results in Panel A, is that increases in *PUBLIC* around earnings announcements are greater than the increases in *PRIVATE*. In contrast, our results in the last column of Panel B based on the modified measure of dispersion suggest that *PRIVATE* increases around earnings announcements are significantly *greater* than increases in *PUBLIC*.[20] While both panels indicate that *CONSENSUS* continually decreases as one moves closer to the end of the year, the reduction in *CONSENSUS* is more than twice as much in Panel B, moving from 0.82 before first quarter announcement

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[19] We only included those firm-years where all estimates of public information quality were positive since the theoretical development in BKLS always yields a positive consensus. We also repeated our analysis with an expanded sample where *PUBLIC* and the *CONSENSUS* were set to zero for cases where the estimated *PUBLIC* was negative. For these observations, *PRIVATE* was estimated as 1/*D*. Our inferences were similar in this expanded sample.

[20] Consider, for example, the increase in public information precision (10.307 − 8.949 = 1.358) and the increase in private information precision (0.774 − 0.459 = 0.315) around the first quarter earnings announcement in Panel A. Contrast these increases with the corresponding values (5.930 − 5.505 = 0.425) and (2.151 − 1.484 = 0.667) around the first quarter earnings announcement in Panel B.

TABLE 5

Consensus, Precision of Public Information, and Precision of Private Information around Interim Earnings Announcements

Panel A: *CONSENSUS*, *PUBLIC*, and *PRIVATE* Calculated using Observed Dispersion (n = 10,968)

Forecast Windows	Median *CONSENSUS*	Test of Change in *CONSENSUS* around Earnings Announcement[a]	Median *PUBLIC*	Test of Change in *PUBLIC* around Earnings Announcement[a]	Median *PRIVATE*	Test of Change in *PRIVATE* around Earnings Announcement[a]	Comparison of Change in *PUBLIC* and Change in *PRIVATE* around Earnings Announcement[b]
Q1-PRE	0.951		8.949		0.459		
Q1-POST	0.935	(< 0.001)	10.307	(< 0.001)	0.774	(< 0.001)	(< 0.001)[c]
Q2-PRE	0.932		15.336		1.107		
Q2-POST	0.896	(< 0.001)	19.433	(< 0.001)	2.395	(< 0.001)	(< 0.001)[c]
Q3-PRE	0.876		36.214		4.880		
Q3-POST	0.760	(< 0.001)	50.168	(< 0.001)	17.430	(< 0.001)	(< 0.001)[c]

Panel B: *CONSENSUS*, *PUBLIC*, and *PRIVATE* Calculated using Adjusted Dispersion (n = 10,968)

Forecast Windows	Median *CONSENSUS*	Test of Change in *CONSENSUS* around Earnings Announcement[a]	Median *PUBLIC*	Test of Change in *PUBLIC* around Earnings Announcement[a]	Median *PRIVATE*	Test of Change in *PRIVATE* around Earnings Announcement[a]	Comparison of Change in *PUBLIC* and Change in *PRIVATE* around Earnings Announcement[b]
Q1-PRE	0.816		5.506		1.489		
Q1-POST	0.765	(< 0.001)	5.930	(< 0.001)	2.151	(< 0.001)	(0.001)[d]
Q2-PRE	0.770		8.691		2.961		
Q2-POST	0.676	(< 0.001)	10.023	(< 0.001)	5.292	(< 0.001)	(<.001)[d]
Q3-PRE	0.603		16.211		11.185		
Q3-POST	0.412	(< 0.001)	15.211	(< 0.001)[e]	25.654	(< 0.001)	(<.001)[d]

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[a] Wilcoxon signed ranks test of the null hypotheses that $\Delta CONSENSUS = 0$, $\Delta PUBLIC = 0$, and $\Delta PRIVATE = 0$.

[b] Wilcoxon signed ranks. We base tests on the statistic: $(\Delta PUBLIC - \Delta PRIVATE)$, the null hypothesis is that $(\Delta PUBLIC - \Delta PRIVATE) = 0$.

[c] Results indicate that $\Delta PUBLIC > \Delta PRIVATE$.

[d] Results indicate that $\Delta PUBLIC < \Delta PRIVATE$.

(continued on next page)

TABLE 5 (continued)

[e] Results indicate that $\Delta PUBLIC < 0$, i.e., precision of *public* information decreased after 3rd quarter's earnings announcement.

This table demonstrates how precision of public information (*PUBLIC*) and precision of private information (*PRIVATE*) contained in analysts' forecasts and the ratio of common-to-total information (*CONSENSUS*) change around interim earnings announcements throughout the year. In Panel A, *CONSENSUS*, *PUBLIC*, and *PRIVATE* are calculated based on observed analysts' forecasts dispersion, while in Panel B, they are calculated based on dispersion adjusted for analysts' strategic behavior. For both panels, forecast windows before earnings announcements run from day −45 through −1. Forecast windows after earnings announcements run from day 0 through 29, where day 0 is the Compustat quarterly earnings announcement date. This analysis requires each firm-year to have valid observations in all six windows (*Q1-PRE*, *Q1-POST*, *Q2-PRE*, *Q2-POST*, *Q3-PRE*, and *Q3-POST*). In the sample used, there are 1,828 firm-years, each having six observations, which lead to 10,968 observations overall.

Variable Definitions:

$CONSENSUS = \frac{SE - \frac{D}{N}}{(SE - \frac{D}{N}) + D}$, where $SE = (\text{mean forecasts} - \text{EPS})^2$, $N =$ number of analysts, $D = (STD)^2$, $STD =$ standard deviation of analysts' forecasts;

$PUBLIC = \frac{SE - \frac{D}{N}}{[(SE - \frac{D}{N}) + D]^2}$; and

$PRIVATE = \frac{D}{[(SE - \frac{D}{N}) + D]^2}$.

(*Q1-PRE*) to 0.41 after third quarter announcement (*Q3-POST*) when compared to corresponding results in Panel A (from 0.95 to 0.76). These results provide strong evidence that a significant reduction in consensus over time is caused by relatively substantial increases in quality of private information. This contrasts with the Panel A results that indicate significant increases in public information quality relative to modest increases in private information quality moderate the reduction in consensus. More generally, our results indicate that researchers investigating how changes in consensus, private, and public information around information events affect analysts' information-gathering activities should use our modified measure of dispersion in order to minimize the possibilities of drawing incorrect inferences.

The cross-sectional determinants of dispersion bias also provide some guidance on the types of firms for which measures of public and private information based on the observed dispersion will be more significantly biased toward public information. The cross-sectional results in Table 4, Panel B suggest that the difference between *CONSENSUS* derived from observed dispersion and its counterpart derived from our proposed dispersion measure will be significantly larger for those firms that have higher herding tendency (*HERDING INTENSITY*), higher variation in analyst experience (*CV*(*EXPERIENCE*)), higher variation in analyst capabilities (*CV*(*ACCURACY*)), higher levels of financial distress (*DISTRESS*), and higher levels of forecast optimism (*OPTIMISM*), and will be significantly lower in firms with higher levels of *CV*(*TIMELINESS*) and *CV*(*FREQUENCY*). To test these predictions, we regress the bias in *CONSENSUS* (estimated as the difference between consensus based on the observed dispersion measure and that based on the modified dispersion measure) on the above determinants of analysts' strategic behavior using a pooled cross-sectional regression with controls for time point of the year such as *Q1-PRE*, *Q1-POST*, etc., and the year. The regression results are presented in Table 6.

Consistent with the predictions, we find significant positive associations between the magnitude of upward bias in *CONSENSUS* and *HERDING INTENSITY*, *CV*(*ACCURACY*), *DISTRESS*, and *OPTIMISM*. In contrast to our predictions, we also observe a positive association between the bias in *CONSENSUS* and *CV*(*TIMELINESS*) *and CV*(*FREQUENCY*).[21] Overall, the regression results are consistent with public information quality being significantly overstated relative to private information quality when observed dispersion is used, particularly in firms with a greater degree of analyst strategic behavior.

Given the empirical evidence that private (public) information quality is understated (overstated), we next examine a research setting where the relative impact of public and private information precision plays a central role in research design. Botosan et al. (2004; hereafter, BPX) find that cost of equity capital is decreasing in consensus and increasing in the difference between a firm's private information precision rank and the firm's public information precision rank, a variable they label *RDIFF*. Combining the two results, BPX infer that cost of equity capital is decreasing in public information precision and increasing in private information precision. We examine whether these inferences are robust to how a change in the dispersion measure affects *CONSENSUS* and *RDIFF*. Table 7 provides the results of the following regression specification used in Table 5 of BPX. Specifically, we estimate:

$$\begin{aligned} \textit{Cost of equity capital} = {} & \theta_0 + \theta_1 BETA_{it} + \theta_2 GROW_{it} + \theta_3 LMKVL_{it} + \theta_4 BP_{it} \\ & + \theta_5 CONSENSUS_{it}\ (\text{or } RDIFF_{it}) + error_{it}, \end{aligned}$$

and provide the summary of nine annual cross-sectional regressions for the period from 1993 to

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[21] It is possible that firms that have a high level of variability in forecast timeliness and frequency are also those with a significant quantity of liquidity trading. We speculate that these firms may be characterized by low consensus of opinion among traders and therefore a high level of estimation bias in the consensus measure.

TABLE 6

Cross-Sectional Determinants of the Bias in BKLS Consensus (n = 10,968)

	Predicted Sign	Estimated Coefficient	t-statistic	p-value
Intercept		0.173	16.98	< 0.001
HERDING INTENSITY	+	0.218	17.58	< 0.001
CV(EXPERIENCE)	+	0.001	0.71	0.868
CV(ACCURACY)	+	0.084	16.26	< 0.001
CV(B_SIZE)	+	0.006	1.21	0.228
CV(TIMELINESS)	−	0.013	4.24	< 0.001
CV(FREQUENCY)	−	0.055	6.39	< 0.001
DISTRESS	+	0.004	3.11	< 0.001
OPTIMISM	+	0.868	30.23	< 0.001
Year and Year-time Control		Yes		
Adjusted R^2		0.224		

This table shows the pooled-regression results of the following model:

$$\begin{aligned} CONSENSUS_{it} - CONSENSUSMOD_{it} = {} & \delta_0 + \delta_1 HERDING\ INTENSITY_{i,t} + \delta_2 CV(EXPERIENCE)_{i,t} \\ & + \delta_3 CV(ACCURACY)_{i,t} + \delta_4 CV(B\_SIZE)_{i,t} + \delta_5 CV(TIMELINESS)_{i,t} \\ & + \delta_6 CV(FREQUENCY)_{i,t} + \delta_7 DISTRESS_{i,t} + \delta_8 OPTIMISM_{i,t} \\ & + error_{i,t}. \end{aligned}$$

We use the fractional rank for all the independent variables. We also include year dummy and year-time dummy (*Q1-PRE*, *Q1-POST*, *Q2-PRE*, *Q2-POST*, and *Q3-PRE*) in the regression.

In the sample used, forecast windows before earnings announcements run from day −45 through −1. Forecast windows after earnings announcements run from day 0 through 29, where day 0 is the Compustat quarterly earnings announcement date. We require each firm-year to have valid observations in all six windows (*Q1-PRE*, *Q1-POST*, *Q2-PRE*, *Q2-POST*, *Q3-PRE*, and *Q3-POST*). There are 1,828 firm-years, each having six observations, which lead to 10,968 observations over all.

Variable Definitions:

$CONSENSUS = \frac{SE - \frac{D}{N}}{\left(SE - \frac{D}{N}\right) + D}$, where $SE = (\text{mean forecasts} - EPS)^2$, N = number of analysts, $D = (STD)^2$, STD = standard deviation of analysts' forecasts;

$CONSENSUSMOD = \frac{SE - \frac{DMOD}{N}}{\left(SE - \frac{DMOD}{N}\right) + DMOD}$, where $DMOD = (STDMOD)^2$, *STDMOD* is the standard deviation adjusted for the downward bias in *STD*, where the bias is calculated for every sample observation based on the ML estimation of the following model:

$$\begin{aligned} Log(STD_{it}) = {} & \beta_0 + \beta_1 SIZE_{it} + \beta_2 BM_{it} + \beta_3 BETA_{it} + \beta_4 MOMENT_{it} + \beta_5 VAREPS_{it} + \beta_6 \Delta EPS_{it} + \beta_7 LOSS_{it} \\ & + \beta_8 RD_{it} + \beta_9 SDRES_{it} + \beta_{10} LOGNANALYST_{it} + \beta_{11} TURN_{it} + \beta_{12} DE_{it} + \beta_{13} SA_{it} \\ & + \beta_{14} LOG(ABSM)_{it} + \beta_{15} LOGP_{it} + v_{it} - u_{it}; \end{aligned}$$

HERDING INTENSITY = number of herding forecasts divided by the total number of forecasts for each firm-year. A herding forecast is a revised forecast that is between the existing consensus and the analyst's previous forecast;

CV(B_SIZE) = standard deviation of the size of the brokerage houses that employ analysts who release annual forecast for a certain firm, divided by the mean size of the brokerage houses. Size of a brokerage house is measured by the number of analysts the brokerage house employed in a certain quarter;

CV(EXPERIENCE) = standard deviation of the analysts' tenure divided by the mean tenure of the analysts who release annual forecasts for a certain firm. An analyst's tenure is measured as how many days this analyst has been releasing annual earnings forecasts in the I/B/E/S database;

CV(TIMELINESS) = standard deviation of analysts' forecast timing, divided by the mean timing. Forecast timing is measured as the number of days between a forecast's release date and the quarterly earnings announcement date;

(continued on next page)

TABLE 6 (continued)

CV(ACCURACY) = standard deviation of forecast accuracy deciles, divided by the mean forecast accuracy deciles. An analyst's forecast accuracy is calculated based on her last year's average absolute forecast errors within an industry. The bigger the average forecast errors are, the lower the deciles she belongs to;
CV(FREQUENCY) = standard deviation of forecast frequency of the analysts who release annual forecast for a certain firm, divided by the mean forecast frequency. An analyst's forecast frequency is calculated as the number of annual forecasts she released for a certain firm in a given year;
OPTIMISM = analysts' mean forecast minus earnings, scaled by stock price; and
DISTRESS = Zmijewski's (1984) Financial Distress Score.

2001. Table 7, Panels A and B, respectively, present the results when *CONSENSUS* and *RDIFF* are calculated using observed and modified dispersion measures. As can be seen from Panel B, the negative association between cost of capital and consensus continues to hold when consensus is derived from the modified dispersion measure. However, the association between cost of capital and *RDIFF* reverses sign from positive in Panel A to significantly *negative* in Panel B when our modified dispersion measure is used to estimate public and private information precision. The sign-reversal on *RDIFF* is also observed when both *CONSENSUS* and *RDIFF* are included in the regression specification. The regression coefficients on the *CONSENSUS* and *RDIFF* in Panel B suggest that *both* relatively high levels of public information precision and relatively high levels of private information precision are associated with lower levels of information asymmetry and, therefore, a lower cost of equity capital. Interestingly, this empirical non-monotonic relationship between public information quality and information asymmetry is consistent with theoretical results of endogenous information acquisition described in Kim and Verrecchia (1991).[22]

Studies that Use Dispersion as an Independent Variable

Our empirical results, which indicate that there is a downward bias in dispersion and that the modified dispersion measure has a higher cross-sectional variation than observed dispersion, also have crucial implications for research designs where dispersion features as an important independent variable and observed dispersion are used as a proxy for the dispersion construct.[23] Specifically, one cannot appeal to the standard result from the classical errors-in-variable approach that measurement error in an independent variable biases its regression coefficient toward zero. This is because the measurement error in dispersion has a *negative* correlation with the underlying construct.[24]

Maddala (2001, 459) shows that in the correlated-errors-in-variable situations, the covariance between the true measure and measurement error has a significant influence on the nature and magnitude of the bias in the regression coefficient. When negative correlated error is present, three

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[22] Kim and Verrecchia (1991) show that information asymmetry increases as the precision of an anticipated public announcement increases up to some point, and steadily decreases thereafter (that is, its behavior is unimodal, Proposition 2 [Kim and Verrecchia 1991, 286]).

[23] We observe that the variance of observed dispersion is smaller than variance of our modified dispersion measure not only in our full sample, but also in subsamples of small and large firms, early and late sample period observations, as well as early in the year observations and late in the year observations.

[24] This is because, *variance of observed dispersion* = *variance of dispersion* + *variance of error* + 2 *times the covariance between dispersion and error*. The first two terms on the right-hand side are strictly positive; therefore, it is necessary that the covariance between dispersion and error be negative for observed dispersion to have a lower variance than dispersion.

TABLE 7

Multivariate Tests of the Association between Cost of Equity Capital and Relative Information Quality Measures

Panel A: *CONSENSUS* and *RDIFF* Calculated Using Observed Dispersion (n = 10,789)

Dependent Variable = Cost of Equity Capital

	Intercept	*BETA*	*GROW*	*LMKVL*	*BP*	*CONSENSUS*	*RDIFF*	Adjusted R^2
Pred. Sign		(+)	(+)	(−)	(+)	(−)	(−)	
Coefficient	0.127	0.005	0.039	−0.005	0.025	−0.003		
(t-statistic)	(22.85)	(4.00)	(2.30)	(−5.95)	(16.26)	(−2.02)		19.66%
Coefficient	0.119	0.005	0.043	−0.005	0.022		0.020	
(t-statistic)	(20.37)	(3.86)	(2.58)	(−6.53)	(12.97)		(2.11)	20.69%
Coefficient	0.115	0.005	0.043	−0.005	0.021	−0.005	0.022	20.86%
(t-statistic)	(19.53)	(3.87)	(2.60)	(−6.17)	(12.94)	(−3.82)	(7.88)	

Panel B: *CONSENSUS* and *RDIFF* Calculated Using Adjusted Dispersion (n = 10,789)

Dependent Variable = Cost of Equity Capital

	Intercept	*BETA*	*GROW*	*LMKVL*	*BP*	*CONSENSUS*	*RDIFF*	Adjusted R^2
Pred. Sign		(+)	(+)	(−)	(+)	(−)	(−)	
Coefficient	0.122	0.005	0.039	−0.004	0.025	−0.005		
(t-statistic)	(20.04)	(4.01)	(2.33)	(−5.32)	(16.56)	(−3.40)		20.72%
Coefficient	0.125	0.005	0.039	−0.004	0.025		−0.027	
(t-statistic)	(19.87)	(4.02)	(2.31)	(−5.45)	(11.24)		(−4.54)	21.58%
Coefficient	0.113	0.005	0.039	−0.004	0.025	−0.012	−0.030	
(t-statistic)	(22.65)	(3.97)	(2.36)	(−5.31)	(15.82)	(−5.42)	(−14.94)	23.96%

In both Panel A and Panel B, each row is a summary of nine annual cross-sectional regressions from 1993–2001. The reported coefficients are the mean coefficients. The t-statistics are based on the standard error of the coefficient estimates across the nine years, adjusted for auto-correlation in the annual coefficients based on an assumed AR1 autocorrelation structure. The sample contains 10,789 firm-year observations, ranging from 849 firms in 1993 to 1,431 firms in 1999. In Panel A both *CONSENSUS* and *RDIFF* are calculated using observed dispersion, while in Panel B they are calculated based on dispersion adjusted for analysts' strategic behavior.

Variable Definitions:

$Cost\ of\ Equity\ Capital = A + \sqrt{A^2 + \frac{EPS_1}{P_0}\left[\frac{1}{2}\left(\frac{EPS_2 - EPS_1}{EPS_1} + \frac{EPS_5 - EPS_4}{EPS_4}\right) - (r_f - 0.03)\right]}$ (based on a valuation model originating from Ohlson and Juettner-Nauroth [2005] and estimated as in Gode and Mohanram [2003]) where EPS_i is analysts' earnings forecasts for the *i*th year in the future, P_0 is the closing price for the estimated period, and $A = \frac{1}{2}\left[(r_f - 0.03) + \frac{dps_1}{P_0}\right]$, dps_1 = forecasted dividend per share, r_f = yield on ten-year notes;

BETA = beta estimated using a market model with a minimum of 30 monthly returns over the 60 months prior to June 30th of the year expected cost of capital is estimated;

GROW = I/B/E/S forecasts of long-range (three to five years) earnings growth;

LMKVL = natural log of the market value of equity;

BP = book value of common equity divided by the market value of common equity;

$CONSENSUS = \frac{SE - \frac{D}{N}}{\left(SE - \frac{D}{N}\right) + D}$, where $SE = (\text{mean forecasts} - EPS)^2$, N = number of analysts, $D = (STD)^2$, STD = standard deviation of analysts' forecasts; and

RDIFF = *RPRIVATE* − *RPUBLIC*, where *RPRIVATE* is the fractional rank of *PRIVATE*, *RPUBLIC* is the fractional rank of *PUBLIC*, and $PUBLIC = \frac{SE - \frac{D}{N}}{\left[\left(SE - \frac{D}{N}\right) + D\right]^2}$, $PRIVATE = \frac{D}{\left[\left(SE - \frac{D}{N}\right) + D\right]^2}$.

mutually exclusive cases are possible: (1) sign-reversal: the observed and true regression coefficients have *opposite* signs, (2) amplification: the coefficients have the same sign and the observed coefficient is biased away from zero, and (3) attenuation: the coefficients have the same sign and the observed coefficient is biased toward zero.

The above discussion provides guidance about specific situations where the use of our modified measure of dispersion is warranted. First, the fact that sign-reversal is a possibility has implications for studies that develop and test competing hypotheses about the predicted association between a variable of interest and dispersion. It is important for these studies to necessarily verify that empirical evidence in support of one of the two competing hypotheses, when observed dispersion is used as a proxy, is not overturned in favor of the alternative hypothesis when the modified dispersion measure suggested in this paper is used.[25] Second, the amplification effect, if present, may be problematic for those studies where the observed magnitude of the regression coefficient plays a key role in interpreting the findings. The smaller coefficient on the modified dispersion measure may more accurately reflect the true magnitude of the association in these cases. Finally, the attenuation effect, when present, may lead researchers to not reject the null hypothesis of no relationship between dispersion and the variable of interest; these researchers can perform more powerful tests by using the modified dispersion measure because it would more likely capture the true relationship if that exists.

VI. SUMMARY AND CONCLUSIONS

We analyze the cross-sectional determinants of forecast dispersion and provide large-sample evidence that in addition to being significantly associated with proxies for fundamental risk, uncertainty, and divergence of beliefs, forecast dispersion also suffers from a downward bias. We interpret our evidence as suggesting that observed dispersion is affected by analysts' strategic behavior and, on average, understates the dispersion of unmanaged forecasts.

We also provide a method to quantify the downward bias at the firm-quarter level and find that, on average, the observed dispersion understates the dispersion of unmanaged forecasts by 53.4 percent. There is also considerable cross-sectional variation in the effect of strategic behavior on forecast dispersion with important implications for studies that employ dispersion-based proxies of analysts' information environments. Specifically, we show that the relative magnitude of private information quality is significantly understated relative to public information quality, providing potentially incorrect inferences concerning analysts'' information-gathering activities around information events such as earnings announcements.

The estimation procedure that we suggest has been in use in the production economics literature for more than three decades. The availability of Maximum Likelihood (ML) estimation routines in almost all statistical packages facilitates implementation of the estimation procedure in large samples of financial data as we have illustrated. While it is impossible to observe and document analysts' actual beliefs and the associated dispersion of these beliefs, we argue that our modified measure of dispersion is a more precise measure of the underlying dispersion of analysts' actual beliefs. The various empirical demonstrations that we provide lend further credibility to this belief.

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[25] For example, Güntay and Hackbarth (2010) examine the association between corporate bond credit spreads and forecast dispersion by developing two competing hypotheses. The first hypothesis views dispersion as a measure of divergence of opinion and argues that higher forecast dispersion leads to higher firm values and, hence, lower credit spreads. The second hypothesis contends that forecast dispersion proxies for future cash flow uncertainty and, therefore, higher forecast dispersion leads to higher credit spreads. Güntay and Hackbarth (2010) find support for the second hypothesis.

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APPENDIX A

Demonstration of Dispersion Bias

Suppose there are three analysts A, B, and C, following a specific firm. Let F_A, F_B, and F_C, respectively, be their unmanaged forecasts. Assume $F_A < F_B < F_C$. Assume that one of these three analysts has more expertise than the others and is perceived by the other two (and herself) as the "leader." The leader releases her forecast first and one or both of the other analysts simply copy the leader even if the leader's forecast is inconsistent with their information. The following table shows 9 different scenarios for the above setup, and illustrates how this type of analyst behavior influences forecast dispersion and mean forecast.

Follower	Leader	Dispersion	Mean Forecast
A	B	↓	↑
A	C	↓	↑
B	A	↑	↓
B	C	↑	↑
C	A	↓	↓
C	B	↓	↓
AB	C	↓	↑
BC	A	↓	↓
AC	B	↓	ambiguous

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CEO Pay, Managerial Power, and SFAS 123(R)

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ABSTRACT: This study presents evidence that option expensing, whether voluntary under SFAS 123 or mandatory under SFAS 123(R), is associated with changes in CEO compensation that are beneficial to shareholders. I find that the reporting benefits of aggregate-employee option grants under SFAS 123 are associated with the change in the mix of CEO option grants and stock grants that occurs after SFAS 123(R), suggesting that reporting benefits of option grants may have influenced some CEO compensation decisions. Additionally, I find that the reduction in CEO pay after SFAS 123(R) is greater when shareholders have more power to act in their own best interest and when there is evidence of pre-expensing CEO compensation rents. The findings suggest that SFAS 123 may have encouraged inefficient, CEO-preferential pay practices and that SFAS 123(R) may have contributed to a reduction in CEO compensation inefficiencies.

Keywords: *stock option grants; CEO; compensation; management entrenchment; pay-for-luck; SFAS 123(R).*

Data Availability: *All data are publicly available.*

I. INTRODUCTION

For annual reporting periods beginning after June 15, 2005, SFAS 123(R) (FASB 2004) requires firms to recognize employee stock option grants as compensation expense and prohibits the alternative practice of footnote-only disclosure of *pro forma* net income effects that SFAS 123 had previously permitted. Consistent with prior research, this study shows that the switch to income statement recognition is associated with changes in all components of CEO pay. Whether those changes in CEO compensation are improvements for shareholders remains an open question. I provide some evidence that they are. Using a sample of voluntary and involuntary option

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expensing firms, I find that the changes in CEO compensation after the implementation of SFAS 123(R) are consistent with a reduction in inefficient contracting and rent extraction.[1]

Relying on the perceived cost hypothesis (Murphy 2002), this study first examines whether a measure of the "reporting benefits" of employee option grants under SFAS 123 is associated with changes in CEO compensation that occur after the adoption of option expensing. I define reporting benefits as the incremental return on equity due to the compensation expense that a firm avoided from option grants to all employees as a group *vis-à-vis* alternative forms of compensation that would have been expensed under SFAS 123. Reporting benefits is a proxy for the importance of financial reporting considerations in compensation decisions under SFAS 123. If financial reporting considerations did not influence CEO compensation decisions under SFAS 123, then I would expect reporting benefits of option grants in the pre-expensing period to be independent of the changes in CEO pay components that occur after firms begin to expense options.

In the period before options were expensed, I find that there is a higher rate of growth in CEO option grants and lower rate of growth in restricted stock grants when the reporting benefits of option grants is larger, consistent with Carter et al. (2007), who find an increasing preference for option grants over stock grants as reporting concerns increase. I also find that the rate of growth in CEO option grants decreases and that the rate of growth in restricted stock grants increases after the implementation of SFAS 123(R), consistent with the perceived cost hypothesis and several prior studies. Further, I find that the changes in both growth rates are more pronounced when the reporting benefits of option grants under SFAS 123 is larger. It is unlikely that the association between reporting benefits and CEO option and stock grants is mechanical because reporting benefits is based on option grants to all employees as a group and not just grants to the CEO. The findings suggest that, if reporting considerations were sufficiently important, some firms may have relied more on CEO option compensation and less on CEO restricted stock compensation under SFAS 123 than if both forms of compensation were expensed. The results also imply that some firms may have sacrificed CEO incentive considerations in exchange for the financial statement reporting benefits of CEO option grants under SFAS 123 and, further, that those compensation inefficiencies diminished after SFAS 123(R) eliminated the financial reporting advantage of option compensation.

A second set of tests relies on the theory of managerial power and examines whether the reduction in CEO compensation that occurs after the implementation of SFAS 123(R) is consistent with a curtailment of CEO compensation rents. As a proxy for managerial power, I use a measure of management entrenchment (Bebchuk et al. 2009). Entrenched managers are difficult to remove from office and can influence the pay process to their advantage (Bebchuk et al. 2009; Bebchuk and Fried 2003). Under managerial power theory, entrenched CEOs are likely to have avoided some or all of the reduction in CEO compensation rents that may have occurred after firms began to expense options. Stated alternatively, if SFAS 123(R) increased reporting transparency and revealed CEO compensation rents that were previously obscured by SFAS 123, the reduction in CEO compensation that occurs after the implementation of SFAS 123(R) should be more pronounced, *ceteris paribus*, when shareholders have more power to remove the CEO and to reshape CEO compensation in the shareholders' interest. Under the premise that shareholders are rational and the CEO labor market is competitive, shareholders would not reduce CEO compensation unless they believe that CEO compensation rents exist for fear of losing the CEO to another firm. I find that the post-expensing reductions in the rates of growth in option compensation, equity compensation, and total compensation are larger for less entrenched CEOs who, compared to their more entrenched counterparts, are less able to resist changes in compensation that are beneficial to shareholders but not the CEO. The findings are consistent with a reduction in CEO compensation rents after the implementation of option expensing.

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[1] I use "SFAS 123(R)" to refer to option expensing and "SFAS 123" to refer to footnote-only disclosure of *pro forma* net income effects of implied option compensation, for both voluntary and involuntary expensing firms.

In a final series of tests, I use the sensitivity of CEO pay to strong sector performance in the SFAS 123 period as a proxy for rent extraction and examine how that proxy is associated with CEO compensation under SFAS 123 and with changes in CEO compensation following the implementation of SFAS 123(R). Following Albuquerque (2009), I use industry-size peer group stock returns to measure sector performance. When firms link CEO compensation to good *sector* performance, CEOs are in effect rewarded for good luck because sector performance represents the contribution that exogenous shocks made to firm performance rather than the contribution that the CEO made to firm performance. Rewarding CEOs for good sector performance will provide them with excess compensation (Bertrand and Mullainathan 2001) unless there are offsetting cuts to CEO compensation elsewhere. Agency theory would predict that firms would reward a CEO for performance relative to that of other firms in the same sector and, hence, not reward the CEO simply for strong sector performance.

I first estimate separately for each firm a measure of "pay for good luck" during the SFAS 123 period. Pay for good luck is equal to the sensitivity of CEO compensation to good sector performance relative to the sensitivity of CEO pay to firm performance net of sector performance. Pay for good luck is positive for more than 50 percent of the firms in my sample, indicating that, for most firms, CEO pay was more sensitive to good sector performance (i.e., good luck) under SFAS 123 than to firm performance net of sector performance (i.e., the CEO's contribution). I find that CEOs with above-average pay for good luck experience higher rates of growth in compensation in the pre-expensing period for the pay component of cash, options, stock, and total compensation compared to other CEOs. This result is expected if pay for good luck is associated with excessive CEO pay. Additionally, those same CEOs experience greater reductions in the rates of growth in cash, option, and total compensation and a smaller increase in the rate of growth in restricted stock compensation compared to other CEOs after their firms begin to expense options. The results are consistent with a reduction in compensation rents after the implementation of SFAS 123(R). In summary, the findings in this study suggest that the changes in CEO compensation that occurred after the implementation of SFAS 123(R) are beneficial to shareholders.

II. THIS STUDY'S CONTRIBUTION TO EXISTING RESEARCH

For a sample of firms that were among the first to voluntarily expense option grants beginning in 2002 or 2003, Carter et al. (2007) find a larger decrease in option grants and a larger increase in restricted stock grants after they begin to expense options compared to firms that did not expense options, consistent with my findings in this respect. However, the authors find no difference in the change in total CEO compensation between the two groups of firms and conclude that "firms find it difficult to downsize the large executive pay packages that resulted from the favorable accounting treatment for stock options" (Carter et al. 2007, 329). In contrast, using a model that controls for calendar-year trend effects and other factors expected to affect CEO compensation, I do find a statistically significant reduction in the rate of growth in total CEO pay after firms began to expense option grants for a sample of firms that generally were late to expense options, some voluntarily under SFAS 123 and most involuntarily under SFAS 123(R). The difference between my findings and Carter et al. (2007) may reflect the increasing public criticism of lofty CEO pay levels, systematic differences between voluntary and involuntary expensing firms, or both.

Previous studies examine how financial reporting concerns are associated with option compensation *prior to* option expensing (Matsunaga 1995; Core and Guay 1999; Carter et al. 2007) and how financial reporting concerns and corporate governance are associated with changes in executive compensation in *anticipation of* option expensing (Brown and Lee 2007). For the decade between 1979 and 1989, Matsunaga (1995) shows that option grants are greater when income is below a target income level than when it is above the target level and also when a firm uses a relatively high proportion of income-increasing accounting methods. Carter et al. (2007) show that firms with reporting concerns and with plans to access capital markets from 1995 to 2001 are more likely to grant options to CEOs. Brown and Lee (2007) find that the proportion of compensation paid in stock options to the top five executives decreased in 2005 in anticipation of SFAS 123(R) and that the reduction in option grants is larger for firms with financial reporting concerns and weak governance.

Building on previous studies that document that the reporting advantage of option grants influenced compensation decisions, this paper makes several contributions to the literature. First, my analysis considers all CEO pay components, individually and in the aggregate, which is vital because firms should adjust pay components jointly rather than individually. Previous research has focused primarily, although not exclusively, on option and restricted stock grants. Second, my sample includes voluntary and involuntary expensing firms. In contrast, Carter et al. (2007) consider only voluntary expensing firms, and Brown and Lee (2007) exclude early voluntary expensing firms. Third, my research design captures the full effect of option expensing on CEO pay, including changes from the year immediately prior to option expensing through the first two implementation years. By comparison, the changes in compensation in Brown and Lee (2007) include only changes in anticipation of option expensing. Fourth, I examine how management entrenchment and pay for good luck are associated with changes in CEO compensation around the change in accounting standards for stock options. The results for entrenchment and pay for good luck, which the option expensing literature has not examined previously, provide strong support for the assertion that SFAS 123(R) reduces CEO compensation rents.

This study also adds to what we know from Brown and Lee (2010), who find that CEO option grants are lower in the post-Enron period (2002–2006) than in the pre-Enron period (1998–2001). As noted by Brown and Lee (2010), the post-Enron period saw significant regulatory changes, a renewed public focus on executive compensation, and the issuance of SFAS 123(R), all of which are likely to have influenced the post-Enron changes in CEO compensation that they document. By comparison, more than 80 percent of my sample first expensed options in fiscal year 2006, the last possible period for option expensing. As a result, the benchmark compensation for the majority of my sample in fiscal years 2003 and 2004 already reflects Enron effects. Moreover, inferences in this paper are unchanged when I restrict the sample to firms that expensed involuntarily (not tabulated). Thus, the changes in CEO compensation documented in this study appear to be reasonably interpreted as primarily the effect of option expensing *per se*, i.e., as changes in CEO pay practices that occur after the implementation of SFAS 123(R) and that are incremental to the changes in CEO compensation that occurred after other regulatory changes and changes in public sentiment about CEO pay levels.

III. HYPOTHESES

Reporting Benefits of Option Grants and CEO Compensation

The perceived cost hypothesis (Murphy 2002) predicts that if option grants are not charged against reported earnings, then some companies will favor option grants over other forms of compensation. Hall and Murphy (2003) argue that the absence of a reported expense for stock option grants under SFAS 123 (and earlier under APB 25) helped fuel the growth in option compensation during the 1990s and early 2000s. Studies that are consistent with the perceived cost hypothesis include those that document larger option grants in the presence of financial reporting concerns under SFAS 123 (Brown and Lee 2007; Carter et al. 2007; Matsunaga 1995) and those that conclude that some firms accelerated the vesting date of previously issued employee stock options to avoid option-related compensation expense that otherwise would have been recognized under SFAS 123(R) (Balsam et al. 2008; Choudhary et al. 2009).

Several studies conclude that option grants were undervalued under SFAS 123, thus disguising the actual amount of employee compensation. Yermack (1998) estimates that *Fortune* 500 firms undervalued CEO options by an average of 8.9 percent. Aboody et al. (2006) and Bartov et al. (2007) find that firms used their discretion over option-pricing model inputs to understate option values. Collectively, these studies conclude that the understatement of option values is greater when CEO compensation is excessive, when implied option compensation expense is higher, and when there are incentives to understate compensation. Similarly, Balsam et al. (2008) find that accelerated vesting in advance of SFAS 123(R) is more likely when the avoided expense is higher. Hodder et al. (2006, 967) conclude that firms that understate option values "have incentives to manage earnings and to disguise the size and value of compensation packages."

In summary, prior research suggests that some firms may have believed they could disguise the economic cost of employee compensation by favoring option grants over other forms of compensation under SFAS 123, by accelerating option vesting dates, and by understating the value of option compensation. The latter practice is consistent with Libby et al. (2006) who find that audit partners may be more willing to tolerate misstatements of option compensation when it is disclosed in the footnotes rather than recognized in the financial statements.

From the perspective of reported earnings, an option grant had no marginal cost under SFAS 123. Thus, from a strict earnings perspective, an option grant could be justified even if no favorable incentive effects were expected. As concerns about the level of reported income became more important, a firm had an increasing incentive to substitute option grants for other forms of compensation. By contrast, under SFAS 123(R), reported earnings reflect the incentive effects of option grants and also their cost. Accordingly, earnings-focused compensation committees would be expected to reconsider their compensation practices after SFAS 123(R).

This study uses implied, aggregate-employee option compensation expense in the pre-expensing period, scaled by market value of equity (Aboody et al. 2004), as a proxy for the importance of reported income effects for compensation decisions in the pre-expensing period. Prior research suggests that the reporting benefits of option grants under SFAS 123, as measured in this study, correlates with an assortment of corporate governance deficiencies and earnings management incentives (debt covenant constraints, earnings shortfalls, capital market access, etc.). Thus, my measure of reporting benefits parsimoniously encapsulates the net effect of the opportunities and incentives to manage reported earnings and to understate compensation expense through option grants under SFAS 123.

Building on the perceived cost hypothesis, this study's first hypothesis predicts that firms with larger reporting benefits under SFAS 123 will have a greater decrease in the rate of growth in CEO option grants and a greater increase in the rate of growth in CEO stock grants after SFAS 123(R) than firms with smaller reporting benefits. This prediction is based on prior evidence that firms with reporting concerns preferred option grants to restricted stock grants when option grants were not expensed. The first hypothesis also predicts that firms with larger reporting benefits under SFAS 123 will have larger post-expensing reductions in the rate of growth in total CEO pay than firms with smaller reporting benefits. This second prediction is based on prior evidence that the absence of financial statement recognition of option compensation may have encouraged excessive CEO compensation and, further, that any CEO pay component can be responsible for excessive compensation. For example, firms could have designed option grants to the CEO optimally under SFAS 123 and chosen to pay excess compensation in the form of salary and bonus. The absence of compensation expense for option compensation, especially in conjunction with understatement of the implied option compensation expense in SFAS 123 disclosures, could have partially or fully concealed excess compensation. It is unlikely that any association between reporting benefits of option grants under SFAS 123 and changes in CEO compensation after the implementation of SFAS 123(R) is a simple mechanical effect because reporting benefits of options grants is based on option grants to all employees and not option grants to the CEO alone.

H1: The greater a firm's reporting benefits from aggregate-employee option grants under SFAS 123, the greater will be the firm's reduction in the rate of growth in CEO option pay and CEO total pay and the greater will be the firm's increase in rate of growth in CEO restricted stock compensation after the adoption of SFAS 123(R).

Managerial Power and CEO Compensation

Bebchuk and Fried (2003) argue that powerful managers can often influence the compensation process to their advantage. I use the construct "CEO entrenchment" and the related E-index (Bebchuk et al. 2009) to capture managerial power. The E-index is a function of six corporate governance provisions that limit shareholder power to replace management: staggered boards with overlapping terms, shareholder limits on amending corporate bylaws, shareholder limits on amending corporate charters, supermajority approval of mergers, golden parachutes, and poison pills. Bebchuk et al. (2009) show that firms with higher values of the E-index, which indicates more deeply entrenched managers, are more likely to have significantly lower future abnormal returns than firms with less entrenched managers.

I argue that more entrenched CEOs are more likely than other CEOs to successfully lobby their compensation committees for excess compensation. Entrenched managers are difficult to remove from office. Thus, they have a relatively low likelihood of forfeiting any compensation rents and a correspondingly high expected return on lobbying efforts. By not requiring the cost of options to be expensed, SFAS 123 is also very likely to have facilitated executive lobbying for greater option compensation to conceal rent extraction (Bebchuk and Fried 2004; Yermack 1998; Aboody et al. 2006; Hodder et al. 2006; Bartov et al. 2007).

The second hypothesis makes two predictions that, if confirmed, are consistent with rent extraction under SFAS 123 and with subsequent improvements in compensation efficiency under SFAS 123(R). First, in the pre-expensing period, I expect more entrenched CEOs to experience higher rates of growth in both option compensation and total compensation than less entrenched CEOs. Second, because more entrenched CEOs face fewer consequences from the increase in transparency that accompanies the adoption of option expensing, I expect more entrenched CEOs to experience smaller reductions in rates of growth in both option compensation and total compensation than less entrenched CEOs after the implementation of SFAS 123(R).

H2: More entrenched CEOs will have higher rates of growth in option and total compensation in the pre-expensing period and smaller reductions in rates of growth in option and total compensation after the adoption of SFAS 123(R) than less entrenched CEOs.

Sensitivity to Sector Performance and CEO Compensation

When an agent's effort level is not observable and when peer group or sector performance is correlated with firm performance, a second-best compensation contract will rely on observable outcome measures and some form of relative performance evaluation (Holmström 1982). Relative performance evaluation removes the effect of exogenous factors (i.e., good and bad luck) from the observable performance measure and provides a residual measure of performance that captures an agent's contribution (Lambert 2001). For example, when firm performance is adjusted for sector performance, the portion of firm earnings related to sector-wide losses (gains) is added to (deducted from) firm-level earnings. Therefore, relative performance evaluation adjusts CEO performance measures to remove the effect of factors beyond the control of the CEO, shields CEO compensation from the effects of sector gains and losses, and tightens the link between compensation and a CEO's contribution to firm performance. Based on proxy statement disclosures required since 2006, Gong et al. (2011) find that 27 percent of S&P 1500 firms explicitly acknowledge that they use relative performance evaluation in executive compensation. In practice, it is unlikely that firms can identify performance measures that are completely free of the effects of exogenous factors. Nevertheless, relative performance evaluation should mitigate the effect that exogenous factors (luck) would otherwise have on CEO compensation.

Bertrand and Mullainathan (2001) conclude that CEOs are rewarded as much for luck as for skill. Likewise, and consistent with CEO pay for luck, Albuquerque (2009) reports that several broad-based archival studies find that CEO compensation is based on, rather than shielded from, the effects of sector performance. Garvey and Milbourn (2006) argue, however, that a manager has relatively little incentive to seek *symmetric* rewards for luck. Instead, a manager will benefit from a reward structure that pays the manager more when the firm experiences good luck and yet shields the manager's compensation from the effects of bad luck. Asymmetric pay for luck leads to higher expected compensation with less downside risk for the manager. Garvey and Milbourn (2006) report evidence that penalties for bad luck are significantly smaller than rewards for good luck, consistent with pay-for-luck asymmetry that favors the CEO.

Albuquerque (2009) argues that previously documented pay for luck may reflect poorly chosen peer-group indices rather than an absence of relative performance evaluation. Using cross-sectional regression models with peer-group performance benchmarks based on a firm's industry classification and size quartile, Albuquerque (2009) finds evidence that CEO compensation is shielded from the effects of luck, at least partially, consistent with relative performance evaluation. Similarly, I also find support for relative performance evaluation for my sample in cross-sectional analyses with industry-size market-return benchmarks (details available upon request). The use of industry-size performance benchmarks, rather than the typical industry-alone benchmarks, is based on the observation that larger firms have more strategic flexibility, fewer financial constraints, greater diversification (Albuquerque 2009), and more talented CEOs (Gabaix and Landier 2008). In short, industry-size performance benchmarks parsimoniously reflect factors that are relevant to CEO performance benchmarking (Goplan et al. 2009; Rajgopal et al. 2006; Oyer 2004) but that are not reflected in performance benchmarks based on industry classification alone.

As described in detail later, I use the results of firm-specific time-series compensation regressions to sort firms by the sensitivity of CEO pay to good sector performance under SFAS 123. Following Albuquerque (2009), I assume that industry-size market returns are appropriate relative performance evaluation benchmarks and thus use those benchmarks as measures of good (and bad) sector performance in my firm-specific regression models. I find significant variation in the sensitivity of CEO pay to good sector performance (good luck) across the sample, with some firms rewarding CEOs for good luck and others shielding, partially or completely, CEO compensation from the effects of good luck. Although evidence from cross-sectional regression models with industry-size benchmarks suggests that CEO compensation is shielded from the effects of good and bad luck *on average* (Albuquerque 2009), that evidence does not preclude the significant variation in pay for luck that is indicated by the results of my firm-specific models.

My third hypothesis examines whether compensation rents were present under SFAS 123 and whether those rents are smaller under SFAS 123(R). I use the sensitivity of CEO pay to good luck under SFAS 123 as a proxy for potential rent extraction. Good and bad luck are defined, respectively, as positive and negative industry-level stock returns for a firm's industry classification and size quartile. Luck represents that portion of a firm's stock return that is due to systematic factors beyond the control of the CEO. The sensitivity of CEO pay to luck in my model in Section IV is negative when a firm uses relative performance evaluation to at least partially remove the contribution that luck made to firm performance, and greater than or equal to zero when a firm compensates luck as much as or more than a CEO's individual contribution to firm performance. As the sensitivity of CEO pay to good luck increases, *ceteris paribus*, CEO pay increases and, after some point, the CEO pay structure becomes inconsistent with relative performance evaluation. As described fully in Section IV, I use the sensitivity of CEO pay to good luck under SFAS 123 to classify CEOs by potential rent extraction.

While both pay for good luck and pay for bad luck are inefficient pay practices, I do not use the sensitivity of CEO pay to bad luck to classify CEOs by potential rent extraction. When sector performance is negative, a pay structure that is *consistent* with relative performance evaluation will result in higher CEO compensation than a pay structure that is *inconsistent* with relative performance evaluation, *ceteris paribus*. Relative performance evaluation shields CEO pay from the effects of luck and, *ceteris paribus*, will result in higher CEO pay when an industry underperforms than a compensation structure that penalizes bad luck. However, this higher pay is not a form of rent extraction, but is, instead, simply a consequence of efficient contracting when sector performance is negative. In contrast, when sector performance is positive, a compensation structure that rewards good luck will result in higher CEO pay than a compensation structure that shields CEO pay from the effects of good luck through relative performance evaluation. In summary, the pay-premium that can occur when CEOs are rewarded for good luck suggests *inefficient* pay practices and rent extraction. However, no pay-premium is expected when CEOs are "rewarded" for bad luck. Instead, the pay-premium, if any, is expected when CEO pay is shielded from the effect of bad luck and thus suggestive of *efficient* pay practices but not rent extraction. Thus, I do not use the sensitivity of CEO pay to negative sector returns to classify CEOs by potential rent extraction.

I acknowledge that my measure of pay-for-luck sensitivity has limitations as an index of contracting efficiency and rent extraction. First, industry-size returns may be a poor measure of exogenous factors compared to benchmarks that are tailor-made for a firm. Second, there may be instances where the principal would find it beneficial to tie CEO compensation to exogenous factors (Lambert 2001; Oyer 2004). Third, as a practical matter, it is unlikely that the principal can ever completely remove the effect of exogenous factors from performance measures. Thus, a second-best compensation contract will expose the agent to some good and bad luck as a necessary consequence of relying on contractible performance measures.

The third hypothesis predicts that the rate of growth in CEO compensation in the pre-expensing period is higher when CEO pay is more sensitive to good luck. Additionally, if SFAS 123(R) makes compensation rents more visible, then the third hypothesis predicts that the reduction in the rate of growth in CEO compensation following the adoption of SFAS 123(R) is greater for those CEOs whose pay is more sensitive to good luck under SFAS 123.[2]

> **H3:** Firms with CEOs whose compensation is highly sensitive to good luck in the pre-expensing period will have higher rates of growth in CEO compensation in that period and greater reductions in the rates of growth in compensation after the adoption of SFAS 123(R) compared to firms with CEOs whose compensation is less sensitive to good luck under SFAS 123.

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[2] There is a plausible alternative prediction. Favorable CEO pay for luck under SFAS 123 might, in part, result from entrenched managers who, as argued in H2, are likely to limit the consequences of the increased transparency that results from SFAS 123(R). If pay for luck in the pre-expensing period is considered a proxy for managerial power that results in rent extraction, then H3 becomes a test of the relative influence of managerial power and reporting transparency. H3 is stated under the assumption that reporting transparency will have the dominant influence.

IV. RESEARCH DESIGN

Preliminary Tests

H1 through H3 assume that SFAS 123(R) affects pay levels and pay mix. The following model is therefore estimated as a preliminary step for each component of CEO compensation:

$$\ln C_{it} = \alpha_0 + \alpha_1 TREND_{ij} + \alpha_2 FAS_{it} + \beta_1 R_{it} + \beta_2 DIV_{it} + \beta_3 CFLOW_{it} + \beta_4 BM_{it-1} + \beta_5 \ln SALES_{it-1} + \beta_6 IR_{it-1} + \beta_7 \ln C_{it-1} + \varepsilon_{it}, \quad (1)$$

where $t = 0$ is the first year a firm used option expensing; $\ln C_{it}$ is the log of CEO compensation, with C representing any one of five CEO pay components (option grants, stock grants, equity grants, cash compensation, and total compensation); *TREND* is a variable that indexes by fiscal year j (calendar time) and thus controls for regulatory and other systematic factors that may have affected compensation; FAS_{it} is an indicator variable that equals 1 when $t \geq 0$, and 0 otherwise; R_{it} is the stock return for firm i in year t; *DIV* is a dividend constraint indicator; *CFLOW* is cash flow shortfall; *BM* is book-to-market; ln*SALES* is the log of sales; and *IR* is the incentive residual from the Core and Guay (1999) model. Appendix A provides formal definitions for all variables.

The control variables *DIV*, *CFLOW*, *BM*, ln*SALES*, and *IR* follow Core and Guay (1999). I use the same model for all pay components (including cash pay) to facilitate comparison. For equity grants, the coefficient on *BM* should be negative because lower growth firms are expected to make smaller equity grants; the coefficients on *CFLOW* and *DIV* should be positive because firms are expected to have a preference for equity compensation when they are cash or dividend constrained; and the coefficient on *IR* should be negative because equity grants in year t are expected to be higher (lower) when a CEO's equity incentives in year $t-1$ are below (above) their expected value (Core and Guay 1999). For all forms of compensation, the coefficient on ln*SALES* should be positive to reflect higher CEO pay in larger firms.

I expect the coefficient on firm performance (R) to be positive, consistent with pay for performance. The model includes lagged compensation ($\ln C_{it-1}$) to allow for "stickiness" in pay practices (Carter et al. 2007). With $\ln C_{it-1}$ in the model, the coefficient on FAS reflects the difference between the rate of change in compensation in the post-expensing period and the rate of change in the pre-expensing period, after controlling for the effect of other factors.[3]

Tests of Hypotheses 1 and 2

The first hypothesis (H1) predicts that, after the adoption of option expensing, the reduction in the growth in option pay, increase in the growth in stock grants, and reduction in the growth in total pay will be more pronounced for firms with greater reporting benefits from option grants in the pre-expensing period. The regression model is:

$$\ln C_{it} = \alpha_0 + \alpha_1 TREND_{ij} + \alpha_2 FAS_{it} + \alpha_3 OPTEXP_i + \alpha_4 (OPTEXP_i * FAS_{it}) + \beta_1 R_{it} + CONTROLS + \varepsilon_{it}, \quad (2)$$

where *OPTEXP* is an index that ranks firms by reporting benefits of option grants to all employees as a group during the pre-expensing period, *CONTROLS* refers to firm-specific control variables from Equation (1), and other variables are as previously defined.

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[3] For example, consider a case where compensation decreases by 5 percent per year in the pre-expensing period and 20 percent in the post-expensing period. Assuming that compensation is completely determined by the decay rates (5 percent and 20 percent) and using the model $\ln C_{it} = \alpha + \beta_1 \ln C_{it-1} + \beta_2 FAS_{it} + \varepsilon_i$, the coefficient $\alpha = -0.05129$, $\beta_1 = 1$, and $\beta_2 = -0.17185$. The decay rate in the pre-implementation period is equal to $\exp(-0.05129) - 1 = -0.05$; and, the change in decay rate is found as $\exp(-0.05129 - 0.17185) - \exp(-0.05129) = -0.15$.

For *OPTEXP*, I first find each firm's median IOE_{it} from $t \leq -2$, where IOE_{it} is equal to the implied option expense in year t for firm i scaled by market value of equity (Aboody et al. 2004). Next, I rank median IOEs from 0 to 1 to form *OPTEXP*.[4] Higher values of *OPTEXP* represent more favorable income effects (reporting benefits) from option grants *vis-à-vis* alternative forms of compensation in the pre-expensing period. As discussed above, H1 is conditioned on $\alpha_2 < 0$ for option grants and total pay, and $\alpha_2 > 0$ for restricted stock grants. H1 predicts that the effect of requiring options to be expensed will be increasing in the extent to which the firm relied on options for employee compensation prior to the expensing requirement, such that $\alpha_4 < 0$ for CEO option grants and total pay, and $\alpha_4 > 0$ for CEO stock grants.

The second hypothesis (H2) predicts that more entrenched CEOs will experience higher growth in option and total pay in the pre-expensing period and will experience a less severe post-expensing reduction in option and total pay than less entrenched CEOs. The model is:

$$\ln C_{it} = \alpha_0 + \alpha_1 TREND_{ij} + \alpha_2 FAS_{it} + \alpha_3 EINDEX_i + \alpha_4 (EINDEX_i * FAS_{it}) + \beta_1 R_{it} + CONTROLS + \varepsilon_{it}, \tag{3}$$

where $EINDEX_i$ is the median of firm i's entrenchment index (Bebchuk et al. 2009) from its pre-expensing period ($t \leq -2$), and other variables are as defined previously. *EINDEX* ranges from 0 to 6, with higher values indicating more deeply entrenched management. H2 predicts $\alpha_3 > 0$ and $\alpha_4 > 0$ for option grants and total pay.

Tests of Hypothesis 3

For the third hypothesis (H3), I use an index of pay for good luck as a measure of rent extraction. Using an adaptation of the procedure in Bertrand and Mullainathan (2001), I separately estimate for each firm the sensitivity of CEO pay to good and bad luck. Let R_{ij} equal the return for firm i in fiscal year j and R_j equal the average return for the matching industry-size portfolio. The industry-size benchmark return, R_j, represents that portion of firm stock return in year j due to sector performance or observable luck. $R_{ij} - R_j$ represents firm performance due to managerial skill and unobservable luck.

Next, I define three new variables: luck ($L = R_j$), good luck (*GL*), and bad luck (*BL*), where $GL = \max(0,L)$ and $BL = \min(0,L)$. I then estimate the following time-series equation for each firm:

$$\ln C_{ij} = \alpha_i + \beta_i R_{ij} + \gamma_i GL_{ij} + \tau_i BL_{ij} + \varepsilon_{ij}, \tag{4}$$

where $\ln C_{ij}$ is the natural log of compensation for firm i in fiscal year j. Under relative performance evaluation, the coefficients γ_i and τ_i should equal $-1 * \beta_i$, which can be seen by expanding Equation (4), with $R_{ij} = S_{ij} + GL_{ij} + BL_{ij}$, where S_{ij} is the performance due to skill (and unobservable luck). In other words, the effect of observable luck should be completely removed from performance measures, consistent with relative performance evaluation. In the presence of pay for good luck, by contrast, γ_i will exceed $-1 * \beta_i$. When $\gamma_i = 0$, good luck is rewarded as much as skill. Notice that pay for good luck (γ_i) is measured relative to each firm's pay for skill (β_i).

I use Equation (4) to find good luck pay sensitivity, γ_i, for each pay component separately, based on time-series data for the period prior to adoption of option expensing. I exclude a CEO's compensation in the CEO's first year in the position because pay that year is atypical (Rajgopal et al. 2012) and usually for less than a full year. Because option, stock, and equity compensation are censored at zero, I use a Tobit formulation for these pay components. Cash and total compensation regressions are based on OLS.

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[4] As an illustration, Magnetek, Inc. has nine years of pre-expensing data for this calculation, starting in fiscal 1996 ($t = -10$) and ending with fiscal 2004 ($t = -2$). For each year, I calculate IOE_{it} (Compustat's implied option expense [$XINTOPT_{it}$] scaled by market value of equity [$CSHO_{it} \times PRCC\_F_{it}$]). For Magnetek, IOE ranges from a low of 0.0019 to a high of 0.0398, with a median value over the nine years of 0.0103. I repeat this process for each of the 1,048 firms in the restricted sample from Table 1. For Magnetek, *OPTEXP* is 0.8885. This means that, in my sample, Magnetek's median pre-expensing IOE stands at the 89th percentile.

H3 predicts that higher pay for good luck (γ_i) under SFAS 123 will be associated with higher growth in CEO compensation in the pre-expensing period and a larger reduction in the growth in CEO compensation after SFAS 123(R). The model is:

$$\ln C_{it} = \alpha_0 + \alpha_1 TREND_{ij} + \alpha_2 FAS_{it} + \alpha_3 PGL_i + \alpha_4 (PGL_i * FAS_{it}) + \beta_1 R_{it} + CONTROLS + \varepsilon_{it}, \tag{5}$$

where PGL_i is an indicator variable equal to 1 when γ_i is greater than the sample median, and 0 otherwise. Hence, *PGL* is 1 when CEO pay is abnormally sensitive to good luck relative to its sensitivity to CEO skill, consistent with a compensation policy that is comparatively CEO-friendly. H3 predicts that $\alpha_3 > 0$, consistent with CEO compensation rents in the pre-expensing period when pay is unusually sensitive to good luck under SFAS 123. H3 further predicts that $\alpha_4 < 0$, consistent with a larger reduction in the growth in CEO compensation after SFAS 123(R) for CEO's with compensation rents under SFAS 123.[5]

V. SAMPLE SELECTION AND DESCRIPTIVE STATISTICS

Sample Selection

Table 1 reports the frequency of the intrinsic value method of accounting for option grants (i.e., no expensing) versus the grant-date fair value method (i.e., expensing) over calendar time from 1998 to 2007 and over event time from $t = -4$ to $t = +4$, where $t = 0$ is the first year a company expensed option grants. Any firm adopting the grant-date fair value method prior to fiscal 2006 did so voluntarily. For voluntary adopters, I define the adoption date as the earlier of the adoption date reported by McConnell et al. (2004) or the Compustat adoption date. (See Appendix A for additional discussion.)

Table 1 reports frequency data for two sets of firms. In the first six columns of Table 1, frequency counts are restricted to firm-years with asset (Compustat), compensation (ExecuComp), and market return data (CRSP), and at least five other firms in their industry-size portfolio with market return data. The last six columns further restrict the sample to firms with data required for regression Equation (1) for the five years from $t = -3$ to $t = +1$. Comparing the two samples illustrates the reduction in sample size due to primary data requirements. For example, at $t = 0$, there are 1,527 firms in the initial sample and 1,048 in the sample of firms with five years of continuous data.

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[5] In robustness tests, I include in my regression models the variable *PBL* (pay for bad luck), which is equal to 1 when the sensitivity of CEO pay to bad luck, τ_i, is *less than* the sample median, and 0 otherwise. As with good luck sensitivity, the sensitivity of CEO pay to bad luck, τ_i, is negative when a firm uses relative performance evaluation to at least partially remove the contribution that bad luck made to firm performance, and greater than or equal to 0 when a firm "rewards" bad luck as much as or more than a CEO's contribution to firm performance. Thus, *PBL* is 1 when a CEO's pay is shielded from the effects of bad luck more than average and thus not only comparatively CEO-friendly, but also *consistent* with relative performance evaluation. As discussed earlier, τ_i is not a useful way to capture differences between firms with respect to compensation rents because, *ceteris paribus*, CEO pay will be higher when pay is shielded from the effect of bad luck even though that pay practice is *consistent* with relative performance evaluation. By contrast, a CEO-friendly compensation policy with respect to good luck (where γ_i is *greater than* the sample median and $PGL = 1$) is *inconsistent* with agency theory. When I include *PBL* in the regression model, inferences are unchanged; *PBL* is significant only for cash compensation.

TABLE 1

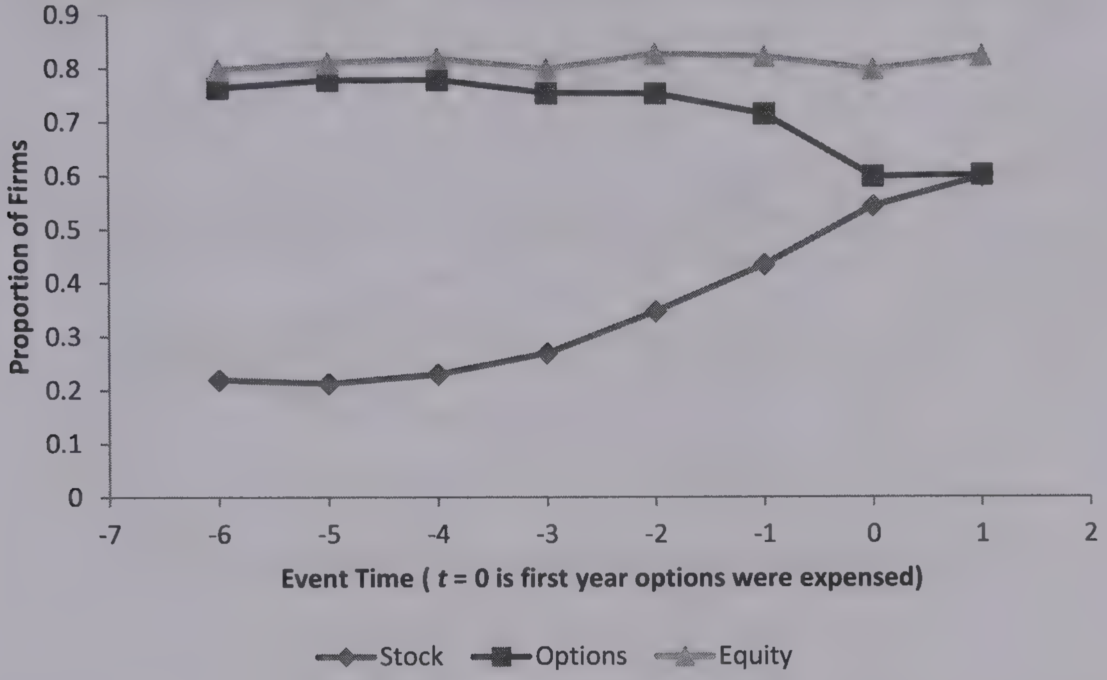
Sample Distribution

The Frequency of the Intrinsic Value Method (No Expensing) and the Grant-Date Fair Value Method (Expensing) of Accounting for Option Grants by ExecuComp Firms Classified by Fiscal Year and Event Time

Firms with Return, Assets, and Compensation Data						Firms with Data for Equation (1) over $t = <-3,+1>$					
Fiscal Year (j)	Intrinsic Value Method	Grant-Date FV Method	Event Time (t)	Intrinsic Value Method	Grant-Date FV Method	Fiscal Year (j)	Intrinsic Value Method	Grant-Date FV Method	Event Time (t)	Intrinsic Value Method	Grant-Date FV Method
1998	1038	3	−4	1288		1998	951	1	−4	1047	
1999	1152	3	−3	1358		1999	987	1	−3	1048	
2000	1268	4	−2	1411		2000	1012	1	−2	1048	
2001	1278	5	−1	1502		2001	1033	1	−1	1048	
2002	1291	30	0		1527	2002	1034	12	0		1048
2003	1193	196	1		1459	2003	926	122	1		1048
2004	1216	223	2		483	2004	910	138	2		329
2005	1233	287	3		227	2005	860	182	3		130
2006		1529	4		190	2006		1034	4		106
2007		1447				2007		1028			

Table 1 is restricted to ExecuComp firms with Compustat and CRSP coverage. The "Intrinsic Value Method" column reports the number of firms using the intrinsic value method of accounting for option grants (no expense) and the "Grant-Date FV Method" column reports the number of firms using grant-date fair value method of accounting for option grants (expensing).The first six columns are limited to any firm with data for market return, assets, compensation, and at least five other firms in their industry-size peer group in the indicated year. A firm's peer group is defined as all firms in the same industry (four-digit GIC code) and in the same market size quartile within the industry. The last six columns are further limited to firms with all data needed for Equation (1) over the five years $t = -3$ to $t = +1$. For each sample, the first three columns report the number of firms with data available in each fiscal year, classifying firms by whether they used the intrinsic value method or the grant-date fair value method in fiscal year j, and the next three columns report the number of firms with data available in each year over event time t, where $t = 0$ is the first year a firm expensed option grants.

FIGURE 1
Proportion of Firms Making Stock, Option, or Equity Grants to Their CEOs



Descriptive Statistics

Figure 1 plots the proportion of firms making option, stock, and equity grants to their CEOs over $t = -6$ to $t = +1$. In Figure 1, the proportion of firms granting options declines and the proportion granting restricted stock increases beginning from year $t = -5$ to year $t = -4$. The proportion of firms granting one or both forms of equity is almost flat at about 80 percent.

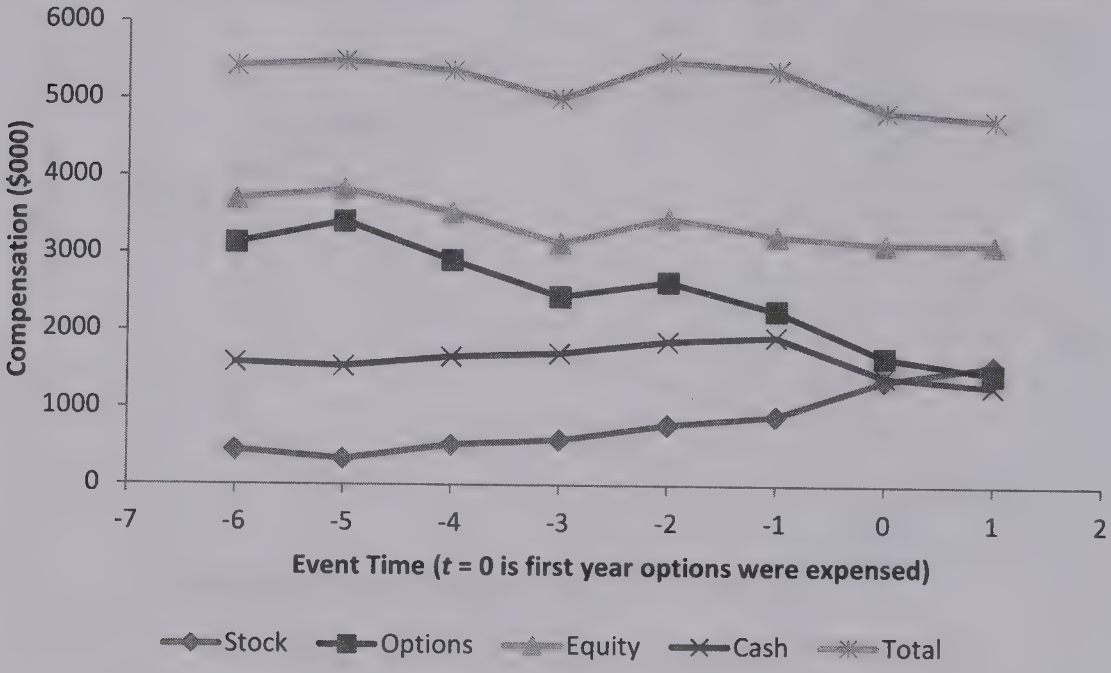
Figure 2 plots mean CEO compensation in thousands of dollars for each of five CEO pay components (stock grants, option grants, equity grants, cash compensation, and total compensation) over the same period, with each component adjusted to constant 2007 dollars based on the Consumer Price Index. Figure 2 shows similarly that mean option grants decline and mean restricted stock grants increase from year $t = -5$ to year $t = -4$. The average total equity grant is relatively constant over time, particularly after $t = -3$. The peak year for option grants in dollars and in proportion of total compensation is $t = -5$. For most of the sample, $t = -5$ is calendar year 2000, which is five years before SFAS 123(R) became mandatory, shortly after the top of the technology stock bubble, and shortly before several major accounting scandals. Both graphs are based on the initial sample of firms from Table 1. The graphs are very similar for the smaller sample.

Panel A of Table 2 provides descriptive statistics for the restricted sample from Table 1. The left side of Panel A reports separate columns for the pre- ($t = < -3,-2 >$) and post- ($t = < 0,+1 >$) expensing periods. These sample statistics are based on 4,192 firm-years (n = 1,048 firms × 4 years), split evenly between pre-expensing (n = 1,048 firms × 2 years) and post-expensing periods. I find no systematic differences between the initial sample (not tabulated) and the restricted sample.

From the pre-expensing to the post-expensing period, the average annual CEO option grant decreases significantly (p-values not tabulated) from $2.555m to $1.566m, the average restricted stock grant increases significantly from $0.686m to $1.499m, and the average equity compensation decreases, but insignificantly, from $3.308m to $3.153m. Mean cash and total compensation both decrease significantly after firms start to expense option grants, consistent with a reduction in CEO

FIGURE 2

Mean Value of CEO Stock, Option, Equity, Cash, and Total Compensation



compensation rents. The proportion of firms granting options decreases significantly from 75.7 percent to 60.4 percent, the proportion granting restricted stock increases significantly from 30.7 percent to 58.5 percent, and the proportion granting equity increases, but insignificantly, from 81.5 percent to 82.4 percent (not tabulated).[6]

The right side of Panel A provides descriptive statistics for management entrenchment, implied option expense, and pay for luck with one value per firm. The sample sizes are less than 1,048 due to data requirements. Management entrenchment (*EINDEX*) and implied option expense (*IOEmedian*) are median values from firms' pre-expensing periods. Measures of pay for good luck (*GAMMA*) and pay for bad luck (*TAU*) are log transformations of γ and τ, respectively. (See Appendix A.)

Implied option expense (*IOEmedian*) averages 0.56 percent of market capitalization, and ranges from 0 percent to 21.38 percent. The *mean* values for *TAU* and *GAMMA* are negative for option grants, equity grants, and cash compensation, indicating that these CEO pay components are at least partially adjusted for sector performance and, thus, weakly consistent with relative performance evaluation. On the other hand, *median* values for *TAU* and *GAMMA* indicate pay-for-luck asymmetry that is advantageous to CEOs. The median values for *TAU* are negative for all pay components (except stock grants), and the median values for *GAMMA* are positive for all pay components (except option grants). Thus, most of the firms in the sample at least partially shielded CEO compensation from the effects of unfavorable peer-group performance (bad luck), but most did not shield CEO compensation from the effects of favorable peer-group performance (good

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[6] In tests for changes in means, I use the non-parametric Wilcoxon rank-sums test for option and stock compensation to allow for the non-normality of their truncated distributions, and the parametric t-test for the other pay components. p-values (two-tailed) are < 0.001 for options, stock, and cash compensation, 0.070 for total compensation, and 0.552 for equity compensation. Changes in proportion are based on the Chi-square test. p-values are < 0.001 for option and stock compensation, and 0.470 for equity compensation.

TABLE 2

Descriptive Statistics

Panel A: Descriptive Statistics for Variables Used in Tests of H1 through H3, for the Restricted Sample in Table 1

Variables with Annual Values[a]

Variable[c]	n	Means $t = -3,-2$	Means $t = 0,+1$	Min.	Max.
O_{it} (\$000)	4192	2,555	1,566	0	27,028
S_{it} (\$000)	4192	686	1,499	0	11,471
EC_{it} (\$000)	4192	3,308	3,153	0	31,996
TCC_{it} (\$000)	4192	1,795	1,341	55	8,571
TC_{it} (\$000)	4192	5,261	4,805	188	37,885
R_{it}	4192	0.296	0.115	−0.731	2.670
L_{it}	4192	0.336	0.147	−0.438	1.425
GL_{it}	4192	0.353	0.165	0.000	1.425
BL_{it}	4192	−0.016	−0.018	−0.438	0.000
DIV_{it}	4192	0.325	0.303	0.000	1.000
$CFLOW_{it}$	4192	−0.006	−0.013	−0.266	2.396
BM_{it}	4192	0.631	0.639	0.032	2.048
$\ln SALES_{it-1}$	4192	7.298	7.600	0.090	12.457
IR_{it-1}	4192	0.003	−0.015	−4.201	2.839

Variables with One Value Per Firm Pre-Expensing Period[b]

Variable[c]	n	Mean	Median	Min.	Max.
EINDEX	1019	2.034	2.000	0.000	6.000
IOEmedian	1037	0.560	0.310	0.000	21.380
GAMMA:					
Option grants	830	−0.073	−0.002	−4.265	4.038
Stock grants[d]	228	0.006	0.493	−4.386	4.268
Equity grants	855	−0.036	0.047	−4.037	3.996
Cash pay	989	−0.010	0.015	−2.036	1.787
Total pay	992	0.012	0.054	−2.325	2.215
TAU:					
Option grants	830	−0.217	−1.089	−5.037	7.482
Stock grants[d]	228	0.357	0.000	−5.335	8.127
Equity grants	855	−0.241	−0.999	−4.996	6.719
Cash pay	989	−0.158	−0.258	−3.312	3.847
Total pay	992	−0.276	−0.594	−3.549	4.077

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[a] For annual variables, means are reported separately for the pre-expensing period when the intrinsic value method of option grant accounting was employed ($t = -3,-2$) and the post-expensing period when the grant-date fair value method was employed ($t = 0,+1$). The year prior to grant-date fair value adoption ($t = -1$) is omitted from the tabulation.

[b] Variables found once per firm are from the pre-expensing period as described in the paper.

[c] See Appendix A for variable definitions. O_{it}, S_{it}, and EC_{it} are for all firms regardless of whether they made equity grants. CEO compensation (O, S, EC, TCC, and TC) is in constant dollars with 2007 as the base year. GL_{it} and BL_{it} are for firms with non-zero values for GL_{it} and BL_{it}, respectively. Variables are winsorized at the 1st and 99th percentile, except those that are log transformed.

[d] The small number of pay-for-luck estimates for stock grants is due to the low frequency of stock compensation during the pre-expensing period for sample firms.

(continued on next page)

TABLE 2 (continued)

Panel B: Pearson Correlations for Primary Variables Used in Test of H1 through H3, Pooled over $t = \{-3,-2, 0,+1\}$

	$\ln O_t$	$\ln S_t$	$\ln EC_t$	$\ln TCC_t$	$\ln TC_t$	FAS	R_t	GL_t	BL_t	DIV_t	$CFLOW_t$	BM_{t-1}	$\ln SALES_{t-1}$	IR_{t-1}
$\ln O_t$	1.00	*0.03*	**0.73**	**0.22**	**0.55**	**−0.18**	0.02	0.02	−0.01	**−0.04**	−0.00	**−0.15**	**0.22**	0.03
$\ln S_t$		1.00	**0.49**	**0.14**	**0.41**	**0.28**	**−0.06**	**−0.14**	0.03	**−0.07**	**−0.05**	**0.06**	**0.25**	**−0.05**
$\ln EC_t$			1.00	**0.25**	**0.71**	0.01	−0.01	**−0.05**	−0.00	**−0.05**	**−0.03**	**−0.11**	**0.30**	−0.01
$\ln TCC_t$				1.00	**0.64**	**−0.14**	**0.07**	−0.01	**0.06**	**−0.14**	**−0.07**	−0.02	**0.45**	0.01
$\ln TC_t$					1.00	−0.03	0.02	**−0.07**	0.01	**−0.12**	**−0.08**	**−0.13**	**0.56**	**0.05**
FAS						1.00	**−0.20**	**−0.36**	−0.01	−0.02	*−0.04*	0.02	**0.09**	−0.01
R_t							1.00	**0.50**	**0.27**	**0.08**	**0.06**	**−0.25**	**−0.07**	0.02
GL_t								1.00	**0.31**	**0.05**	0.03	**−0.05**	**−0.15**	0.00
BL_t									1.00	−0.00	*−0.04*	**−0.10**	*0.04*	*0.04*
DIV_t										1.00	**0.15**	−0.02	**−0.30**	0.01
$CFLOW_t$											1.00	**0.10**	**−0.19**	0.00
BM_{t-1}												1.00	**0.09**	−0.01
$\ln SALES_{t-1}$													1.00	0.02
IR_{t-1}														1.00

See Appendix A for variable definitions. There are 4192 observations for each correlation statistic. Correlation coefficients in bold (italic) are significant at less than 0.01 (0.05) level using a two-tailed test.

luck). Instead, for most of the firms in the sample, the sensitivity of CEO pay to favorable peer-group performance is greater than the sensitivity of CEO pay to firm-specific performance (i.e., a firm's stock return less the mean return for the firm's industry-size peer group). The results suggest that a majority of firms use relative performance evaluation for bad luck but not good luck and that, while pay-for-luck practices vary considerably across firms, most firms reward good luck at least as much as firm-specific performance.

Panel B of Table 2 provides Pearson correlation statistics for key sample variables. As expected, CEO compensation (in log form) and $\ln SALES_{t-1}$ are positively correlated, ranging from 0.22 for option compensation ($\ln O$) to 0.56 for total compensation ($\ln TC$). The positive correlation (0.25) between equity and cash pay ($\ln EC$ and $\ln TCC$) is consistent with common factors, including size, affecting both forms of compensation. As would be expected, firm stock return (R) is positively correlated with good luck (GL;+0.50) and bad luck (BL;+0.27).

VI. REGRESSION RESULTS

Preliminary Analysis

Table 3 reports the results for Equation (1), which tests for changes in growth in CEO pay components following the adoption of option expensing. I use a Tobit model for option, restricted stock, and equity grants to allow for censoring of the dependent variable at 0,[7] and an OLS regression model for cash and total compensation. The analyses in Table 3 (and Tables 4 through 7) include the two years before ($t = -3$, $t = -2$) and the two years after ($t = 0$, $t = +1$) the adoption of option expensing for the 1,048 firms with continuous data over $t = -3$ to $t = +1$ (see Table 1). The "transition" year preceding adoption ($t = -1$) is excluded because CEO compensation in that year may have been influenced by anticipated option expensing and, therefore, may not be representative of either pre- or post-expensing CEO pay.[8]

The coefficients on *FAS* indicate that, compared to their growth rates in the pre-expensing period, option grants *decrease* and restricted stock grants *increase* when firms adopt option expensing (p-values < 0.001, p-values two-tailed).[9] These findings are consistent with both the perceived cost hypothesis and prior research for voluntary option expensing (Carter et al. 2007). The rate of growth in total equity compensation also decreases significantly ($p = 0.037$), consistent with Brown and Lee (2007) who find less than a dollar-for-dollar substitution of stock for option grants in anticipation of SFAS 123(R).

The rate of growth in cash and total compensation both decrease after firms adopt option expensing. This result is consistent with improved transparency and heightened concern over potential political costs when all forms of compensation are expensed. After controlling for other factors affecting CEO compensation, the coefficient on *FAS* in the total compensation regression indicates that, on average, the rate of change in total CEO compensation for the sample firms

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[7] An alternative to Tobit is the Heckman procedure, which would provide estimates of both the probability of observing an equity grant and the determinants of the expected level of equity pay conditional on a grant. The Tobit procedure is constrained such that the independent variables and coefficients on those variables are the same for both the selection and the outcome equation (Breen 1996). I use the Tobit model in the interest of parsimony given the analysis of multiple pay components.

[8] Using $t = -4$ to $t = -2$ as the pre-expensing period and $t = -1$ to $t = +1$ as the post-expensing period, I find results that are qualitatively the same as those presented here.

[9] Technically, for the Tobit specifications, the correct statement is "compared to their probability and growth rates in the pre-expensing period, the probability of and rate of growth in option grants *decrease*, and the probability of and rate of growth in restricted stock grants *increase*." The simpler construction is used for brevity and consistency of expression.

TABLE 3

Changes in CEO Compensation following the Adoption of Grant-Date Fair Value Option Accounting

	Form of Compensation				
Variable[a]	Options	Stock	Equity	Cash	Total
Intercept	−0.278	−15.133	−0.758	2.218	2.490
	−0.3	−10.9***	1.1	16.1***	15.3***
$TREND_{it}$	−0.007	0.477	0.086	−0.029	−0.003
	−0.13	5.0***	1.8*	−3.4***	−0.3
FAS_{it}	−1.329	1.377	−0.369	−0.201	−0.119
	−5.9***	3.9***	−2.1**	−6.2***	−3.1***
R_{it}	0.267	0.313	0.275	0.143	0.167
	1.7*	1.2	2.2**	6.3***	6.3***
DIV_{it}	−0.021	0.076	0.244	0.009	0.058
	−0.1	0.3	2.1**	0.4	2.3**
$CFLOW_{it}$	1.616	−1.809	0.618	0.094	0.193
	2.4**	−1.4	1.2	1.0	1.7*
BM_{it-1}	−1.989	1.933	−1.190	−0.037	−0.346
	−7.2***	4.4***	−5.6***	−1.0	−7.4***
$\ln SALES_{it-1}$	0.359	0.687	0.480	0.086	0.189
	7.6***	9.2***	12.9***	11.8***	20.5***
IR_{it-1}	−0.269	−0.492	−0.270	−0.013	−0.036
	−3.6***	−4.4***	−4.8***	−1.3	−3.0***
$\ln C_{it-1}$	0.702	0.824	0.460	0.656	0.545
	32.3***	26.4***	25.8***	61.8***	43.7***
Regression method	Tobit	Tobit	Tobit	OLS	OLS
n total	4192	4192	4192	4192	4192
n censored	1341	2321	756	None	None
OLS R^2				60.7%	56.1%

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively, using a two-tailed test.
Other variables have a two-tailed p-value of 0.10 or higher.

[a] Dependent variable is the log of compensation granted to CEO of firm *i* in year *t*. Parameter estimate is listed above and t-statistic below for each independent variable. See Appendix A for variable definitions. $\ln C_{it-1}$ refers to the log of lagged compensation for the type of compensation represented in a regression model.

following the implementation of SFAS 123(R) is approximately 11.2 percentage points lower than the rate of change in the pre-expensing period ($-0.112 = \exp(-0.119) - 1$).

Results for control variables are consistent with expectations. All pay components are positively associated with their lagged values ($\ln C_{it-1}$) and with firm size ($\ln SALES_{t-1}$) (p-values < 0.001). The association between firm performance (R) and compensation is positive and significant for cash, equity, and total pay (p-values $\leq$ 0.036), weakly positive for option pay (p-value = 0.089), and insignificant for stock grants. As expected, equity and total compensation are negatively associated with BM_{it-1} and IR_{it-1} (p-values $\leq$ 0.01) and positively associated with dividend constraints (DIV) (p-values $\leq$ 0.040). Only option compensation is significantly and positively associated with cash flow constraints ($CFLOW$) (p-values = 0.018). Because cash compensation is largely independent of BM, IR, and DIV, the association between total compensation and those three variables is apparently due to equity compensation effects that flow through to total compensation. In summary, after controlling for other factors known to affect CEO compensation, the results in Table 3 show that option expensing is associated with a change in the mix of compensation components and a decrease in the rates of growth in cash and total compensation for sample firms over the two years after versus before SFAS 123(R) adoption.

Results for H1

If financial reporting considerations were unimportant to compensation design choices under SFAS 123, then there would be no association between the effect that employee stock option grants had on pre-expensing reported income and subsequent changes in the rate of growth in CEO compensation. The perceived cost hypothesis maintains that financial reporting considerations did play a key role in compensation decisions. Building on the perceived cost hypothesis, H1 predicts that the effect of SFAS 123(R) on CEO compensation will be increasing in the extent to which firms relied on employee option compensation under SFAS 123.

The regression model for H1 in Table 4 differs from the model in Table 3 by the inclusion of *OPTEXP* (the 0 to 1 ranking of firms by median implied option compensation expense under SFAS 123) and $OPTEXP * FAS$. In Table 4, the coefficient on *OPTEXP* is positive for option grants ($p\text{-value} = 0.002$) and negative for stock grants ($p\text{-value} = 0.011$). In other words, the rate of growth in CEO option compensation is increasing and the rate of growth in CEO stock compensation is decreasing in the pre-expensing period as implied option expense increases. Consistent with H1, the coefficient on the interaction term $OPTEXP * FAS$ is negative and significant for option grants ($p\text{-value} = 0.024$) and positive and significant for restricted stock grants ($p\text{-value} = 0.014$). Thus, both the decline in the growth rate in option grants and the increase in the growth rate in restricted stock grants in the post-expensing period are larger, on average, for firms with higher levels of implied option expense in the pre-expensing period. These findings suggest that income considerations may have influenced the mix of CEO option and stock compensation under SFAS 123.

Next, consider the association between *OPTEXP* and equity and total compensation. The coefficient on *OPTEXP* is significantly positive ($p\text{-values} < 0.001$) for both compensation components. The coefficient on $OPTEXP * FAS$, however, is significantly negative for equity compensation and negative but not significant for total compensation. The results for *OPTEXP* and $OPTEXP * FAS$ for equity compensation, taken together, suggest that the reporting advantage of option compensation under SFAS 123 may have spurred a growth in equity compensation that was partially reversed when the reporting advantage of option grants ended with SFAS 123(R). On the other hand, the combined results for those two variables for total CEO compensation cast some doubt on the findings for equity compensation because I would expect the equity compensation effects to flow through to total compensation. Finally, as in Table 3, the results show that the rate of growth in CEO cash compensation decreases in the post-expensing period as shown by the negative and significant coefficient on *FAS*. However, the decrease in the rate of growth in cash pay is not associated with pre-expensing implied option compensation expense.

The results in Table 4 suggest that the reporting benefits of option compensation under SFAS 123 may have influenced some firms' compensation design choices, particularly the mix of option and stock compensation. The findings suggest that, under SFAS 123, the rate of growth in CEO option grants was higher and the rate of growth in stock grants was lower than either would otherwise have been without the financial reporting advantage of stock option compensation. The results further suggest that when reporting considerations were sufficiently important, some firms may have altered the incentive properties of CEO compensation packages in exchange for the reporting benefits of option grants.

TABLE 4

Regression Results for H1 Showing the Association between Implied Option Expense and Changes in CEO Compensation following the Adoption of Grant-Date Fair Value Option Accounting

	Form of Compensation				
Variable[a]	Options	Stock	Equity	Cash	Total
Intercept	−0.690	−14.483	−1.136	2.280	2.50
	−0.8	−10.5***	−1.7*	16.1***	15.2***
$TREND_{ij}$	−0.030	0.500	0.069	−0.028	−0.007
	−0.5	5.2***	1.5	−3.2***	−0.7
FAS_{it}	−0.772	0.421	0.039	−0.201	−0.073
	−2.4**	0.82	0.2	−4.2***	−1.3
$OPTEXP_i$	1.109	−1.538	0.957	−0.031	0.198
	3.1***	−2.5***	3.4***	−0.6	3.2***
$OPTEXP_i * FAS_{it}$	−1.058	1.835	−0.790	−0.019	−0.084
	−2.3**	2.5***	−2.2**	−0.3	−1.1
R_{it}	0.206	0.385	0.232	0.141	0.160
	1.3	1.5	1.9*	6.2***	6.0***
Controls	Yes	Yes	Yes	Yes	Yes
Regression	Tobit	Tobit	Tobit	OLS	OLS
n total	4148	4148	4148	4148	4148
n censored	1297	2287	722	None	None
OLS R^2				59.8%	55.7%

*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively, using a two-tailed test.
Other variables have a two-tailed p-value of 0.10 or higher.

[a] Dependent variable is the log of compensation granted to CEO of firm *i* in year *t*. Parameter estimate is listed above and t-statistic below for each independent variable. See Appendix A for variable definitions. Table 4 uses the same control variables as Table 3; results for controls are not tabulated in the interest of brevity. The coefficients and significance levels of control variables are qualitatively and quantitatively similar to those reported in Table 3.

Results for H2

This paper hypothesizes that some firms may have disguised CEO compensation rents under SFAS 123. If that is the case and if compensation rents are more transparent under SFAS 123(R), then shareholders with sufficient power to act fully in their best interests could and would enforce the elimination of rents after SFAS 123(R). Conversely, more entrenched CEOs who are protected by governance provisions that limit shareholder power would be able to preserve all or some of those rents.

In Table 5, *EINDEX* represents each firm's median entrenchment index under SFAS 123. Under H2, the coefficient on *EINDEX* is expected to be positive because I expect more entrenched CEOs to have higher rates of growth in compensation in the pre-expensing period. The results in Table 5 provide only weak support for this prediction. Of the five pay components, stock grants (p-value = 0.008) and equity compensation (p-value = 0.069) are significantly and positively associated with *EINDEX*. All other pay components are statistically independent of *EINDEX* (p-values ≥ 0.205).

The second variable of interest in Table 5 is the interaction term *EINDEX* ∗ *FAS*. Because more entrenched CEOs are expected to avoid some of the post-expensing reduction in the rate of growth in compensation, the coefficient on the interaction term is expected to be positive. Consistent with

TABLE 5

Regression Results for H2 Showing the Association between Management Entrenchment and Changes in CEO Compensation following the Adoption of Grant-Date Fair Value Option Accounting

	Form of Compensation				
Variable[a]	**Options**	**Stock**	**Equity**	**Cash**	**Total**
Intercept	−0.322	−15.234	−0.755	2.160	2.483
	−0.4	−10.6***	−1.1	15.2***	14.9***
$TREND_{ij}$	−0.013	0.437	0.072	−0.026	−0.002
	−0.2	4.5***	1.5	−2.9***	−0.2
FAS_{it}	−1.798	1.214	−0.853	−0.194	−0.211
	−5.7***	2.5***	−3.5***	−4.3***	−4.0***
$EINDEX_i$	0.093	0.326	0.105	−0.001	0.009
	1.3	2.7***	1.8*	−0.1	0.7
$EINDEX_i * FAS_{it}$	0.218	0.116	0.247	−0.008	0.042
	2.1**	0.7	3.0***	−0.5	2.4**
R_{it}	0.212	0.197	0.219	0.144	0.157
	1.3	0.8	1.8*	6.2***	5.8***
Controls	Yes	Yes	Yes	Yes	Yes
Regression	Tobit	Tobit	Tobit	OLS	OLS
n total	4076	4076	4076	4076	4076
n censored	1306	2245	733	None	None
OLS R^2				60.7%	56.4%

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively, using a two-tailed test.
Other variables have a two-tailed p-value of 0.10 or higher.

[a] Dependent variable is the log of compensation granted to CEO of firm *i* in year *t*. Parameter estimate is listed above and t-statistic below for each independent variable. See Appendix A for variable definitions. Table 5 uses the same control variables as Table 3; results for controls are not tabulated in the interest of brevity. The coefficients and significance levels of control variables are qualitatively and quantitatively similar to those reported in Table 3.

H2, the coefficient on $EINDEX * FAS$ is positive and significant (p-values ≤ 0.038) for options, equity, and total compensation. The post-expensing reductions in rates of growth for those three pay components are significantly less pronounced for more entrenched CEOs as predicted by H2. The change in the rate of growth in cash compensation, however, is statistically independent of management entrenchment (p-value = 0.595).

Taken as a whole, the results in Table 5 indicate that more entrenched CEOs were relatively less affected by the requirement to expense options compared to less entrenched CEOs. First, more entrenched CEOs receive higher rates of growth in restricted stock grants in the pre-expensing period (the coefficient on $EINDEX > 0$ for restricted stock). Second, they receive the same post-expensing increase in the rate of growth in restricted stock compensation as their less entrenched counterparts (the coefficient on $EINDEX * FAS$ is insignificant for restricted stock). Third, they retain more of their pre-expensing rate of growth in option compensation than their less entrenched counterparts (the coefficient on $EINDEX * FAS > 0$ for option grants).

In summary, the results suggest that more entrenched CEOs avoided some of the decrease in the rate of growth in option compensation experienced by their less entrenched counterparts and, at the same time, were able to match the increase in the rate of growth in stock grants of their less entrenched counterparts. The net effect is that more entrenched managers experienced smaller reductions in the rate of growth in equity and total compensation after SFAS 123(R) compared to less entrenched CEOs. Stated alternatively, when governance provisions gave shareholders more power to control the design of CEO compensation, firms made larger cuts in the rate of growth in CEO compensation after SFAS 123(R). The results suggest that SFAS 123 may have helped conceal inefficient, CEO-preferential pay practices and that SFAS 123(R) may have contributed to the reduction in those CEO compensation inefficiencies through more transparent reporting of the total economic cost of employee compensation.

Results for H3

Rewarding CEOs for positive sector performance (i.e., positive industry-size stock returns), which I term rewarding good luck, is a form of rent extraction that generally results in higher compensation, *ceteris paribus*. Thus, H3 predicts that compensation in the pre-expensing period will be higher for CEOs whose compensation is unusually sensitive to positive sector performance under SFAS 123. H3 further predicts that SFAS 123(R) will reduce compensation rents, leading to the expectation that the post-expensing reduction in CEO compensation will be greater when CEO pay is unusually sensitive to good sector performance under SFAS 123.

In Table 6, *PGL* is an indicator variable equal to 1 when the sensitivity of pay to good luck (γ) is above the sample median, and 0 otherwise. Pay-for-luck sensitivities are firm-specific and *ex ante* to any post-expensing compensation changes. *PGL* is derived separately for cash and total compensation. For option and stock compensation, however, I base *PGL* on pay-for-luck sensitivities for total *equity* compensation for two reasons. First, basing *PGL* on option and stock grants on total equity compensation, rather than separately on option and stock grants, increases the number of usable observations dramatically as seen in Table 2, Panel A. Second, stock and option compensation are, to some degree, substitutes, suggesting that pay-for-luck sensitivities for option and stock grants will be observed most reliably through total equity grants.

The earlier results in Panel A of Table 2 indicate that, for the majority of firms, CEO cash compensation, equity compensation, and total compensation in the pre-expensing period are more sensitive to good sector performance or good luck than to firm performance adjusted for sector performance or CEO skill. Thus, *PGL* is 1 for firms that compensated CEOs for good luck in a way that was unusually advantageous to their CEOs and inconsistent with relative performance evaluation.

The results in Table 6 are consistent with H3. The coefficient on *PGL* is positive and significant for all components of CEO compensation (p-values ≤ 0.012), indicating that the rate of growth in CEO pay in the pre-expensing period is higher, on average, when CEO pay is unusually sensitive to good luck in the pre-expensing period. The coefficient on the interaction term $PGL * FAS$ is negative and significant for option, equity, cash, and total compensation (p-values ≤ 0.042) and negative and marginally significant for restricted stock compensation (p-value $= 0.076$). On average, there is a greater reduction in the rate of growth in option, equity, cash, and total compensation and a smaller increase in the rate of growth in restricted stock grants after the implementation of SFAS 123(R) when CEO pay is unusually sensitive to good luck in the pre-expensing period.

The results in Table 6 provide new evidence about the potential negative consequences of pay for luck. When firms deviated more than normally from relative performance evaluation by compensating CEOs for good sector performance, CEOs received unusually high rates of growth in annual compensation under SFAS 123. The results suggest that rewarding CEOs for good luck was a potential source of CEO compensation rents. Additionally, the post-expensing reduction in the rate of growth in CEO compensation was largest for CEOs whose annual compensation was most sensitive to good sector performance in the pre-expensing period. The evidence suggests that, for

TABLE 6

Regression Results for H3 Showing the Association between Pay for Good Luck and Changes in CEO Compensation following the Adoption of Grant-Date Fair Value Option Accounting

	Form of Compensation				
Variable[a]	**Options**	**Stock**	**Equity**	**Cash**	**Total**
Intercept	1.496	−16.171	0.963	2.477	2.522
	1.6	−10.2***	1.4	17.0***	14.8***
$TREND_{ij}$	−0.069	0.566	0.049	−0.039	−0.006
	−1.1	5.2***	1.0	−4.3***	−0.6
FAS_{it}	−1.000	1.491	−0.152	−0.104	−0.064
	−3.6***	3.3***	−0.7	−2.7***	−1.4
PGL_i	0.585	0.853	0.720	0.144	0.096
	3.1***	2.5***	4.9***	5.2***	2.9***
$PGL_i * FAS_{it}$	−0.677	−0.804	−0.623	−0.160	−0.094
	−2.5***	−1.8*	−3.0***	−4.1***	−2.0**
R_{it}	0.295	0.340	0.318	0.147	0.173
	1.8*	1.2	2.6***	6.4***	6.3***
Controls	Yes	Yes	Yes	Yes	Yes
Regression method	Tobit	Tobit	Tobit	OLS	OLS
n total	3420	3420	3420	3956	3968
n censored	928	1816	459	None	None
OLS R^2				58.7%	56.6%

*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively, using a two-tailed test.
Other variables have a two-tailed p-value of 0.10 or higher.

[a] Dependent variable is the log of compensation granted to CEO of firm *i* in year *t*. Parameter estimate is listed above and t-statistic below for each independent variable. See Appendix A for variable definitions. Table 6 uses the same control variables as Table 3; results for controls are not tabulated in the interest of brevity. The coefficients and significance levels of control variables are qualitatively and quantitatively similar to those reported in Table 3.

some firms, rent extraction occurred under SFAS 123 and that there was reduction in those compensation rents after the implementation of SFAS 123(R).

Simultaneous Test of *OPTEXP*, *EINDEX*, and *PGL*

Table 7 reports the results of compensation regressions that simultaneously include implied employee option expense, management entrenchment, and the sensitivity of CEO pay to good luck. Two considerations warrant the additional analysis. First, the results for implied option expense in Table 4 and pay for good luck in Table 6 are similar, making it possible that the two measures capture a single phenomenon. This concern is compounded by the fact that, *ceteris paribus*, options awarded for good luck will lead to higher option grants. Thus, options awarded for good luck may partially explain cross-sectional differences in aggregate-employee implied option expense, particularly if pay for good luck extended to all or most employees. Second, entrenched managers may be more successful at arguing for pay for good luck, leading to an expectation that powerful managers are an underlying characteristic of firms that pay for good luck. Table 7 protects against a potential correlated omitted variables problem that might otherwise bias coefficient estimates.

The results in Table 7 show that the findings in Tables 4 through 6 are robust to the simultaneous inclusion of entrenchment, pay for good luck, and implied option expense in the

TABLE 7

Regression Results for the Simultaneous Effect of Implied Option Expense, Management Entrenchment, and Pay for Good Luck on Changes in CEO Compensation following the Adoption of Grant-Date Fair Value Option Accounting

	Form of Compensation				
Variable[a]	**Options**	**Stock**	**Equity**	**Cash**	**Total**
Intercept	1.127	−15.142	0.608	2.463	2.531
	1.2	−9.2***	0.8	16.1***	14.4***
$TREND_{ij}$	−0.089	0.536	0.019	−0.037	−0.012
	−1.4	4.9***	0.4	−3.9***	−1.1
FAS_{it}	−0.698	0.380	−0.024	−0.084	−0.088
	−1.6	0.5	−0.1	−1.4	−1.2
$OPTEXP_i$	0.981	−1.713	0.953	0.004	0.249
	2.6***	2.6***	3.3***	−0.1	3.8***
$EINDEX_i$	0.043	0.229	0.020	0.004	0.005
	0.6	1.7*	0.4	0.3	0.4
PGL_i	0.632	0.713	0.777	0.140	0.104
	3.3***	2.1**	5.3***	4.9***	3.1***
$OPTEXP_i * FAS_{it}$	−1.360	2.092	−0.964	−0.033	−0.111
	−2.8***	2.5***	−2.6***	−0.5	−1.3
$EINDEX_i * FAS_{it}$	0.216	0.034	0.220	−0.007	0.042
	2.0**	0.2	2.7***	−0.5	2.3**
$PGL_i * FAS_{it}$	−0.776	−0.629	−0.724	−0.151	−0.106
	−2.8***	−1.4	−3.5***	−3.8***	−2.3**
R_{it}	0.191	0.367	0.239	0.150	0.157
	1.2	1.3	1.9*	6.3***	5.6***
Controls	Yes	Yes	Yes	Yes	Yes
Regression method	Tobit	Tobit	Tobit	OLS	OLS
n total	3336	3336	3336	3824	3832
n censored	900	1762	442	None	None
OLS R^2				58.2%	56.4%

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively, using a two-tailed test.
Other variables have a two-tailed p-value of 0.10 or higher.

[a] Dependent variable is the log of compensation granted to CEO of firm *i* in year *t*. Parameter estimate is listed above and t-statistic below for each independent variable. See Appendix A for variable definitions. Table 7 uses the same control variables as Table 3; results for controls are not tabulated in the interest of brevity. The coefficients and significance levels of control variables are qualitatively and quantitatively similar to those reported in Table 3.

compensation regressions. The three measures appear to capture three different constructs that operate relatively independently in terms of their association with rates of growth in CEO pay components that occur before options were expensed and with changes in rates of growth in CEO pay components that occur after the adoption of option expensing. *PGL* is associated with higher rates of growth in compensation in the pre-expensing period for all pay components. The favorable income effect of option grants (*OPTEXP*) is associated with higher rates of growth in option and equity grants and lower rates of growth in stock grants under SFAS 123 and a more pronounced reversal in the rates of growth in those pay components after SFAS 123(R) adoption, consistent with reporting considerations that favored option grants over stock grants under SFAS 123. The favorable rate of growth associated with pay for good luck in the pre-expensing period is partially reversed after option expensing. The reduction in the growth in pay that occurs after SFAS 123(R) adoption is smaller for more entrenched CEOs.

VIII. SUMMARY AND CONCLUSION

This study provides evidence that the implementation of SFAS 123(R) is associated with changes in CEO compensation that are beneficial to shareholders. The evidence supports the conclusion that SFAS 123 generated favorable compensation consequences for CEOs but potentially negative economic consequences for shareholders. The first set of findings suggests that the earnings advantage of option grants under SFAS 123 influenced the mix of option and stock compensation that firms chose to offer. When firms lost the reported income advantage associated with option grants, there was a reduction in the rate of growth in CEO option compensation and an increase in the rate of growth in CEO stock compensation. Moreover, both changes in growth rates are more pronounced in firms with the larger favorable income effects from option grants under SFAS 123. These results suggest that some firms allowed reported income effects to influence the option/stock compensation mix decision under SFAS 123. The results further suggest that reported income effects may have led firms to sacrifice incentive considerations in their design of CEO compensation.

I also find that entrenched CEOs are less affected by some of the changes in CEO compensation that occurred with the implementation of SFAS 123(R). Firms with deeply entrenched managers have governance provisions that limit shareholder rights to replace, and therefore discipline, management. As management entrenchment diminishes, shareholder rights to replace and discipline the CEO increase. The findings show that the reduction in the rate of growth in CEO compensation after SFAS 123(R) is larger in firms where shareholders had relatively more power to discipline the CEO. Rational shareholders with sufficient power over the CEO would not be expected to reduce the rate of growth in CEO compensation unless compensation rents were evident. These results suggest that SFAS 123(R) is associated with a restructuring of CEO compensation in ways that were beneficial to shareholders.

In a novel set of tests, I find that when CEO pay is unusually sensitive to good sector performance or good luck under SFAS 123, the rate of growth in CEO compensation in the pre-expensing period is higher, and that the reduction in the rate of growth after SFAS 123(R) is greater. Rewarding CEOs for good sector performance is inconsistent with relative performance evaluation and a potential source of CEO compensation rents. The findings suggest that the reduction in the rate of growth in CEO compensation after SFAS 123(R) is larger in firms that paid larger CEO compensation rents under SFAS 123. Taken as a whole, this study provides evidence that changes in CEO compensation practices that occurred after SFAS 123(R) are beneficial to shareholders.

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APPENDIX A

VARIABLE DEFINITIONS

Acronym	Description	Calculation[a]
BM_{it}	book-to-market	book value of assets (*AT*)/[book value of liabilities $(AT - SEQ)$ + market value of equity (*PRCC_F* × *CSHO*)];
C_{it}, $\ln C_{it}$	CEO compensation	C_{it} is compensation paid to the CEO of firm i in year t as reported by ExecuComp, where C can stand for any one of five compensation components (options, stock, equity, cash, total). Compensation is adjusted for inflation using the consumer price index with 2007 as the base year. Compensation can be one of five measures shown just below. $\ln C_{it}$ refers to $\log(1 + C_{it})$;
O, $\ln O$	option grants	grant-date fair value of option grants equal to *OPTION_AWARDS_BLK_VALUE* when available, and *OPTION_AWARDS_FV* otherwise;
S, $\ln S$	stock grants	grant-date fair value of restricted stock grants equal to *RSTKGRNT* when available, and *STOCK_AWARDS_FV* otherwise;
EC, $\ln EC$	equity compensation	total of option and restricted stock grants, found $O + S$;
TCC, $\ln TCC$	cash compensation	salary plus bonus equal to *TOTAL_CURR*;
TC, $\ln TC$	total compensation	total CEO compensation found as $EC + TCC + Other\ Pay$, where *Other Pay* is equal to *OTHERANN* when available, and *OTHCOMP* otherwise;
$CFLOW_{it}$	cash flow shortfall	prior three-year average ($t-3$ to $t-1$) of [common and preferred dividends $(DVC + DVP)$ − investing cash flow (*IVNCF*) − operating cash flow (*OANCF*)]/total assets (*AT*);
DIV_{it}	dividend constraint	indicator variable set to 1 for any firm classified as dividend constrained in any of the prior three years ($t-3$ to $t-1$), and 0 otherwise. Dividend constrained classification occurs when [retained earnings at year end (RE) + cash dividends for the year $(DVP + DVC)$ + stock repurchases during year (*PRSTKC*)]/[prior year cash dividends and stock repurchases] < 2. *DIV* is also set equal to 1 if denominator is 0 for all three years;
$E\text{-}index_{it}$	entrenchment index	entrenchment index (Bebchuk et al. 2009) for firm i in year t;
$EINDEX_i$	entrenchment index	median value of *E-index* from the pre-expensing period for firm i;
FAS_i	grant-date fair value method indicator (expensing)	indicator variable set to 1 for any year a firm used grant-date fair value accounting for option grants (expensing), and 0 for any year a firm used intrinsic value method for option grants (no expensing). The adoption year is defined as the earlier of the Bear Stearns (2004) reported grant-date fair value adoption date and the Compustat adoption date. If Compustat stock option footnote (*STKCO_FN*) equals *OE*, then a firm-year is marked an option-expensing year. The decision to supplement the Bear Stearns report with *STKCO_FN* was made after comparing Bear Stearns and Compustat data indicating the adoption year. While the Compustat adoption date indicator is unreliable for very early adopters (many false negatives), the indicator is extremely accurate for later early adopters (no false positives) based on a comparison of annual report and proxy disclosures when Compustat and Bear Stearns had different adoption years;

(continued on next page)

APPENDIX A (continued)

Acronym	Description	Calculation[a]
IOE_{it}	implied option expense	implied option expense (*XINTOPT*) in year t as a proportion of market value of equity (*CSHO* × *PRCC_F*) year-end; measured annually for each firm-year prior to option expensing. Value is expressed as a percentage in tabulations;
$IOEmedian_i$	median IOE	median values of IOE_{it} for each firm from their pre-expensing period;
$OPTEXP_i$	ranked *IOEmedian*	*IOEmed* ranked from 0 to 1. The ranking forms the observed cumulative density function for financial reporting cost in the pre-expensing period;
IR_{it}	incentive residual	residual from the Core and Guay (1999) model. *IR* represents the deviation of a CEO's equity incentives from their predicted optimal level, where equity incentives are derived from the CEO's portfolio of equity holdings (restricted stock plus options);
L_{ij}	luck	firm return (R_{ij}) attributable to peer-group performance. See R_j below;
GL_i	good luck	Max(0,*L*);
BL_i	bad luck	Min(0,*L*);
S_{ij}	skill	$R_{ij} - L_{ij}$ represents firm performance that is attributable to other than peer-group performance. See R_{ij} below;
R_{ij}	stock return	buy-and-hold stock return for fiscal year j, derived from monthly returns with dividends as reported by CRSP;
R_j	industry-size, peer group stock return	peer group mean stock return in fiscal year j for firm i, excluding firm i. A firm's peer group is defined as all firms in the same industry (four-digit GIC code) and in the same market size quartile within the industry;
$\ln SALES_{it}$	sales	log of sales (*SALE*);
γ_i and τ_i	pay for luck	γ is the sensitivity of pay to good luck and τ is the sensitivity of pay to bad luck, both from the equation $\ln C_{ij} = \alpha_i + \beta_i R_{ij} + \gamma_i GL_{ij} + \tau_i BL_{ij} + \varepsilon_{ij}$ (Equation (4)) and measured on a firm-by-firm basis over the period a firm used the intrinsic value method for option grants as described fully in the paper. Parameters γ and τ are found separately for each of the five pay components (option grants, stock grants, equity grants, cash compensation, and total compensation);
$GAMMA_i$ TAU_i	pay for luck	$\log(1 + \gamma_i)$ when $\gamma_i \geq 0$, and $-1 * \log(1 + abs(\gamma_i))$ otherwise. *TAU* is found as $1 * \log(1 + \tau_i)$ when $\tau_i \geq 0$, and $-1 * \log(1 + abs(\tau_i))$ otherwise. *GAMMA* increases and *TAU* decreases with increasingly CEO-advantageous pay for luck;
PGL_i PBL_i	pay for luck	*PGL* is an indicator variable equal to 1 when γ_i is greater than sample median, and 0 otherwise; *PBL* is an indicator variable equal to 1 when τ_i is less than the sample median, and 0 otherwise; and
$TREND_{ij}$	calendar trend	$TREND_{ij}$ is equal to fiscal year j minus 1992. *TREND* indexes observations by calendar (fiscal year) time.

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[a] Compustat XPF variable names are in parentheses in italicized capital letters. ExecuComp variables are in italicized capital letters.

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BOOK REVIEWS

Stephen A. Zeff, Editor

Editor's note: Two copies of books for review should be sent to the Book Review Editor: Stephen A. Zeff, Rice University, Jesse H. Jones Graduate School of Business, 6100 Main St., Houston, TX 77005. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

RONALD J. HUEFNER, *Revenue Management: A Path to Increased Profits* (New York, NY: Business Expert Press, 2011, ISBN 13: 978-1-60649-141-6, pp. x, 145).

In reading Ronald Huefner's book, I could not help but be reminded of my own experiences in teaching cost accounting and management. Teaching materials and plans in this area inevitably revolve heavily around concepts and techniques for allocating costs and measuring the profitability of products, services, and customers, leaving comparatively little time for the "so what?" questions. Once the measurement is done, how should we use the information to improve profitability?

Without a systematic framework, this part of the discussion can quickly become a generic exercise in developing a "laundry list" of broad approaches—such as discount pricing for large and predictable orders, preferential pricing for "strategic" customers, and even the wholesale "firing" of customers—with little in the way of prescription for how to choose among these approaches, let alone facilitate their implementation. These approaches are all variants of differential pricing, and Huefner's book reminds us that the field of revenue management provides the appropriate systematic framework for making these choices optimally. In doing so, the book makes a strong case for the need to better integrate cost and revenue management processes within organizations.

Huefner's target audience is practitioners, and the book seems to be particularly aimed at accounting and financial managers. Perhaps appropriately, given this target audience, the book is written at a relatively high level and focuses on providing a general introduction to revenue management applications and techniques as well as methods for measuring and monitoring their efficacy. The "30,000 foot" view taken in the book, however, presents both strengths and weaknesses. On the one hand, it makes the topic of revenue management in all its various forms accessible at an introductory level. On the other, it allows little in the way of detailed guidance for developing or implementing revenue management practices within organizations.

It is, of course, difficult to accomplish both tasks in one book. Huefner's focus on the former makes the book most appropriate for financial and accounting managers who need a general introduction to the topic of revenue management. It may also be useful for marketing and operations managers who need a general introduction to cost accounting concepts that can be utilized to evaluate and ensure the profitability of revenue management processes.

The first two chapters highlight the importance of revenue management as a field and provide a brief historical overview of its origins in airlines, along with its subsequent development and application in a variety of other service industries. Chapter 2 provides a particularly useful overview of industry- and firm-level characteristics that give rise to the demand for revenue management, including the presence of fixed and perishable capacity (e.g., airline seats, hotel rooms), high fixed costs, and uncertain but predictable demand patterns.

Chapters 3–6 focus on the integration of revenue management with various cost measurement and analysis techniques, ranging from contribution margin and capacity analysis to opportunity costs and the theory of constraints. Huefner provides a strong case in this section for the role of cost systems in ensuring the success of revenue management efforts. In drawing a much-needed link between cost and revenue management techniques, this is perhaps the strongest portion of the book. However, given the target audience of finance and accounting managers who are likely to understand cost accounting concepts comparatively well, these chapters tilt too heavily toward cost measurement rather than revenue management. This is most notable in Chapter 5, where Huefner provides a very good, and relatively detailed, overview of the CAM-I capacity model, but devotes less than a paragraph to applying the model in revenue management decisions.

Despite this, Huefner should certainly be given some credit for drawing the link between capacity cost analysis and revenue management. Detailed models of an organization's cost structure should allow managers to proactively assess, for any given level of revenue-generating activity, the appropriate level of capacity required in various types of resources (personnel, office space, hardware, and other indirect support resources). In this vein, his work in Chapter 5 appropriately articulates a view similar to that of Cooper and Kaplan's (1999) "fundamental equation of activity-based costing," which goes something like the following:

Cost of Resources Supplied = Cost of Resources Used + Cost of Unused Capacity.

The left-hand side of this equation captures the cost of resources committed during a particular accounting period, while the right-hand side simply partitions this cost into the costs of resources utilized in productive and non-productive activities. As Cooper and Kaplan point out, cost systems like activity-based costing are focused on the right-hand side of this equation—modeling the costs of resources used and, by extension, providing transparency into where unused capacity exists in the organization. With information from such cost systems in hand, managers need to decide whether to reduce unused capacity by reducing the cost of resources supplied (cost management) or by reallocating that capacity to productive revenue-generating use (revenue management).

Chapters 7–9 focus on the "bread and butter" issue of revenue management—differential pricing. These chapters provide a broad overview of different approaches to segregated pricing (e.g., by customer, location, distribution channel, or product) and more specific techniques such as discounting, bundling, and markdowns. While these chapters are useful in providing a review of different pricing approaches that fall under the broad umbrella of revenue management, the weakness of the "30,000 foot" approach is apparent here, as no guidance or framework is offered for choosing which approaches work best and under what circumstances.

Chapters 10–12 point to potentially important roles for accounting and finance professionals to play in measuring and monitoring the efficacy of revenue management efforts within organizations. These chapters highlight the importance of considering the effects of current revenue management efforts on future customer behavior and profitability. The price discrimination at the heart of many revenue management techniques carries potential risks for organizations in the form of customer perceptions of unfairness, eroded trust, and reduced loyalty (Chapter 10). Revenue management decisions, in the form of price discounting, bundling, and other techniques also need not necessarily result in more profitable customers (Chapter 11).

Huefner begins to address the issue of how to measure all of these effects in Chapter 12, where he advocates a detailed analysis of different revenue sources. Overall, these chapters do a good job in highlighting the importance of these issues, but the ideas could be pushed further—particularly with regard to what type of management reports and reporting processes would be necessary to monitor, evaluate, and manage the risks and benefits of revenue management efforts over time.

In sum, the book serves as a good introduction to revenue management concepts for finance or accounting professionals who are unfamiliar with this field. More importantly, it provides a call to arms for better integration of revenue and cost management systems and points to an important and relatively unexplored role for these professionals in monitoring, evaluating, and managing ongoing revenue management efforts within organizations.

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Reporting Business Risks: Meeting Expectations (London, U.K.: ICAEW Financial Reporting Faculty, 2011, ISBN 978-0-85760-291-6, pp. v, 79). Downloadable at http://www.icaew.com.

Reporting Business Risks: Meeting Expectations is a thought leadership piece developed by the ICAEW's Financial Reporting Faculty as part of its *Information for Better Markets Initiative*. In response to calls for improved disclosures on entity business risks following the global financial crisis of 2007, its stated purpose is to make "a timely contribution to debate about how risk reporting should evolve" (p. iii). The principal authors are Brian Singleton-Green and Robert Hodgkinson.

Chapter 1 discusses the growing demand for risk reporting and highlights a series of past reports issued by the ICAEW, calling for improved risk disclosures, beginning with *Financial Reporting of Risk: Proposals for a Statement of Business Risk*, published in 1997. Chapter 2 discusses implications of selected prior research studies involving quantitative or qualitative risk disclosures and concludes that researchers "often express a degree of disappointment with it [risk reporting], sometimes suggest that disclosure requirements have had a limited effect, and tend to make comments along the lines that there is 'formal disclosure but substantial non-disclosure'" (p. 16). Chapter 3 presents the authors' views on challenges to risk reporting, and in Chapter 4 the authors present seven principles they propose that are potentially generally applicable [across industry sectors] "points for consideration by those interested in improving risk reporting and by preparers of corporate reporting information" (p. 35).

The publication should be a useful primer on business risk reporting for those who have not followed related developments in the U.S. and abroad. For example, Appendices 1 and 2 provide summaries of current risk reporting requirements embodied in U.S. SEC regulations, EU Directives, German law and accounting standards, the U.K. Corporate Governance Code and related guidance, the Basel II Accord and IFRS, as well as excerpts from selected "significant" documents calling for improved risk reporting that have been published during the past 20 years. However, the report falls short of my high (perhaps unattainable) personal standard on what constitutes thought *leadership*.

I do not believe that the "seven principles for better risk reporting" presented in Chapter 4 constitute thought leadership. These principles are: (1) tell users what they need to know, (2) focus on quantitative information, (3) integrate into other disclosures, (4) think beyond the annual reporting cycle, (5) keep lists of principal risks short, (6) highlight current concerns, and (7) report on risk experience. These guiding principles are useful but not particularly revealing.

On the positive side, the discussion presented in Chapter 3, on risk reporting challenges, highlights some significant impediments to effective risk reporting (e.g., the inherent unreliability of forward-looking information and costs versus benefits perceived by managers). For example, the authors recount the following issue raised during a 1997 discussion between researchers and standard setters in the U.S.—"there is no ex post settling up so there is no basis for assessing completeness or accuracy of risk disclosures" (p. 20)—and conclude in the Chapter 3 summary, "It is impossible to know even after the event whether most qualitative, and some quantitative, risk reporting is accurate or inaccurate. This must limit the reliance that users can place on it" (p. 31). On this issue, I believe additional thought-provoking discussion could have been provided by the authors on questions such as what sources of evidence, if disclosed, would help illuminate to investors the nature, extent, and time-horizon of principal business risks and what, fundamentally, it means to "settle up" within a risk assessment and response context. During the period of high inflation in housing prices leading up to the financial crisis of 2007, some financial institutions may have adopted a high-risk, high-return strategy of loosening credit underwriting standards in order to increase the volume of mortgages written and securitized, while other institutions may have adopted a more conservative strategy of tightening such standards to mitigate the risk of future loss should future prices revert toward the mean. Would qualitative disclosures of such strategies, and changes in them over time, have improved investors' understanding of the nature, extent, and time-horizon of risks? Is the execution of such a strategy verifiable? Auditors already review credit underwriting policies and procedures, related internal controls, and samples of other loan documentation when assessing the reasonableness of provisions for loan losses and fair value estimates for retained interests. What are examples of principal business risks for which there is no "ex post settling up?" Additional discussion on questions such as these would have been helpful to me.

Also, Chapter 3 includes an interesting discussion on risk-disclosure costs versus benefits as they might be perceived by managers. The authors differentiate a cost-of-capital reduction due to reducing information asymmetries from a cost-of-capital increase in response to increased transparency of downside risks. The implication is that managers may rationally resist additional downside risk disclosures that may reduce information asymmetry, because the net effect on the firm's cost of capital and managers' remuneration would not be beneficial to the manager. Such would be the case if, for example, the reduction in a security's risk premium due to increased market-wide confidence in the completeness and accuracy of company disclosures is more than offset by the increase in the risk premium caused by revelation of "worse than expected" downside risks. This might help explain why managers prefer "substantial non-disclosure of risks," but I believe that the inclusion of additional discussion that links implications of risk opacity back to investor behavior, moral hazard, and market efficiency would have provided for a more comprehensive discourse on costs versus benefits.

In Chapter 4, I was particularly interested in section 4.5 on thinking beyond the annual reporting cycle, where the authors recommend that "consideration be given to how risk reporting . . . might be taken out of the annual reporting cycle and instead updated as the need arises on firms' websites" (p. 41). I found appealing the notion that such an approach would increase the speed and frequency of feedback on changes in risk assessments and related outcomes, and this made me wonder whether, and if so how, more timely feedback and response could possibly have dampened financial and behavioral trends preceding and continuing into the financial crisis.

Also, I was interested in the Chapter 4 discussion in section 4.2 on the guiding principle, "focus on quantitative information," particularly the authors' proposal that "The stress tests organized by banking and insurance supervisors also provide valuable information about risk, and it would be helpful to explore the use of such disclosures as an additional form of risk reporting by banks and insurers" (p. 37). However, I found somewhat contradictory the authors' conclusion following this proposal that "In general, we do not think that attempts to quantify the probability of one-off future events or to forecast the potential losses that might result from them are likely to provide useful information" (p. 38). This statement appears to me to suggest that the authors believe that disclosures involving scenario or sensitivity analyses likely would not heighten users' awareness of risks.

Consider the following statement made by Robert S. Kaplan during his plenary address to the 2010 Annual Meeting of the American Accounting Association: "[Lehman Brothers] somehow failed after a 5 percent decline in US real estate prices. After 40+ years of academic research on capital markets and financial economics, is Lehman's [2008] 10-K disclosure the best we can offer to quantify and disclose a company's risk exposure? I hope not." (Reproduced in Appendix 4, Risk Reporting and the Financial Crisis, p. 73.) Lehman did disclose the following qualitative risk information in its 2008 annual report: "Further declines in real estate values in the US and continuing credit and liquidity concerns could further reduce our level of mortgage loan originations and increase our mortgage inventory while adversely affecting its value" (p. 73). Now, let's suppose that Lehman had disclosed not only this qualitative information, but also their best estimate of what percentage decline in real estate prices would likely impair their ability to meet current financial obligations. Would such a disclosure have reduced information asymmetry? Should regulations require quantitative disclosures of best estimates of such "tipping points" for the principal risks threatening the reporting entity? Would such estimates be more reliable and verifiable than measures involving management's estimate of the probability that the tipping point (5 percent decline in real estate prices) will obtain? Would such a disclosure prompt investors to make their own assessments of the probability of a 5 percent decline in real estate prices? Additional discussion in section 4.2 on questions such as these may have improved my understanding of the rationale supporting the authors' conclusion that they "do not think that attempts . . . to forecast the potential losses that might result from them [one-off future events] are likely to provide useful information."

Finally, I was somewhat surprised that, against the backdrop of the recent global financial crisis and growing demand for better risk reporting, substantial variance in risk reporting requirements across different jurisdictions, and evidence suggesting current risk reporting requirements often have only limited effectiveness, the authors concluded, "[F]or these reasons, we do not propose new or tougher regulation of risk reporting" (p. 35). I believe in the old saying, "If it's not broke, don't fix it." However, if investors around the world who purchased mortgage-backed securities in the period leading up to the financial crisis were blind-sided due, in part, to a poor understanding and estimation of risks, which was due, in part, to opacity, which was due, in part, to ineffective risk disclosure regulation, a continuing debate on how risk reporting regulations might be improved seems warranted.

In conclusion, perhaps the ICAEW's current report *is* thought leadership, as my reflections on its contents have prompted my consideration of a variety of issues pertaining to the current state of affairs in business risk reporting that, in turn, has *led* my thinking to new places. For me, personally, the work achieved its stated purpose: to make "a timely contribution to debate about how risk reporting should evolve" (p. iii).

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Annual Report and Editorial Commentary for *The Accounting Review*

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I. INTRODUCTION

Having completed one year as *The Accounting Review*'s (*TAR*) Senior Editor, I appreciate the opportunity to report to the AAA Publications Committee and Board of Directors, and to the readers, authors, reviewers, and fellow co-editors of *TAR*, as well as to share some of my thoughts on *TAR* processes. I believe that such communication concerning *TAR* and our editorial processes will give all constituents, including prospective authors, a better sense of what we seek to do and how we seek to do it.

II. *THE ACCOUNTING REVIEW* EDITORIAL PROCESS

My experience as one of 13 co-editors during Steve Kachelmeier's 2008–2011 regime has been very helpful to me during my first year in this position. Because I found Steve's editorial approach and specific policies to be effective, I have generally continued them. The biggest change during our first year was the conversion to the AllenTrack online submission and manuscript management system, which I will discuss later in this report. Key policies that we continued include delegating most editorial decisions to co-editors, while centralizing a reviewer-selection process that is critically important, in part because of *TAR*'s two-reviewer system. We also centralize communication with reviewers, editors, and authors as all such communications go through the *TAR* office via the AllenTrack system.

My impression when my term started was that finding the right *TAR* "team" would be the most important determinant of how effectively the *TAR* system would operate in the next three years. Today I believe this even more strongly. Our team is comprised of 14 knowledgeable and fair-minded co-editors, 143 insightful and conscientious Editorial Board members, and 543 *ad hoc* reviewers; an Editorial Assistant, Stacy Hoffman, who is both incredibly fast and almost infallible; and doctoral student research assistants, Michele Frank and Eric Chan, who know, or can learn, who does what very well in almost every conceivable area of accounting research, and even more importantly, which of these experts are available to review a manuscript on any given day. When the rest of the team does their job in a conscientious and skillful manner, there is not much the senior editor can do to mess things up, and of course, the converse is also true.

During the 12 months before my term started on June 1, 2011 my two biggest tasks were recruiting the *TAR* "team" described above and determining whether an online manuscript processing system would work for *TAR*. On the first challenge, each of my fellow co-editors was very gracious and generous when I approached them with an exceptional opportunity; i.e., the

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chance to increase their workload significantly and lose further control over their personal schedule, all while making potentially contentious decisions and receiving no compensation. Other team members have been correspondingly professional, efficient, and generally a pleasure to work with.

Six months before my term started, I feared the worst in terms of the prospective conversion to an online submission and manuscript management system. Fortunately, AAA leadership, particularly Publications Manager Diane Hazard, orchestrated a change in vendors to Allen Press. Our technical support from Elizabeth Garrett at Allen Press has been extremely responsive and effective in working with Stacy and me.

Stacy, Elizabeth, and AAA Publications Assistant Lindsay Peters organized a relatively seamless transition whereby all submissions starting June 1, 2011, whether new manuscripts or revisions, entered the new online system, with Stacy manually entering the data for a number of manuscripts originally processed under the previous email-based system and for authors unfamiliar with such systems. At the same time, we maintained a parallel email-based system to support processing of some of the other 190 manuscripts that were already "in play" as of June 1, 2011. I have compared our two operating manuscript management systems in parallel, each with hundreds of manuscripts, to operating a railroad with both an "old train" and a "new train" running at the same time on roughly parallel tracks, while gradually transferring the "passengers" (manuscripts) to the "new train" whenever possible. After 12 months, I am happy to say that virtually all passengers are now on the "new train" online system with essentially zero lost passengers, delays, or complaints in the process, for which I cannot thank Stacy and Elizabeth enough. Further, my sense is that most authors like the online system, and even if a few would have preferred we not change, they have adapted and have done so without complaining loud enough that I could hear.

Stacy and I meet most days with *TAR* doctoral research assistants, Michele Frank and Eric Chan, to identify reviewers and an editor for recently submitted manuscripts. Michele and Eric alternate days attending the *TAR* meeting, bringing to our meeting their research of the literature in the area of each new manuscript to identify two or three potential reviewers from our Editorial Board, one of whom we will select and Stacy will invite via the online system, and the other(s) whom we will list as the alternate if the first choice declines. Likewise, using the same general approach, we identify a primary and alternate second reviewer, who typically is not an Editorial Board member. The second reviewer may be more or less experienced than the Editorial Board member, depending on the specific circumstances unique to each manuscript. Further, the online system assigns the terms "Reviewer #1" and "Reviewer #2" based on the sequence in which we receive the reviews, so these terms do not reveal any particular characteristics of the reviewers.

We almost always have at least five to ten new manuscripts in need of reviewers, and during periods of heavy submissions, the inventory of new manuscripts ready for an invitation to reviewers and an editor can grow to 25 or so, which we attempt to gradually work down over the next several weeks. Our output becomes Stacy's input as she first seeks agreement from the editor to handle the manuscript and to either accept the proposed pair of reviewers or to suggest alternative names. Once the editor has agreed on two reviewers, Stacy invites the reviewers and if both agree to review, Stacy uses the online system inventory to track the progress of the reviews. As a result, we require an average of two weeks to identify potential reviewers, receive agreement from the proposed editor to handle the manuscript using either the proposed reviewers or alternative names the editor prefers, invite the reviewers, and receive their agreement, along with inviting alternative reviewers when our first choices decline. The final section of this report returns to this issue by examining the distribution of manuscript turnaround times.

III. EDITORIAL AND PUBLICATION STATISTICS

Table 1: Annual Activity Summary

The 2012 report follows the structure of Steve Kachelmeier's 2011 report (Kachelmeier 2011), which should facilitate comparisons across years. As a general overview comment, the 2012 workflow was generally similar to that in 2011. I will next discuss the results shown in each of the tables.

Table 1, Panel A reports *TAR*'s comparative workflow statistics for each of the years 2009–2012, with the data on a journal year basis from June 1 of one year through May 31 of the following year. Panel A shows that in 2012 the volume of new submissions increased to 568 from 495 in 2011, with the 568 figure being very close to the 557 submissions received in Steve's first year, which ended May 31, 2009. Likewise, the 153 resubmissions in 2012 are comparable to the 163 resubmissions received in the first year of Steve's regime, but somewhat below the resubmission totals of 212 and 228 in his second and third years. This probably reflects some authors accelerating their revision efforts in an attempt to achieve closure on a final decision before the change in editorial regime on June 1, 2011.[1] Finally, Panel A of Table 1 shows that overall *TAR* sent out 722 editorial decisions during the year ending May 31, 2012, which is up slightly from 708 decisions in 2011.

Next, to permit comparability to years before 2009, Table 1, Panel B provides data on the calendar-year (CY) basis, which was used in 2008 and previous years. Panel B indicates that the total of 582 new manuscripts submitted to *The Accounting Review* in CY2011 continues a trend of growing submissions. Although the growth rate has slowed in recent years, new submissions today are at approximately twice the level in 2000. The third and fourth columns of Table 1, Panel B show that the increased submissions prompted an increase from four to five issues a year in 2006, and then to six issues a year in 2008, along with an even greater increase in the annual *TAR* total page count from approximately 500–600 pages in 1998–2000 to more than 2,200 pages in 2010 and 2011.

Table 2: Annual Outcome Summary

The four lines of Table 2, Panel A report the outcomes for the total decision letters (column (e)) generated during each of the fiscal years 2009–2012. The final two columns of Panel A of Table 2 use the data in columns (a) to (e) to generate two estimated annual "acceptance rates" for each of the last four fiscal years. For 2012, Rate 1 in column (e) divides the 75 acceptances by the 75 + 426 = 501 "final outcome" decisions, yielding the acceptance rate of 15.0 percent. Rate 2 in column (f) retains the same 75 acceptances in 2012 in the numerator but now adds to the denominator the 221 "Revise and Uncertain Decisions" in 2012, which yields an acceptance rate of 10.4 percent (75/722). Rate 1 can be viewed as an upward biased measure of the acceptance rate, whereas Rate 2 is downward biased, such that the "true" acceptance rate lies somewhere in between—perhaps between 12 and 13 percent for 2012. The results for 2012 in Table 2, Panel A are largely similar to those for 2011, consistent with the current editorial team's decision to generally retain policies from the preceding regime, together with a generally similar experience in the submission and review process in 2012 as in 2010–2011.

Table 2, Panel B presents a second perspective on acceptance rates. Panel B treats each year's set of new submissions as a unique "cohort" and tracks the eventual outcomes for that cohort over the next several years. Thus, the first line of Panel B shows that for the journal year ending May 31,

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[1] In response to authors' concerns that a revised manuscript received soon after the editorial transition would be handled by a new editor without full appreciation of the manuscript's history, Steve and I have coordinated on a policy that generally seeks to retain the original editor whenever there is more significant doubt about an editorial decision and the original editor is available and willing to continue as editor even though the editor's three-year term has ended. In particular, when a resubmission is received on or after June 1 of the transition year and the reviewers have made split recommendations, we have made every effort to retain the original editor.

TABLE 1

Annual Activity Summary

Panel A: Annual Activity Summary by Journal Year

Journal Year Ending May 31	Manuscripts in Process at Beginning of Year (a)	New Submissions Received (b)	Resubmissions Received (c)	Manuscripts Available For Evaluation (d) = (a) + (b) + (c)	Decision Letters Sent (e)	Manuscripts in Process at End of Year (f) = (d) – (e)
2009	133	557	163	853	719	134
2010	134	502	212	848	673	175
2011	175	495	228	898	708	190
2012	190	568	153	911	722	189

Panel B: New Submissions by Calendar Year

Calendar Year	New Submissions	*TAR* Volume (Number of Issues)	Total Page Count Per Volume
1998	196	Vol. 73 (4)	577
1999	239	Vol. 74 (4)	530
2000	260	Vol. 75 (4)	492
2001	328	Vol. 76 (4)	701
2002	324	Vol. 77 (5)	1,034
2003	327	Vol. 78 (4)	1,108
2004	307	Vol. 79 (4)	1,216
2005	389	Vol. 80 (4)	1,274
2006	413	Vol. 81 (5)	1,181
2007	443	Vol. 82 (5)	1,393
2008	482	Vol. 83 (6)	1,698
2009	508	Vol. 84 (6)	2,094
2010	494	Vol. 85 (6)	2,221
2011	582	Vol. 86 (6)	2,253

Explanations of columns in Panel A:

(a) Manuscripts in process as of the beginning of the year include all new submissions and revisions pending decision (generally awaiting review), but exclude manuscripts awaiting revision from authors.

(b) New manuscripts received during the year, including desk-rejected manuscripts but excluding resubmissions of revised manuscripts.

(c) Resubmissions of invited revisions.

(d) Summation of columns (a), (b), and (c).

(e) Completed decision letters, including subsequent decisions for manuscripts previously evaluated as a "revise and resubmit" decision within the same fiscal year.

(f) Manuscripts awaiting review and/or editorial decision as of the end of the journal's fiscal year. These manuscripts become the "beginning inventory" for the following year.

2009, *TAR* received 557 new submissions (column (a)), of which only one file (column (d)) was still awaiting further revision and a final decision as of three years later on May 31, 2012. Of the other 556, 96 had been accepted (column (b)) and 460 rejected (column (c)), such that the last two columns of Panel B show that the final acceptance rate for this cohort will be 17.2 (17.4) percent if the final study is rejected (accepted).

TABLE 2
Annual Outcome Summary

Panel A: Outcomes by Fiscal Year

Journal Year Ending May 31	Decision Letters Sent (a)	Rejections (b)	Revise and "Uncertain" Decisions (c)	Acceptances and Conditional Acceptances (d)	Acceptance Rate 1: (e) = (d)/[(b) + (d)]	Acceptance Rate 2: (f) = (d)/(a)
2009	719	408	230	81	16.6%	11.3%
2010	673	403	207	63	13.5%	9.4%
2011	708	437	193	78	15.1%	11.0%
2012	722	426	221	75	15.0%	10.4%

Panel B: Final Outcome Resolution for All New Submissions

Journal Year Ending May 31	New Submissions Received (a)	Cumulative Acceptances through May 31, 2012 (b)	Cumulative Rejections through May 31, 2012 (c)	Files Pending Further Revision as of May 31, 2012 (d)	Lower Bound on Acceptance Rate: (e) = (b)/(a)	Upper Bound on Acceptance Rate: (f) = [(b) + (d)]/(a)
2009	557	96	460	1	17.2%	17.4%
2010	502	62	433	7	12.4%	13.7%
2011	496	31	404	61	6.3%	18.6%

Explanations of columns in Panel A:

(a) This column is the same as column (e) of Table 1, Panel A, reflecting all decision letters sent during the fiscal year, including decisions on manuscripts that had been evaluated previously within the same fiscal year (with invitations to revise and resubmit). Thus, the number of unique manuscript files processed is somewhat lower than the number of decision letters sent.

(b) Exhibit 1 separates the total rejections into manuscripts rejected due primarily to insufficient contribution and manuscripts rejected due primarily to a perceived threat to the validity of the reported claims.

(c) Exhibit 1 separates this column into decisions logged as standard "revise-and-resubmit" outcomes and decisions logged as "uncertain" outcomes that allow but do not necessarily encourage resubmission.

(d) Total acceptances and conditional acceptances include manuscripts published and forthcoming in *The Accounting Review*. Thus, the total acceptances during a year do not represent the actual number of articles published during that year. Acceptance totals include only those files accepted from the normal review process, and exclude invited commentaries from AAA Presidential Scholar Lectures and from occasional research forums.

Next, for the journal year ending May 31, 2010, Panel B shows that of 502 new submissions, as of 24–36 months later on May 31, 2012, 62 were accepted, 433 rejected, and 7 remain pending. The last two columns of Panel B indicate that the final acceptance rate for the journal year 2010 cohort will be between 12.4 percent and 13.7 percent, and probably closer to the latter figure because most of the 7 pending files have a good probability of being accepted. Finally, the last row of Table 2, Panel B shows that of 496 new submissions for the journal year ending May 31, 2011 (and as of May 31, 2012), 31 were accepted, 404 rejected, and 61 remain pending, yielding a final acceptance rate between 6.3 percent and 18.6 percent, depending on the how many of the remaining 61 are eventually accepted. Projecting similar results for pending files as experienced last year, the final acceptance rate for journal year 2011 is likely to be 13–14 percent.

The results in Table 2, Panel B suggest that the *TAR* acceptance rate for annual cohorts of new submissions has declined from approximately 17.3 percent in 2009 to approximately 13–14 percent in 2010 and 2011. This pattern, and in particular, the higher acceptance rate in Steve's first year of 2009, is consistent with Steve's projections in his 2011 *TAR* report and reflects a transition effect resulting from the policy decision by the AAA Publications Committee to increase the number of issues and published studies in *TAR*. By generally permitting revision when at least one reviewer was favorable over the first year of their regime, Steve and his co-editors were able to gradually build a buffer inventory of accepted manuscripts sufficient to ensure full issues on a regular basis. As the senior editor following Steve, I am particularly grateful to have taken over with a healthy inventory of approximately nine or ten months of accepted manuscripts, which we have roughly maintained since then. For example, as of June 30, 2012, we have accepted manuscripts to fill the next five issues through March 2013 and are now filling the May 2013 issue.

As a final note on this analysis of *TAR* acceptance rates over the last several years, I wish to emphasize the importance of our general policy of giving individual editors complete discretion on their decisions. In particular, reviewers provide reports and offer recommendations, both of which are absolutely essential, but the final decision is the editor's call. When both reviewers recommend rejection, the editor typically concurs. The more difficult editorial decisions occur when the reviewers are split and/or when one or more reviewers recommend "Uncertain." These are the cases in which the editor truly earns the generous salary that our system provides. My observation is that just as Steve's team of co-editors managed to make appropriate adjustments at the margin to build the inventory in Steve's first year and then became a bit more conservative, our team's editorial judgments appear to be generally consistent with the experience in the latter part of Steve's term. The data so far suggest that this process is generating acceptance rates in line with recent historical experience.

Exhibit 1: Histogram of Editorial Rounds and Outcomes

Exhibit 1 provides further details on the 722 editorial decisions reported in Table 2 for the journal year ending May 31, 2011. Exhibit 1 shows that 534 of the 722 decisions (74 percent) were first-round decisions, while 105 (15 percent) were second-round decisions and the remaining 83 (11 percent) were third round or later. Of the 534 first-round decisions, Panel A of Exhibit 1 shows rejection was the most common outcome, accounting for approximately 72 percent (247 + 137 = 384 of 534) of the first-round decisions, while the remaining 28 percent of first-round decisions were revisions.[2] Panel A also shows that of the 384 first-round rejections, slightly less than two-thirds (247) were due to insufficient contribution and slightly more than one-third (137) were attributed primarily to concerns with the study's validity.

Next, Exhibit 1, Panel A shows that of the 150 decisions in 2012 that permitted the authors to submit a revised manuscript, 69 were standard "revise and resubmit" decisions, while the other 81 were more qualified "uncertain" decisions. Both "revise and resubmit" and "uncertain" have outcome risk, but the degree of that risk is substantially higher for an "uncertain" decision. Specifically, an "uncertain" letter conveys the sense that neither the reviewers nor the editor can envision a viable revision path that would address the identified concerns, but that the editor recognizes that the author might be able to construct such a path. Accordingly, such a letter gives the author an *option* to revise and resubmit, but without explicitly encouraging the author to do so. The intent is to provide full disclosure to the author that withdrawing the manuscript might be in the author's best interest if the author's candid assessment is that the concerns raised cannot feasibly be addressed. Experience indicates that almost all recipients of "uncertain" letters choose to revise and

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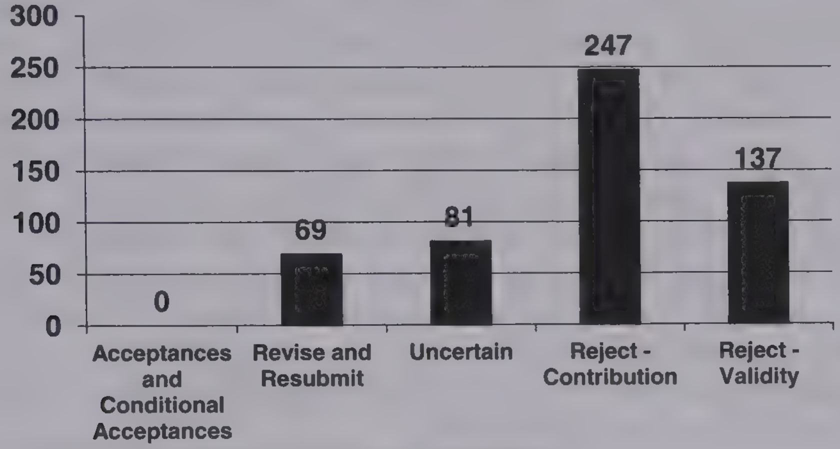
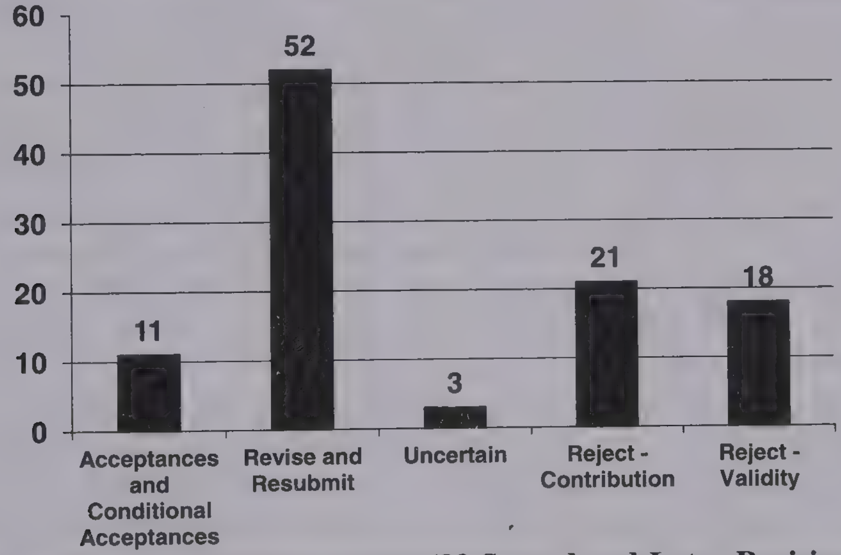
[2] The 384 rejections include 10 desk rejections that were returned to the authors without review and with a refund of the submission fee. I have classified these 10 decisions as rejections due to validity concerns.

EXHIBIT 1
Histogram of Editorial Rounds and 722 Outcomes
Journal Year Ending May 31, 2012

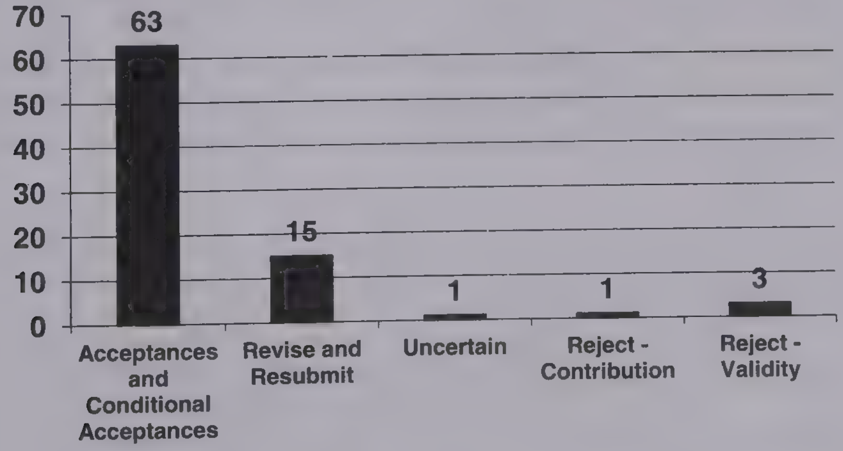
Panel A: First-Round Outcomes (534 New Submissions)



Panel B: Second-Round Outcomes (105 First Revisions)



Panel C: Third- and Later-Round Outcomes (83 Second and Later Revisions)



resubmit in spite of the cautions, but the rejection rate on "uncertain" revisions is substantially higher than that for standard invitations to revise and resubmit.

Moving to the second round or "first revision" decisions, Exhibit 1, Panel B shows that of the 105 total second-round outcomes, 11 were acceptances or conditional acceptances, 55 received invitations for further revision with three of these in the more qualified "uncertain" category, and the remaining 21 + 18 = 39 second-round letters were rejections, which are always painful. Although a second-round rejection is painful, experience suggests that a third-round rejection is even worse. This consideration compels editors to make difficult decisions on manuscripts that appear to have potential but that achieved only limited progress in the first revision.

By the time a manuscript gets to the third round or beyond, Exhibit 1, Panel C documents that the odds of success increase dramatically, with 63 of the 83 fiscal 2012 decisions being acceptance or conditional acceptance, a rate of 76 percent. Sixteen manuscripts received a further revise and resubmit during the third or later round, and four manuscripts were rejected at this advanced stage of the process. Of course, we seek to minimize such late round rejections by making the tough decisions sooner whenever possible, but in some cases further rounds appear to be the most appropriate decision despite the risk, in which case we go ahead. Finally, we note that Panel C also includes eight fourth-round decisions, four fifth-round decisions, and one sixth-round decision; nine of these 13 decisions were conditional acceptances.

Table 3: Submissions and Acceptances by Subject Area and Research Method

Panels A–D in Table 3 describe how submissions compare to acceptances by subject area, by research method, and by the combination of the two. The results offer important insight concerning patterns and trends, including whether submissions in certain subject areas or using certain methods were more or less likely than others to be published in *TAR* over the years 2009–2012. These panels are helpful in responding to conjectures that *TAR* systematically favors or disfavors particular areas or methods of research, where the conjecture may stem simply from comparing the number of publications across topic area or research methods. Table 3 provides careful, systematic data and enables readers to see the importance of evaluating acceptance rates only in light of the corresponding submission rates.

My impression is that this approach has been largely successful in generating a more informed discussion of these issues in recent years, so I am continuing it. Please note that Table 3 counts each study only once even though many studies go through several rounds of revision before eventually being published or rejected. This approach means that the figures in Table 3 will generally not agree precisely with those in Tables 1 and 2, which treat each submitted version of a study as a distinct manuscript, whereas Table 3 counts each study only once even if multiple versions of the manuscript are submitted across several rounds.

The general pattern in Table 3 indicates that *The Accounting Review* accepts articles at rates that generally track the corresponding submission rates very closely, whether by topic, by method, or by topic crossed with method.[3] The overall similarity in submission and acceptance rates is

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[3] The most noticeable deviation between 2012 submission rates and acceptance rates in the Table 3 occurs in Panel A for the financial accounting area, which accounts for 47.8 percent of all submissions but only 36.7 percent of all acceptances for the year 2012, as shown in the left-hand portion of Panel A. The corresponding results for the most recent four years in the right-hand portion of Panel A show financial accounting comprising 51.3 percent of all submissions and 46.9 percent of all acceptances. Conversely, Panel A reports that the managerial accounting area accounts for 13.0 percent of all submissions but 26.5 percent of all acceptances for the year 2012 and 12.8 percent of all submissions and 18.1 percent of all acceptances for the most recent four years of 2009–2012. In both cases the differences in rates between submissions and acceptances declines when we look over a longer period, consistent with the differences being temporary deviations.

TABLE 3

Submissions and Acceptances by Subject Area and Research Method Journal Year Ending May 31, 2012

Panel A: Submissions and Acceptances by Subject Area

	Year Ending May 31, 2012				Four Years from June 1, 2008–May 31, 2012			
	Submissions		Acceptances and Conditional Acceptances		Submissions		Acceptances and Conditional Acceptances	
Primary Subject Area	**Frequency**	**Percentage**	**Frequency**	**Percentage**	**Frequency**	**Percentage**	**Frequency**	**Percentage**
Auditing	122	18.0%	9	18.4%	395	18.0%	52	19.2%
Financial	324	47.8%	18	36.7%	1,128	51.3%	127	46.9%
Governmental and Not-for-Profit	13	1.9%	0	0.0%	31	1.4%	4	1.5%
International	53	7.8%	4	8.2%	141	6.4%	11	4.1%
Managerial	88	13.0%	13	26.5%	281	12.8%	49	18.1%
Systems	9	1.3%	0	0.0%	44	2.0%	6	2.2%
Taxation	50	7.4%	5	10.2%	142	6.5%	21	7.7%
Other	19	2.8%	0	0.0%	38	1.7%	1	0.4%
Total	678	100.0%	49	100.0%	2,200	100.0%	271	100.0%

Panel B: Submissions and Acceptances by Method

	Year Ending May 31, 2012				Four Years from June 1, 2008–May 31, 2012			
	Submissions		Acceptances and Conditional Acceptances		Submissions		Acceptances and Conditional Acceptances	
Primary Research Method	**Frequency**	**Percentage**	**Frequency**	**Percentage**	**Frequency**	**Percentage**	**Frequency**	**Percentage**
Analytical	58	8.6%	4	8.2%	184	8.4%	25	9.2%
Empirical-Archival	517	76.3%	36	73.5%	1,638	74.5%	186	68.6%
Experimental	81	11.9%	9	18.4%	277	13.0%	44	16.2%
Field and Case Study	6	0.9%	0	0.0%	22	1.0%	5	1.8%
Survey	6	0.9%	0	0.0%	47	2.1%	10	3.7%
Other	10	1.5%	0	0.0%	32	1.5%	1	0.4%
Total	678	100.0%	49	100.0%	2,200	100.0%	271	100.0%

(continued on next page)

TABLE 3 (continued)

Panel C: Submissions and (Acceptances) by Subject Area Crossed with Method, Journal Year Ending May 31, 2012

Subject and Method	Auditing	Financial	Managerial	Taxation	All Other Subjects	Total
Analytical	7	22	18	5	6	58
	(1)	(0)	(1)	(2)	(0)	(4)
	1%	3%	3%	1%	1%	9%
	(2%)	(0%)	(2%)	(4%)	(0%)	(8%)
Empirical-Archival	86	275	40	41	75	517
	(6)	(16)	(7)	(3)	(4)	(36)
	13%	41%	6%	6%	11%	76%
	(12%)	(33%)	(14%)	(6%)	(8%)	(73%)
Experimental	26	24	23	4	4	81
	(2)	(2)	(5)	(0)	(0)	(9)
	4%	4%	3%	1%	1%	12%
	(4%)	(4%)	(10%)	(0%)	(0%)	(18%)
All Other Methods	3	3	7	0	9	22
	(0)	(0)	(0)	(0)	(0)	(0)
	0.4%	0.4%	1%	0%	1%	3%
	(0%)	(0%)	(0%)	(0%)	(0%)	(0%)
Total	122	324	88	50	94	678
	(9)	(18)	(13)	(5)	(4)	(49)
	18%	48%	13%	7%	14%	100%
	(18%)	(37%)	(26%)	(10%)	(8%)	

(continued on next page)

TABLE 3 (continued)

Panel D: Submissions and (Acceptances) by Subject Area Crossed with Method, Four Years from June 1, 2008–May 31, 2011

Subject and Method	Auditing	Financial	Managerial	Taxation	All Other Subjects	Total
Analytical	27	76	52	13	16	184
	(4)	(10)	(7)	(4)	(0)	(25)
	1%	3%	2%	1%	1%	8%
	(1%)	(4%)	(3%)	(1%)	(0%)	(9%)
Empirical-Archival	255	959	123	115	186	1,638
	(31)	(102)	(22)	(16)	(15)	(186)
	12%	44%	6%	5%	8%	74%
	(11%)	(38%)	(8%)	(6%)	(6%)	(69%)
Experimental	102	74	65	11	25	277
	(14)	(13)	(13)	(1)	(3)	(44)
	5%	3%	3%	1%	1%	13%
	(5%)	(5%)	(5%)	(0%)	(1%)	(16%)
All Other Methods	11	19	41	3	27	101
	(3)	(2)	(7)	(0)	(4)	(16)
	1%	1%	2%	0%	1%	5%
	(1%)	(1%)	(3%)	(0%)	(1%)	(6%)
Total	395	1,128	281	142	254	2,200
	(52)	(127)	(49)	(21)	(22)	(271)
	18%	51%	13%	6%	12%	100%
	(19%)	(47%)	(18%)	(8%)	(8%)	

In Panels A and B, subject (Panel A) and method (Panel B) categories are as directed by the AAA Publications Committee. Manuscripts that overlap categories are assigned to the category that best captures the study's primary objective, as judged by the Senior Editor. To avoid double-counting invited revisions, the total of 678 unique submissions in each panel excludes submissions earlier within the year for which multiple decisions were recorded within the year on the same manuscript files.

In Panels C and D, cell entries reflect submissions first, followed by acceptances (in parentheses). The first and second rows for each cell indicate raw counts. The third and fourth rows compute percentages of the 678 unique submissions for fiscal 2012 (Panel C) and the 2,200 unique submissions for the four years from June 1, 2008 to May 31, 2012 (Panel D), excluding earlier submissions of the same manuscript files to avoid double counting for invited revisions.

consistent with the journal's policy of not emphasizing one area or method over another, but rather seeking to reflect the broad interests of AAA members. In turn, this general similarity in submission and acceptance rates is driven by our process of selecting editors and reviewers. By choosing reviewers for each submission who are experts in the area of that submission, we seek to subject each submission to a comparable review process.

Authors obviously exhibit self-selection preferences in determining the journals to which they direct their submissions. Although *TAR* publishes far fewer studies in the accounting information systems area or manuscripts that employ field-study methods than archival financial accounting studies, this difference in publication rates is driven primarily by differences in submission rates rather than by differences in acceptance rates. To see this, compare the two percentages in the lower row of each cell of Table 3, Panel D. The first (second) percentage indicates that cell's percentage of all submissions (acceptances).

In this way Table 3, Panel D allows one to compare the percentage of all submissions and acceptances by area and method over the last four years, 2009–2012. For example, the cell in Table 3, Panel D for the combination of the financial accounting area and the archival research method reports "44 percent (38 percent)," indicating that over the last four years 44 percent of all *TAR* submissions have been financial archival studies, while 38 percent of all acceptances have been financial archival studies. The resulting differential of [Acceptance Rate – Submission Rate], which is –6 percent for this cell, is the least favorable toward acceptance of any cell in Panel D, consistent with financial archival studies being the least-favored category of studies by this measure over 2009–2012.

In contrast to the preceding example, the overall pattern of submission and acceptance rates in Table 3, Panel D shows a generally very close correspondence between the two rates. Other than the –6 percent difference noted above, none of the cells have differences with absolute values greater than 2 percent. The only other distinctive pattern that I see in Table 3, Panel D is that managerial accounting studies have greater acceptance rates than submission rates across all four research method categories, resulting in the bottom row showing that managerial studies for all research methods represent 13 percent of submissions but 18 percent of acceptances. In my view, the rate differentials documented here are worth noting and worth tracking in the future. I believe that they reflect primarily random fluctuations that are not likely to persist over time.

The preceding comparison of submissions and acceptances across areas is related to the general *TAR* policy of openness with respect to a variety of research areas and methods within accounting. One of the important signals that the *TAR* senior editor can send with respect to openness to research in particular areas or using particular methods is through design of the team of *TAR* co-editors. For example, I have continued the prior regime's approach of signaling *TAR*'s openness to research in the accounting systems area and to studies using field-based methods by inviting Vern Richardson and Ken Merchant, respectively, to serve as *TAR* editors, continuing the approach that Steve began by inviting Jim Hunton and Shannon Anderson to support these same objectives. Although Table 3, Panels A and B indicate no acceptances in 2012 in either of these two cells, I can assure you that Ken, Vern, and I are eager to identify and publish outstanding research in these categories.

More broadly, I have found the process of identifying an ideal team of *TAR* editors to be one of the most challenging aspects that a new senior editor faces. The challenge comes from attempting to balance the desire to have an editorial team with subject matter and method expertise across a wide range of areas and methods, while at the same time keeping the total number of editors manageable and balancing the workload equitably across editors. An individual author would ideally prefer an editor with considerable expertise and enthusiasm for the author's research area and method, but this is not likely to be feasible for every manuscript in every area/method combination. Steve and I discussed these associated trade-offs at length as I sought to recruit the best possible team of editors.

Two important decisions that I made in attempting to address this issue should help to provide insight concerning why every area/method combination cannot have an expert among the co-editors and not every manuscript can be handled by an editor with expertise in the subject area. My first decision was to expand the total number of editors (besides the senior editor) from 13 in the prior regime to 14. The goal of this expansion was to improve our coverage of various area/method combinations.

Second, I made a conscious effort to adjust the mix of editors to better match the mix of expected submissions. While there is certainly no *TAR* system of designated "slots" for specific editor "types," it is important that the mix of editor expertise corresponds appropriately to the mix of submitted manuscripts. For example, there are obvious advantages to having an editor with tax expertise to handle tax submissions, and similarly for other areas and methods. With this rationale in mind, relative to Steve's distribution of 13 co-editors, I adjusted the mix of editors slightly away from the experimental/behavioral area and toward the financial archival area. I also used the additional 14th editor position to recruit Mike Ettredge to handle empirical auditing studies, an area that Steve could handle himself, given his auditing expertise, which I lacked.

I based these decisions on data showing that the editors with primary behavioral/experimental expertise handled, on average, significantly fewer manuscripts annually than the target figure of 40–50 new submissions per year that Steve and I advertised when recruiting editors. At the same time, Steve had to request some editors with financial accounting archival expertise to handle more than 50 manuscripts per year. Therefore, I made the shift to balance the workload across editors, while simultaneously attempting to balance the typical editorial expertise that authors would encounter across areas and methods.

To expand on the latter point, the senior editor effectively serves as the "residual claimant" for most manuscripts under the current system, such that when we receive more manuscripts than we have capacity to handle in a particular area over a given period of time, the senior editor generally handles these excess manuscripts. From my experience and what I understand of Steve's experience, such overflows are common, such that the senior editor is often writing decision letters outside his or her primary area of expertise. In my view this is a natural result of variation in the inflow of manuscripts, and reflects circumstances that our system can handle effectively. However, one unavoidable result is that some authors will necessarily receive a decision letter written by an editor without primary expertise in the topic or method of the study. In such cases the senior editor will almost always rely more heavily on the reviewers than he or she might if the manuscript were in his or her primary area of expertise. Of course, this can work either for or against the author's prospects of acceptance, depending on the reviewers' evaluation of the manuscript.

Table 4: Author Affiliations

Employer Affiliations

Table 4, Panel A tallies the employer affiliations of all authors who have published in *The Accounting Review*, with separate columns for Volume 87 (calendar year 2012) and four-year cumulative totals for Volumes 84–87 (calendar years 2009–2012). As in prior Annual Reports, Panel A adjusts for co-authorship by attributing $1/k$ of an article to each of its k coauthors. If an author lists two affiliations, then Panel A allocates half of that author's $1/k$ share to each. However, because Panel A does not adjust for faculty size, universities such as The University of Texas at Austin and University of Illinois at Urbana–Champaign with larger research-active faculties have an obvious advantage in the rankings.

Table 4, Panel A lists institutions in rank order based on the cumulative four-year column, as evidence of the extent of concentration/diversity among *TAR* authors. Both columns exclude invited commentaries from AAA Presidential Scholar Lectures, which we accept by policy, as well as three

TABLE 4

Author Affiliations

Panel A: Employer Affiliations for Articles Published in Calendar Years 2009–2012

Employer Affiliation for 278 Articles	Current Year Coauthor Adjusted Articles (Vol. 87, 2012)	Cumulative Articles in Vols. 84–87 (Calendar Years 2009–2012)
The University of Texas at Austin	3.08	11.50
The University of Illinois at Urbana–Champaign	1.50	8.00
University of Pennsylvania	2.50	7.50
University of Michigan	1.33	7.33
Stanford University	1.67	7.17
The University of Georgia	0.67	5.25
University of Toronto	0.50	5.25
Harvard University	0.50	4.92
Texas A&M University	2.00	4.92
The University of Arizona	1.25	4.83
Dartmouth College	1.17	4.58
Michigan State University	0.33	4.50
The University of Iowa	0.92	4.33
University of Notre Dame	1.00	3.92
University of Pittsburgh	0.83	3.83
The University of Chicago	0.50	3.83
The George Washington University	0.00	3.67
The University of Texas at Dallas	1.08	3.67
University of California, Berkeley	1.50	3.50
Indiana University–Bloomington	0.25	3.42
University of Southern California	0.33	3.42
The Hong Kong University of Science and Technology	1.50	3.33
Emory University	1.50	3.17
The University of New South Wales	1.00	3.17
The Ohio State University	0.33	3.08
Erasmus University	1.67	3.08
Nanyang Technological University	0.67	3.00
University of California, Los Angeles	1.00	2.92
The University of North Carolina at Chapel Hill	0.33	2.92
The Pennsylvania State University	0.00	2.83
Florida International University	0.67	2.75
The Hong Kong Polytechnic University	1.08	2.75
The University of Utah	1.00	2.67
University of Missouri–Columbia	0.33	2.50
University of Wisconsin–Madison	0.83	2.42
Brigham Young University	0.92	2.33
The University of Melbourne	0.00	2.33
University of California, Irvine	1.00	2.25
The Chinese University of Hong Kong	1.00	2.25
Maastricht University	0.33	2.17
Boston College	2.00	2.00
The University of Mississippi	0.00	2.00

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TABLE 4 (continued)

Employer Affiliation for 278 Articles	Current Year Coauthor Adjusted Articles (Vol. 87, 2012)	Cumulative Articles in Vols. 84–87 (Calendar Years 2009–2012)
Singapore Management University	0.00	2.00
The University of Texas at Arlington	1.00	2.00
Washington University at St. Louis	1.00	2.00
University of Oregon	1.17	2.00
Rice University	0.33	2.00
University of Alberta	0.75	1.92
Arizona State University	0.50	1.83
University of Florida	0.33	1.83
Massachusetts Institute of Technology	1.00	1.83
Baruch College–CUNY	0.25	1.75
Bentley University	0.00	1.75
The University of Oklahoma	0.00	1.75
George Mason University	1.00	1.67
Georgia Institute of Technology	0.67	1.67
University of Washington	0.67	1.67
University of Amsterdam	1.00	1.50
University of Arkansas	0.50	1.50
University of Colorado at Boulder	1.00	1.50
University of Houston	0.00	1.50
University of Maryland	1.00	1.50
National University of Singapore	0.50	1.50
Rutgers, The State University of New Jersey	1.00	1.50
Southern Methodist University	0.25	1.50
City University of Hong Kong	0.67	1.46
Drexel University	0.00	1.42
Duke University	0.75	1.42
University of South Carolina	0.50	1.42
Georgetown University	1.00	1.33
Miami University (Ohio)	0.00	1.33
University of Rochester	0.00	1.33
Santa Clara University	0.33	1.33
The University of Hong Kong	1.00	1.25
University of Minnesota	0.00	1.25
Purdue University	0.25	1.25
Tilburg University	1.00	1.25
The University of Alabama	0.33	1.17
New York University	0.00	1.17
Seoul National University	0.00	1.17
University of Virginia	0.00	1.17
Aarhus University	0.00	1.00
Universidad Carlos III de Madrid	0.00	1.00
Columbia University	0.00	1.00
Humboldt University–Berlin	0.00	1.00
Iowa State University	0.00	1.00
McGill University	0.00	1.00
University of New Hampshire	0.00	1.00

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TABLE 4 (continued)

Employer Affiliation for 278 Articles	Current Year Coauthor Adjusted Articles (Vol. 87, 2012)	Cumulative Articles in Vols. 84–87 (Calendar Years 2009–2012)
Northwestern University	0.00	1.00
Saint Louis University	0.00	1.00
Tel Aviv University	0.00	1.00
Virginia Commonwealth University	0.67	1.00
The College of William and Mary	0.00	1.00
Xavier University	0.00	1.00
Concordia University	0.00	0.96
Korea University	0.00	0.92
Oklahoma State University	0.67	0.92
DePaul University	0.83	0.83
Cornell University	0.00	0.83
Memorial University of Newfoundland	0.33	0.83
University of Nevada, Las Vegas	0.00	0.83
Lancaster University	0.00	0.75
University of Waterloo	0.25	0.75
University of Colorado at Denver	0.33	0.67
The University of Illinois at Chicago	0.33	0.67
Shanghai Jiaotong University	0.00	0.67
Shanghai University of Finance and Economics	0.00	0.67
University of South Florida	0.00	0.67
Temple University	0.33	0.67
The University of British Columbia	0.00	0.58
University of Connecticut	0.00	0.58
University of Massachusetts–Amherst	0.00	0.58
Northern Illinois University	0.25	0.58
Boston University	0.00	0.50
City University London	0.50	0.50
Florida State University	0.25	0.50
Goethe University	0.00	0.50
John Carroll University	0.50	0.50
King's College London	0.50	0.50
Lingnan University	0.50	0.50
University of Massachusetts–Lowell	0.00	0.50
University of Miami	0.00	0.50
National Cheng Kung University	0.50	0.50
Norwegian School of Management	0.00	0.50
Binghamton University, SUNY	0.50	0.50
University at Buffalo, SUNY	0.00	0.50
Towers Perrin	0.00	0.50
Tsinghua University	0.00	0.50
Utah State University	0.00	0.50
Washington State University	0.00	0.50
Yale University	0.00	0.50
University of Antwerp	0.00	0.33
The University of Auckland	0.00	0.33
The Australian National University	0.00	0.33

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TABLE 4 (continued)

Employer Affiliation for 278 Articles	Current Year Coauthor Adjusted Articles (Vol. 87, 2012)	Cumulative Articles in Vols. 84–87 (Calendar Years 2009–2012)
University of Bern	0.33	0.33
California State University, Long Beach	0.33	0.33
University of California, Davis	0.00	0.33
University of California, San Diego	0.00	0.33
Carnegie Mellon University	0.00	0.33
University of Central Florida	0.00	0.33
China Europe International Business School	0.00	0.33
Colorado State University	0.00	0.33
Georgia Southern University	0.00	0.33
Gradient Analytics, Inc.	0.00	0.33
HEC Paris	0.00	0.33
Inha University	0.00	0.33
INSEAD	0.00	0.33
University of International Business and Economics	0.33	0.33
Katholieke Universiteit Leuven	0.33	0.33
Lehigh University	0.00	0.33
Ludwig Maximilian University of Munich	0.33	0.33
University of Massachusetts–Dartmouth	0.00	0.33
McMaster University	0.00	0.33
Meijo University	0.00	0.33
The University of Memphis	0.33	0.33
Monash University	0.33	0.33
University of Navarra, IESE Business School	0.33	0.33
North Carolina State University	0.00	0.33
Northeastern University	0.00	0.33
Osaka University of Economics	0.00	0.33
University of Saskatchewan	0.33	0.33
Sungkyunkwan University	0.00	0.33
Syracuse University	0.00	0.33
The University of Tennessee	0.00	0.33
University of Toledo	0.33	0.33
Vanderbilt University	0.00	0.33
Virginia Polytechnic Institute and State University	0.00	0.33
Voyant Advisors, LLC	0.00	0.33
University of Western Australia	0.33	0.33
Worcester Polytechnic Institute	0.00	0.33
Ball State University	0.00	0.25
California State University, Fresno	0.00	0.25
Chapman University	0.00	0.25
University of Colorado at Colorado Springs	0.25	0.25
Illinois State University	0.00	0.25
Indian School of Business	0.00	0.25
Indiana University–Indianapolis	0.25	0.25
Kent State University	0.00	0.25
Mississippi State University	0.00	0.25
National Taiwan University	0.00	0.25

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TABLE 4 (continued)

Employer Affiliation for 278 Articles	Current Year Coauthor Adjusted Articles (Vol. 87, 2012)	Cumulative Articles in Vols. 84–87 (Calendar Years 2009–2012)
Prairie View A&M University	0.25	0.25
San Diego State University	0.00	0.25
Wuhan University	0.00	0.25
Barclays Global Investors	0.00	0.17
Copenhagen Business School	0.00	0.17
Interdisciplinary Center Herzliya	0.00	0.17
University of Southern Maine	0.00	0.17

Panel B: Ph.D.-Granting Affiliations for Articles Published in Calendar Years 2009–2012

Ph.D.-Granting Affiliation for 278 Articles	Current Year Coauthor Adjusted Articles (Vol. 87, 2012)	Cumulative Articles in Vols. 84–87 (Calendar Years 2009–2012)
University of Michigan*	4.00	14.92
The University of Iowa*	2.25	12.92
Stanford University*	3.50	12.83
The University of Texas at Austin*	4.00	11.83
The University of Illinois at Urbana–Champaign*	3.08	8.83
University of Washington*	2.75	8.17
University of Pennsylvania	2.50	7.83
The University of North Carolina at Chapel Hill	1.50	7.58
Michigan State University*	0.83	6.50
Northwestern University	1.25	6.42
The Ohio State University*	0.67	6.33
Cornell University*	2.00	6.25
University of Wisconsin–Madison*	0.50	5.75
University of California, Berkeley*	0.83	5.58
The Pennsylvania State University	0.58	5.58
Indiana University	1.17	5.33
University of Southern California	2.33	5.17
Massachusetts Institute of Technology	0.33	4.58
University of Rochester*	1.75	4.50
Carnegie Mellon University*	2.00	4.17
The University of Arizona	0.58	4.00
Columbia University	0.00	4.00
The University of Chicago*	0.58	3.67
New York University	0.25	3.42
University of Pittsburgh	1.33	3.33
The University of New South Wales	2.00	3.33
Texas A&M University	1.25	3.25
Washington University at St. Louis	0.33	3.08
University of Missouri–Columbia	0.33	3.08
University of Amsterdam	2.00	3.00
Emory University	0.67	3.00

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TABLE 4 (continued)

Ph.D.-Granting Affiliation for 278 Articles	Current Year Coauthor Adjusted Articles (Vol. 87, 2012)	Cumulative Articles in Vols. 84–87 (Calendar Years 2009–2012)
University of Minnesota*	0.00	3.00
Yale University	1.50	3.00
The University of Georgia	0.92	2.67
Harvard University	0.50	2.67
University of Colorado at Boulder	1.50	2.58
Arizona State University	0.67	2.33
The University of British Columbia	0.58	2.33
University of Oregon	0.00	2.25
University of Waterloo	0.92	2.08
The University of Melbourne	0.00	2.00
University of Florida	0.00	1.92
Purdue University	1.58	1.92
Oklahoma State University	1.00	1.83
University of Toronto	0.00	1.83
University of Navarra	0.00	1.75
Lancaster University	0.50	1.75
Katholieke Universiteit Leuven	1.67	1.67
The Hong Kong University of Science and Technology	0.00	1.67
Nanyang Technological University	0.33	1.67
University of New England–Australia	0.67	1.67
Tilburg University	0.83	1.67
Louisiana State University	1.00	1.58
City University of Hong Kong	0.83	1.50
Goethe University	0.00	1.50
Hebrew University	0.00	1.50
Duke University	1.00	1.33
Maastricht University	0.00	1.33
University at Buffalo, SUNY	0.33	1.33
The University of Texas at Dallas	0.25	1.33
University of California, Los Angeles	0.00	1.25
Temple University	0.33	1.25
Rutgers University	0.00	1.17
Tulane University	0.00	1.17
University of Antwerp	0.33	1.00
Boston University	0.00	1.00
Concordia University	0.00	1.00
University of Connecticut	1.00	1.00
Free University of Berlin	0.00	1.00
Georgia Institute of Technology	0.00	1.00
University of Maryland	0.50	1.00
Tel Aviv University	0.00	1.00
The University of Alabama	0.00	0.83
The University of Auckland	0.00	0.83
La Trobe University	0.00	0.83
University of Nebraska	0.50	0.83
Oxford University	0.33	0.83

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TABLE 4 (continued)

Ph.D.-Granting Affiliation for 278 Articles	Current Year Coauthor Adjusted Articles (Vol. 87, 2012)	Cumulative Articles in Vols. 84–87 (Calendar Years 2009–2012)
Erasmus University	0.33	0.67
Florida State University	0.00	0.67
Odense University	0.00	0.67
Syracuse University	0.00	0.67
University of Zurich	0.33	0.67
University of Arkansas	0.33	0.58
Kent State University	0.00	0.58
University of Alberta	0.00	0.50
The George Washington University	0.00	0.50
Griffith University	0.00	0.50
Norwegian School of Economics & Business Administration	0.00	0.50
Princeton University	0.00	0.50
Rensselaer Polytechnic Institute	0.50	0.50
The University of Sydney	0.50	0.50
The University of Texas at Arlington	0.00	0.50
University of Vienna	0.00	0.50
Warwick University	0.50	0.50
University of California, Irvine	0.33	0.33
Drexel University	0.00	0.33
University of Essex	0.00	0.33
Florida International University	0.33	0.33
Georgia State University	0.00	0.33
University of Glasgow	0.33	0.33
The Hong Kong Polytechnic University	0.00	0.33
Illinois State University	0.00	0.33
University of Manchester	0.00	0.33
University of Mannheim	0.33	0.33
University of Massachusetts–Amherst	0.00	0.33
University of Munich	0.33	0.33
Nagoya City University	0.00	0.33
University of North Texas	0.00	0.33
Osaka City University	0.00	0.33
Seoul National University	0.00	0.33
University of Sheffield	0.00	0.33
Singapore Management University	0.00	0.33
University of South Carolina	0.00	0.33
University of South Florida	0.00	0.33
Texas Tech University	0.33	0.33
Virginia Polytechnic Institute and State University	0.00	0.33
University of Houston	0.25	0.25
The University of Oklahoma	0.00	0.25
Stockholm University	0.25	0.25
The University of Texas at San Antonio	0.00	0.25
University of Trier	0.25	0.25

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TABLE 4 (continued)

Panel C: U.S. and Non-U.S. Submissions and Acceptances

	Journal Year Ending May 31, 2012	
	Number (Percentage) of Submissions Representing Unique Manuscript Files with Decisions	Number (Percentage) of Acceptances
U.S. Institutions	347.65 (68.2%)	36.92 (78.5%)
Non-U.S. Institutions	161.92 (31.8%)	10.08 (21.5%)

Panel D: Geographical Breakdown of Non-US. Submissions and Acceptances, Journal Year Ending May 31, 2012

	Number (Percentage) of Submissions Representing Unique Manuscript Files with Decisions	Number (Percentage) of Acceptances
Canada	22.33 (4.3%)	0.50 (1.1%)
Mexico, Central, and South America	0.00 (0.0%)	0.00 (0.0%)
Europe	49.25 (9.6%)	2.83 (6.0%)
Middle East	4.42 (1.0%)	0.00 (0.0%)
Africa	0.00 (0.0%)	0.00 (0.0%)
Australia and New Zealand	23.25 (4.5%)	1.33 (2.8%)
Asia	62.67 (12.2%)	5.42 (11.5%)
Total Non-U.S.	161.92 (31.8%)	10.08 (21.5%)

Panel A reports author employer affiliations for all articles published in *The Accounting Review* in Volume 87 (calendar 2012) as well as the four-year cumulative totals for Volumes 84–87 (calendar years 2009–2012), excluding invited commentaries. For articles written by k coauthors, the table allocates $1/k$ of an article to each coauthor's affiliation. For authors indicating two affiliations, that author's $1/k$ share is allocated equally to both affiliations.

Panel B reports author Ph.D.-granting affiliations for the same periods and is constructed in the same manner as in Panel A. An asterisk in Panel B signifies a university that Williams and Rodgers (1995) and Williams et al. (2006) identify as one of 15 "elite" schools.

Panel C reports data for international affiliations, allocating $1/k$ of each manuscript to each of k coauthors. Submissions data for the journal year ending May 31, 2012 reflect the 512 unique files for which decisions were reached in fiscal 2012 based on original submissions or resubmissions in fiscal 2012. Manuscripts for which two or more decisions were reached within the year due to invitations to revise and resubmit are counted only once. We do not include the 190 files for which decisions were made in fiscal 2012 based on submissions or resubmissions prior to fiscal 2012. Acceptance data reflect all acceptances and conditional acceptances reached in the journal's fiscal year 2012, including some acceptances that are scheduled for publication in early 2013, but excluding acceptances based on submissions or resubmissions received prior to June 1, 2011. Hence, the 47 acceptances in this panel do not equal the total acceptances in Panels A and B of Table 4, because Panels A and B only reflect actual articles published in the respective calendar years, excluding forthcoming articles accepted for future publication. The 47 acceptances will also differ from the 75 acceptances for 2012 in Panel A of Table 2 because the 75 acceptances include manuscripts from the 190 files for which decisions were made in fiscal 2012 based on submissions or resubmissions prior to 2012.

Panel D shows the geographical breakdown for the fiscal 2012 non-U.S. submissions and acceptances tallied in Panel C.

invited discussions from research forums, two in 2009 and one in 2012. I also note here that the *TAR* conflict-of-interest policy precludes a *TAR* co-editor or senior editor from serving as editor on a colleague's submission.[4]

The 278 articles included in the four-year cumulative column of Table 4, Panel A reflect 187 different affiliations, with the cumulative article count by affiliation ranging from a high of 11.5 articles to a low of 1/6 of an article (a three-coauthored article with two affiliations for one of the coauthors). In terms of relative concentration/diversity of employment, the 187 different affiliations over four years suggest a limited concentration in *TAR* over this period. The ten highest-ranked affiliations tallied in the Table 4, Panel A cumulative column account for 24.0 percent of the 278 total articles, which is slightly less than the 25.3 percent top-ten statistic similarly computed for *The Accounting Review* by Swanson et al. (2007, 1262) in their analysis of concentration in articles published across four accounting journals and across ten nonaccounting business journals from 1990–2002. Swanson et al. (2007, Table 2) report that *TAR* and *Contemporary Accounting Research* are less concentrated than the other two accounting journals they consider, and that *TAR* is seventh in concentration when compared against ten prominent nonaccounting business journals. Overall, these results are consistent with only a moderate level of affiliation concentration in *TAR* in recent years.

Ph.D. Affiliations

Table 4, Panel B tallies the doctoral alma maters of *TAR* authors published in calendar year 2012 and cumulatively from 2009–2012, attributing $1/k$ of each article to each of its k coauthors, and excluding invited AAA Presidential Scholar Lectures and three invited forum discussions. Panel B lists programs in descending order of the cumulative four-year frequency of doctoral affiliations by school. Without endorsing the notion or measurement of "elite" doctoral programs (Williams and Rodgers 1995; Williams et al. 2006), the data in Panel B of Table 4 indicate that these Ph.D.-granting institutions, identified by an asterisk (*), comprise 42 percent of the *TAR* articles published from 2009 to 2012, which is less than the 62 percent figure reported by Williams et al. (2006, 792) for 2005. The 42 percent is also lower than the 52 percent and 47 percent figures that Williams et al. (2006) apparently characterize as being relatively more reasonable "elite" concentrations from the late 1970s and early 1980s. The general conclusion that I take away from Table 4, Panels A and B, is that *TAR* has been relatively diverse in terms of author affiliation and author Ph.D.-granting institution over the years 2009–2012.

International Affiliations

Table 4, Panel C reports data on international affiliations. The trend from fiscal 2011 to fiscal 2012 is toward increasing percentages of submissions and acceptances from outside the U.S. For fiscal 2012, authors employed outside the U.S. account for about 31.8 percent of *TAR*'s submissions versus about 30 percent for fiscal 2011, and for 21.5 percent of acceptances versus about 20 percent for fiscal 2011. One interpretation of the fact that non-U.S. submissions exceed non-U.S. acceptances is that *TAR* responds less favorably to submissions from non-U.S. authors. However, my personal belief is that it more likely reflects the existence of correlated underlying characteristics that are associated with perceptions of manuscript quality.

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[4] In the same spirit, a manuscript submitted by a *TAR* senior editor should be handled either by one of the other current editors or by another qualified individual such as a previous editor. A manuscript that I coauthored was originally submitted to *TAR* shortly before the May 2011 transition. Steve Kachelmeier agreed to serve as editor with all files and communication routed through Steve rather than through the *TAR* office so that the identity of reviewers, etc., remained blind.

Table 4, Panel D drills deeper by documenting the fiscal 2012 non-U.S. submissions and acceptances by major geographical region. Of the submissions, Canada (4.3 percent of total fiscal 2012 unique submissions), Europe (9.6 percent), and Asia (12.2 percent) constitute the most frequent contributors. Among the acceptances, every non-U.S. region experiences an acceptance percentage somewhat lower than its corresponding submission percentage. While one might ideally like to see the same general equivalence in our distributions of international submissions and acceptances that we experience for different research topics and methods, I nevertheless find it noteworthy—from the "glass half full" perspective—that one in five successful *TAR* authors resides outside the U.S. and that this percentage is increasing.

Table 5: Processing Time

Table 5 reports processing time data for the 722 editorial decisions reached from June 1, 2011 through May 31, 2012, defining "processing time" as the number of days from the submission to the date when Stacy Hoffman sends the decision letter. Our mean and median processing times for 2012 are 96 days and 94 days, up slightly from 90 and 93 days in Steve and Mary's final year. Stacy and I believe that these measures will improve in our second year. In particular, we will focus on reducing the upper tail of 17.1 percent of all manuscripts that require beyond 120 days to process. I expect our greater familiarity with the AllenTrack system to enable us to do so.

IV. NOTES OF THANKS AND RECOGNITION

Now for some thanks and much deserved recognition! Stacy Hoffman, *TAR* Editorial Assistant, has clearly earned the first place in line. The only way an operation like this can succeed (and I will presume we qualify) is with someone who has her or his finger and mind around every single detail at every single minute, and who knows how each and every detail relates to one another. I surely cannot do this, but Stacy can, thank God! When we first posted the announcement for the position on the University human resources system, I almost immediately received a message from Stacy on my faculty email account saying essentially, "Look no further; you have found the person for the job!" When I met Stacy and checked her background, I could see that she was right. Besides being "incredibly fast and almost infallible" as noted earlier, Stacy is always here, always has a smile on her face, interacts very positively with everyone from disgruntled authors and confused reviewers to lost students who wander into our office and the often-bewildered senior editor. Stacy works late and comes in weekends to try to keep up with the flow of manuscripts and, of course, without compensation. Finally, she has extraordinarily good judgment in resolving myriad issues, allowing me to focus on just a few. She is indeed a godsend!

Second, I thank Katz doctoral students Michele Frank and Eric Chan for their excellent performance as *TAR* research assistants. They perform the critical function of identifying potential reviewers with accuracy and thoughtfulness, whether the pile of manuscripts requiring reviewers is small or large. They have also pitched in cheerfully on important tasks such as assembling data for this report. Their dependability, good spirit, and precision are much appreciated. I have every confidence that they both will be highly successful accounting faculty members in a few years.

Third, I enthusiastically thank the 14 outstanding scholars who have joined me in assuming editorial responsibilities for *The Accounting Review* over this first year of our three-year term—Mike Ettredge, Dave Guenther, Leslie Hodder, Amy Hutton, Bin Ke, Lisa Koonce, Charles Lee, Ken Merchant, Greg Miller, Don Moser, Mort Pincus, Vern Richardson, Phil Stocken, and Beverly Walther. Most importantly, they have been a reliable source of advice and counsel as we have evolved policies for handling various issues. In addition, I have leaned on each of them to make a high volume of very important decisions in a timely, consistent, conscientious, and most important, thoughtful and insightful manner. I have also leaned on various editors to handle extra

TABLE 5

Processing Time from Date of Submission to Date of Sending the Decision Journal Year Ending May 31, 2012

Processing Time	Number of Manuscripts	Percentage	Cumulative Number of Manuscripts	Cumulative Percentage
≤ 15 days	6	0.8%	6	0.8%
16–30 days	16	2.2%	22	3.0%
31–45 days	9	1.2%	31	4.3%
46–60 days	42	5.8%	73	10.1%
61–75 days	100	13.8%	173	23.9%
76–90 days	147	20.3%	320	44.3%
91–105 days	160	22.1%	480	66.4%
106–120 days	118	16.3%	598	82.7%
121–135 days	63	8.7%	661	91.4%
136–155 days	29	4.0%	690	95.4%
156–170 days	13	1.8%	703	97.2%
171–219 days	19	2.6%	722	99.9%
Total manuscripts	722			

Mean processing time: 96 days
Median processing time: 94 days

manuscripts, manuscripts that do not quite fit their backgrounds, manuscripts that require very quick processing, cases of appeals of decisions, etc. In particular, with his office just down the hall, I frequently drop in for a "few minutes" of Don Moser's time, and come away with a clearer appreciation of the issues, even if that takes closer to an hour. Invariably, the co-editor I have asked for special assistance has agreed and has handled even the most difficult cases in a very professional manner. My personal thanks to each of you!

Fourth, perhaps the most fundamental ingredient of a top-quality, peer-reviewed journal process is informed, thoughtful, and objective reviews. To all Editorial Board members, and particularly the 51 members who have completed four to seven reviews for *TAR* this year in addition to the many review requests they must be receiving from other journals, I cannot thank you enough. In addition, we obviously could not operate without the additional 498 *ad hoc* reviewers (named in Appendix A) who completed one or more reviews for *TAR* during the fiscal year ending May 31, 2012. There is no way that *The Accounting Review* could maintain a two-reviewer system without the willing assistance of *ad hoc* reviewers and, to quote Steve, they are "*TAR*'s unsung heroes." Thank you.

Fifth (and he deserves higher billing), I cannot thank Steve Kachelmeier enough for the encouragement and tips that he has provided. Even though Steve is just a youngster, he has played a role for Stacy and me that my "Uncle Cy" played as I grew up in our house with six kids. Uncle Cy was my dad's law partner, a brilliant Harvard Law graduate who always had his "ducks in a row," setting high standards for the rest of us. Whenever one of the kids in our house was trying to get by with a project that was not quite right, the call would go out, "Don't let Uncle Cy see that!" Thanks a million, Uncle Cy! In addition, I thank each of Steve's 13 co-editors who have so graciously agreed when I have requested that they continue as editor on files with split decisions for which they handled the first round. Thanks from the authors and from me.

Sixth, I thank the American Accounting Association, particularly the many outstanding professionals on the AAA staff in Sarasota, as well as the generous academic volunteers who serve on the AAA Publications Committee and Board of Directors. The danger of naming names is that one invariably leaves someone out who should be thanked, but with no slight intended to those not named, the AAA staff professionals Stacy and I have dealt with most often include AAA Publications Coordinator Lisa Habblitz, Publications Assistant Lindsay Peters, and Web/Database Projects Director Kathy Casper. AAA Executive Director Tracey Sutherland has been wonderfully supportive on a variety of critical issues this year with her voice of reason, experience, and good judgment being invaluable. Likewise, I have come to appreciate the calm and thoughtful perspective that AAA Publications Committee Chair Christine Botosan has supplied to keep some difficult issues on track.

Seventh, I thank our contacts at Allen Press, especially Elizabeth Garrett and Kristi Bailey in tech support and Managing Editor Jean Baldwin, for their patience and their prompt and helpful answers to our many questions.

Eighth, I thank the University of Pittsburgh and the Katz Graduate School of Business, Dean John Delaney, and Director of Administration Ron Magnuson for their support of *TAR*, providing Stacy and me with the necessary physical and technology support we need. Thanks also to the generosity of my accounting colleagues at Katz for allowing me to have half-time services from Michele and Eric. Likewise, I thank the many current and former Katz faculty and doctoral colleagues who have generously given their time as reviewers.

Ninth, I thank my accounting academic colleagues at universities around the world for this wonderful opportunity and responsibility of serving as *TAR* Senior Editor. While certainly not all of our decisions have been met with enthusiastic agreement, I believe that, in the great majority of cases, authors have respected the decisions and acknowledged the care and effort that has gone into them. I expect that all editors appreciate the occasional note from an author, whether following an acceptance or a rejection, acknowledging the constructive and objective inputs from reviewers and editors, and this first year has been no exception—thanks!

Last and certainly not least, I thank my family for their support. My dear wife, Karen, has covered for me on many fronts for 42 years now, understanding when I routinely disappear to my third-floor study with a pile of manuscripts. More importantly, Karen and our two daughters, Julie and Kate, organize the logistics that allow me the luxury of doing this job while still staying in close touch with our six grandchildren in Pittsburgh. This includes allowing me to share a bowl of cheerios and blueberries with a hungry two-year-old, to pick up the kindergarten and first-grade brothers at school, to share a sidehill swimming pool with excellent two-year-old and seven-year-old sisters and to ride the Pittsburgh bike trails with a nine-year-old philosopher. How lucky I am!

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APPENDIX A

TAR Ad Hoc Reviewers (n = 498)
June 1, 2011–May 31, 2012

Jeffery Abarbanell	The University of North Carolina at Chapel Hill
Lawrence Abbott	University of Wisconsin–Milwaukee
Margaret Abernethy	The University of Melbourne
David Aboody	University of California, Los Angeles
Anup Agrawal	The University of Alabama
Jennifer Altamuro	The Ohio State University
Eli Amir	London Business School
Rowland Atiase	The University of Texas at Austin
Frances Ayres	The University of Oklahoma
Brad Badertscher	University of Notre Dame
Mark Bagnoli	Purdue University
Bok Baik	Seoul National University
Sudhakar Balachandran	Columbia University
Steven Balsam	Temple University
E. Michael Bamber	The University of Georgia
Ran Barniv	Kent State University
Mary Barth	Stanford University
Abhijit Barua	Florida International University
Sudipta Basu	Temple University
George Batta	Claremont McKenna College
Mark Beasley	North Carolina State University
Messod Daniel Beneish	Indiana University–Bloomington
Daniel Bens	The University of Arizona
Jeremy Bertomeu	Baruch College–CUNY
Anne Beyer	Stanford University
Sreedhar Bharath	Arizona State University
Sudip Bhattacharjee	Virginia Polytechnic Institute and State University
Nilabhra Bhattacharya	Southern Methodist University
Sanjeev Bhojraj	Cornell University
Mary Brooke Billings	New York University
Ervin Black	Brigham Young University
Elizabeth Blankespoor	University of Michigan–Ann Arbor
Jasmijn Bol	University of Illinois at Urbana–Champaign
Sarah Bonner	University of Southern California
Christine Botosan	The University of Utah
Jan Bouwens	Tilburg University
Robert Bowen	University of Washington–Seattle
Joseph Brazel	North Carolina State University
François Brochet	Harvard University
Jennifer Brown	Arizona State University–Tempe
David Burgstahler	University of Washington
Jeffrey Burks	University of Notre Dame
Robert Bushman	The University of North Carolina at Chapel Hill
Alexander Butler	Rice University
Donal Byard	Baruch College–CUNY
Brian Cadman	The University of Utah
Andrew Call	The University of Georgia
Dennis Campbell	Harvard University

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APPENDIX A (continued)

Vedran Capkun	HEC Paris
Elizabeth Carson	The University of New South Wales
Judson Caskey	University of California, Los Angeles
Marcus Caylor	Kennesaw State University
Sandra Chamberlain	The University of British Columbia
K. Hung Chan	Lingnan University
Hsihui Chang	Drexel University
Xin (Simba) Chang	Nanyang Technological University
Chih-Ying Chen	Singapore Management University
Kevin Chen	The Hong Kong University of Science and Technology
Peter Chen	The Hong Kong University of Science and Technology
Qi Chen	Duke University
Xia Chen	University of Wisconsin–Madison
Zhihong Chen	City University of Hong Kong
C. S. Agnes Cheng	The Hong Kong Polytechnic University
Mei Cheng	The University of Arizona–Tucson
Shijun Cheng	University of Maryland–College Park
Chen Lung Chin	National Chengchi University
Jong-Hag Choi	Seoul National University
Jongwoon (Willie) Choi	University of Pittsburgh
Peter Ove Christensen	University of Aarhus
Ted Christensen	Brigham Young University
Elizabeth Chuk	University of Southern California
Bryan Church	Georgia Institute of Technology
Peter Clarkson	The University of Queensland
Shana Clor-Proell	Texas Christian University
C. Bryan Cloyd	Virginia Polytechnic Institute and State University
Jeffrey Cohen	Boston College
Lauren Cohen	Harvard University
Daniel Collins	The University of Iowa
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EDITORIAL POLICY AND STYLE INFORMATION

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According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature. Topical areas of interest to the journal include accounting information systems, auditing and assurance services, financial accounting, management accounting, taxation, and all other areas of accounting, broadly defined. The journal is also open to all rigorous research methods.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

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Format

1. All manuscripts should be formatted in 12-point font on 8 1/2 × 11″ paper and should be double-spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins should be at least one inch from top, bottom, and sides.
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5. A cover page should show the title of the paper, all authors' names, titles and affiliations, email addresses, and any acknowledgments.
6. The American Accounting Association encourages use of gender-neutral language in its publications.
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8. Headings should be arranged so that major headings are centered, bold, and capitalized. Second-level headings should be flush left, bold, and both uppercase and lowercase. Third-level headings should be flush left, bold, italic, and both uppercase and lowercase. Fourth-level headings should be paragraph indent, bold, and lowercase. Headings and subheadings should not be numbered. For example:

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Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text; in tables and figures, the symbol % is used.

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An Abstract of about 100 words (150 maximum) should be presented on a separate page immediately preceeding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. The Keywords statement should appear immediately below the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

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The author should note the following general requirements:

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Sample entries are as follows:

Baiman, S., and M. Rajan. 2002a. The role of information and opportunism in the choice of buyer-supplier relationships. *Journal of Accounting Research* 40 (2): 247–278.

Baiman, S., and M. Rajan. 2002b. Incentive issues in inter-firm relationships. *Accounting, Organizations and Society* 27 (3): 213–238.

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NOVEMBER 2012 PLACEMENT ADS

The deadline for free position ads to be included in this section of *The Accounting Review* is two months prior to the desired publication in the January, March, May, July, September, or November issues. Position ads, which are free with the purchase of a job posting in the AAA Career Center, should provide all relevant information about the available positions and must include contact information or application instructions for interested candidates. For more information on how to purchase a job posting in the Career Center, or for up-to-date, detailed information about the placement listings in this issue, please go to the AAA website at http://aaahq.org and click on "Career Center," or call our office at 941-921-7747.

THE COLLEGE AT BROCKPORT, SUNY seeks applicants for a position to teach graduate and undergraduate accounting courses beginning Fall 2013. Teach other business courses as needed consistent with academic/professional experience and the emerging needs of the department's programs. Continuing contributions in the area of scholarship, consistent with standards of performance required for continuing appointment (tenure), AACSB, and graduate faculty standards. Academic and career advisement of students (after first semester). Participation in program/curriculum development. Service activities consistent with the needs of the department, college, and profession (after first year). Continuous professional development. Interaction with stakeholders as needed. Integration of contemporary computer applications into course content. Salary: Competitive Ph.D. or D.B.A. in Accounting from an AACSB-accredited institution. ABD candidates must earn their doctoral degree within six months of hire. Scholarly potential in accounting demonstrated by publications or high-quality work in progress and research plans. Ability to teach in a multi-cultural environment. Strong work ethic and commitment to continuous improvement. Preferred Qualifications: CPA, CFE, CMA, or other professional accounting certification. Demonstrable potential for effectively teaching graduate and undergraduate-level accounting courses. Interest in teaching in the area of forensic accounting. Relevant work experience in accounting. Degree in hand by August 2013. Contact Dr. Donald Kent at: dkent@brockport.edu. Open until filled. Apply at: https://www.brockportrecruit.org/applicants/jsp/shared/frameset/Frameset.jsp?time=1347049560923

MIDDLE TENNESSEE STATE UNIVERSITY, Department of Accounting, located in Murfreesboro, TN, invites applications for two tenure-track faculty positions: (1) tax, rank open (position #110020); and (2) cost/managerial (interest in governmental/nonprofit accounting a plus, other areas considered), Assistant/Associate rank (position #116320). Positions begin August 2013. Earned doctorate in accounting expected (ABD considered if close to completion). Applicants for tax position with J.D. degree from accredited law school and a master's degree in tax will be considered. Department offers a B.B.A. in accounting, a Master of Accountancy, and supports the M.B.A. accounting concentration. Department earned AACSB accounting accreditation in 2004, reaffirmed in 2012. Direct questions to: Dr. G. Robert Smith, Jr. at: smitty.smith@mtsu.edu. Reviews of applications begin September 2, 2012 and continue until filled. To apply go to: https://mtsujobs.mtsu.edu. EO/AA Employer. Women and minorities are encouraged to apply.

UNIVERSITY OF OREGON UNIVERSITY, Lundquist College of Business seeks to fill one anticipated tenure-track position for an Assistant or Associate Professor in Accounting effective September 2013. Duties: publishing in top academic journals, teaching undergraduate and graduate accounting courses, and supervising Ph.D. students. Requires the ability to work effectively with students, faculty, and staff from diverse backgrounds. Requirements: Asst. Prof. level: Completed or nearly completed Ph.D. by Summer 2013 plus demonstrated potential for teaching excellence and ability to publish economics-based research in high-quality academic journals. Senior Asst./Assoc. Prof. level: Ph.D. degree, demonstrated superior teaching competence, and record of scholarly accomplishment that includes publication in high-quality academic journals in accounting or related fields. Salary competitive. For full description go to: http://hr.uoregon.edu/jobs/. Application review begins October 1, 2012; continues until position filled. Application: Send letter of interest, *curriculum vitae*, copies of research papers, evidence of teaching performance, and three letters of reference to: ActgOpenSearch@lcbmail.uoregon.edu. The University of Oregon is an AA/EO/ADA institution committed to cultural diversity.

THE UNIVERSITY OF NOTRE DAME, Department of Accountancy invites applications for position openings at all levels: Assistant, Associate, or Full Professor. All research areas will be considered. The appointment is expected to begin July 1, 2013. Applicants for Assistant Professor positions should expect to complete doctoral degree requirements before the start of the contract period. All candidates should have a strong commitment to scholarly research and excellence in teaching. Salary is very competitive, and research support is excellent. Please submit *curriculum vita*, three letters of recommendation, teaching evaluations, and an example of scholarly work electronically to: acctdept@nd.edu. Also, address inquiries to: acctdept@nd.edu. The deadline for applications is December 7, 2012. The University of Notre Dame is an Affirmative Action Employer with a strong commitment to fostering a culturally diverse atmosphere for faculty, staff, and students. Women, minorities, and those attracted to a university with a Catholic identity are especially encouraged to apply. For more information about the University of Notre Dame, the Mendoza College of Business, and the Department of Accountancy please visit the department's website at: http://business.nd.edu/accountancy/.

UNIVERSITY OF CINCINNATI invites applicants for the position of Department Head of Accounting. The Head's primary responsibilities include leadership of faculty, administration of undergraduate and graduate academic programs, and coordination of business community and alumni relations. As the second largest of ten majors within the Lindner College of Business, accounting plays a *vital* role in the college and has seen significant increases in enrollment in recent years. The University of Cincinnati campus was recently named one of the ten most beautiful in the world by *Forbes Magazine*. Located in the urban center, uptown section of Cincinnati, UC's Accounting Department was ranked in the top ten in the nation in 2009 by *BusinessWeek*. UC is a Research I institution. Applicants should have tenure, or a tenurable record, commensurate with a Research I institution. Previous administrative experience, and/or experience as a Department Head is valuable, but not required. Contact Dr. Jeri Ricketts at: jeri.ricketts@uc.edu. Apply at: http://www.jobsatuc.com/applicants/Central?quickFind=81619. More information regarding UC and the accounting program can be accessed via the UC website at: http://www.uc.edu.

SALISBURY UNIVERSITY, Franklin P. Perdue School of Business is accepting applications for the position of Assistant Professor of Accounting. This position is based in Salisbury Maryland. Primary Job Duties: Teaching at the undergraduate and possibly the graduate level. Minimum Qualifications: Ph.D. degree or ABD in Accounting from an AACSB-accredited institution. If ABD, doctoral degree must be completed before date of hire. Please see full position and required documents at: http://www.salisbury.edu/HR/jobs/. Applications submitted by September 9, 2012 will receive first consideration. The position will remain open until filled. Salary is competitive and commensurate with qualifications and experience. Appointment will be contingent upon verification of eligibility to work in the U.S. and is expected to begin August 2013. SU is an EO Employer, strongly committed to recruiting and retaining a diverse faculty, staff, and student body.

BENTLEY UNIVERSITY, Department of Accountancy at Bentley University invites applications to fill a tenure-track position in the Fall 2013. We are seeking an individual in the financial reporting area, but will consider all qualified applicants regardless of area of specialty. All ranks will be considered. A qualified candidate must have a doctoral degree in accounting (or expect completion of that degree by September 1, 2013), and a strong commitment to excellence in scholarly research and teaching. Interested candidates should submit to the chair of the search committee a current *vita* with a letter expressing interest in a faculty position, evidence of excellence in scholarship and teaching, and the names and phone numbers of three professionals with whom the candidate has worked closely. Recruiting will continue through the 2012–2013 academic year until the position is filled. Please direct inquiries and applications to the Chair of the Faculty Search Committee: Mahendra Gujarathi, Department of Accountancy, Bentley University, 175 Forest Street, Waltham, MA 02452-4705; Phone: (781) 891-2410; Fax: (781) 891-2896; Email: acrecruiting@bentley.edu.

CALIFORNIA STATE UNIVERSITY, EAST BAY invites applications for Assistant or Associate Professor position in accounting to begin Fall 2013. The primary responsibilities are to teach undergraduate and/or graduate accounting or auditing courses, conduct academic research in accounting, and provide service to the University. A candidate should have an earned Ph.D. in Accounting from an AACSB-accredited institution no later than the effective date of appointment, is academically qualified (AQ) in accordance with the CSU East Bay, College of Business and Economics AQ standards; have a demonstrated record of teaching excellence and peer-reviewed scholarship; and have strong communication skills. Professional certifications (CPA, CMA, or CIA) and extensive work in the accounting profession are desirable. Review of applications will begin September 15, 2012 and will continue until the position is filled. Please send a letter of application; a complete and current *vita*; graduate transcripts; copies of major publications, and three letters of recommendation to: Dr. Micah Frankel, Chair, Department of Accounting and Finance, California State University, East Bay, 25800 Carlos Bee Boulevard, Hayward, CA 94542; Phone: (510) 885-3397; Email: accounting.recruitment@csueastbay.edu. Visit the website: http://www.csueastbay.edu.

CALIFORNIA STATE UNIVERSITY, EAST BAY invites applications for Assistant Professor position in taxation to begin Fall 2013. The primary teaching responsibilities are for the graduate taxation courses in an AACSB-accredited online M.S. Tax program, conduct academic research in tax, and provide service to the University. As needed, the individual will also teach undergraduate tax and nontax accounting or business law courses. A candidate should have an earned Ph.D. in Accounting from an AACSB-accredited institution with specialized course work or academic preparation in the area of taxation, demonstrated record of teaching excellence and peer-reviewed scholarship; or M.S. Tax or L.L.M. (Taxation) and a CPA with high-level professional tax experience, demonstrated ability to publish and the requirement to publish in quality accounting or tax peer-reviewed journals within three years of hire; and have strong communication skills. Professional certifications and extensive work in the tax profession are desirable. Review of applications will begin September 15, 2012 and will continue until the position is filled. Please send a letter of application; a complete and current *vita*; graduate transcripts; copies of major publications, and three letters of recommendation to: Dr. Micah Frankel, Chair, Department of Accounting and Finance, California State University, East Bay, 25800 Carlos Bee Boulevard, Hayward, CA 94542; Phone (510) 885-3397; Email: accounting.recruitment@csueastbay.edu. Visit the website: http://www.csueastbay.edu.

WESTERN KENTUCKY UNIVERSITY invites applications for a tenure-track position in all ranks, with specialization in financial accounting and/or auditing, in the Department of Accounting. Candidates with appropriate credentials may be considered for the J. C. Holland Professorship in Accounting. Appointment begins August 2013. Tenure-Track Position: Earned doctorate with an emphasis in Accounting (ABDs near completion and graduates from a Post-Doctoral Bridge to Business Program accounting track may also be considered). Professional certification is desirable. J. C. Holland Professorship: Earned doctorate with an emphasis in Accounting, professional certification highly desirable. Hold a documented record of effective teaching and a willingness to teach at all levels. Expected to have a distinguished and ongoing record of research, culminating in publications in refereed journals. Please submit an application letter, *vita*, transcripts, student evaluations, and three letters of reference to: Accounting Search Committee, Western Kentucky University, 1906 College Heights Boulevard #11061, Bowling Green, KY 42101. Position will remain open until filled. Information about this position can be obtained by contacting (270) 745-3895, or http://www.wku.edu/employment/faculty.php. Western Kentucky University is an Affirmative Action/Equal Employment Opportunity employer.

THE UNIVERSITY OF IOWA, Department of Accounting, Tippie College of Business invites applicants for a tenure-track appointment(s) beginning Fall 2013. We will consider candidates at all ranks. Required qualifications include a demonstrated interest in and capacity to do research publishable in top-tier accounting journals, a high level of teaching competence, and an earned doctorate (entry-level candidates can be near completion). We offer a competitive compensation package. We will screen applicants at all ranks on an ongoing basis. To apply, please log on to our website at: http://jobs.uiowa.edu/jobSearch/faculty/ (reference Requisition # 61273). You will be asked to upload a résumé, representative research output, evidence of teaching capabilities (if available), and to provide the names for at least three letters of recommendation. The University of Iowa is an Affirmative Action/Equal Opportunity Employer. Women, minorities and individuals with disabilities are encouraged to apply.

XIAMEN UNIVERSITY seeks candidates for an Assistant, Associate, or Full Professor whose primary duties include teaching two or three graduate courses per year and writing research papers publishable in reputable international academic journals. All accounting and finance-related disciplines will be considered. Candidates for the Assistant Professor rank should possess a Ph.D. in accounting, finance, or economics, or be very close to completion. Selection will be based on potential for publications in reputable accounting and finance journals. For the Associate and Full Professor ranks, a proven record of publications in reputable accounting and finance journals is further required. Contact Cindy Xie at: ifas@xmu.edu.cn.

BEIJING NORMAL UNIVERSITY–HONG KONG BAPTIST UNIVERSITY, United International College (UIC), Division of Business and Management invites applications for Professor/Associate Prof. / Assistant Prof. in Accounting or in Finance and Financial Services. Job Character: Full-Time. Applicants should hold a Ph.D. degree or a Master's degree with extensive working experience in a related discipline and should be committed to excellence in teaching and undergraduate education. Research interests and experience are desirable. Appointment Terms: Appointment to these positions will initially be made on a fixed-term contract of two years. Commencing salaries will be commensurate with qualifications and experience. Benefits include medical insurance and housing allowance. Re-appointment after the first contract is subject to mutual agreement. To submit applications, please send your application via email: to recruit@uic.edu.hk. Applications should include a *curriculum vitae* and completed Application Form for Teaching Positions at UIC, which can be obtained at: http://uic.edu.hk/jobs.

WASHINGTON STATE UNIVERSITY, Department of Accounting, College of Business invites applications for an Assistant/Associate Professor of Accounting. The position begins August 2013 and duties include teaching, research, and service. A doctorate in accounting from an AACSB-accredited school is required, ABD considered if degree completed by date of employment. Preference given to candidates with primary teaching interest in financial accounting and secondary teaching interest in an additional area of accounting. Candidates must demonstrate the ability to publish in top-tier academic accounting journals and a desire to take an active role working with doctoral students. The following documents are required: letter of application, *curriculum vitae*, an example of scholarly work to be presented to faculty and Ph.D. students if selected for an on-campus visit, and three letters of recommendation. Application materials must be posted online to be considered for the position, at: http://www.wsujobs.com. Finalists will be required to submit a summary of their student evaluations. Review of applications will begin September 15th, and will continue until position is filled. Questions should be directed to Associate Professor and Department Chair, Sue Gill, Phone: (509) 335–5633; Email: gills@wsu.edu. WSU IS AN EO/AA/ADA Employer and educator. WSU employs only U.S. citizens and lawfully authorized non-U.S. citizens. All new employees must show employment eligibility verification as required by the U.S. Immigration and Naturalization Services.

DONGBEI UNIVERSITY OF FINANCE AND ECONOMICS (DUFE), School of Accounting invites applications from Ph.D. graduates in Accounting, Auditing, and Corporate Finance for a full-time position. Job duties for the successful applicant will include research, teaching (which is not required if applying for a research position), and service to the School and the University as a full-time staff. Candidates must have a completed Ph.D. degree in accounting or corporate finance from a prestigious overseas university and concentrate on the research of accounting, auditing, or corporate finance. The annual salary will be $50,000 or above plus health insurance, research grant, Ph.D. allowance. Rank, extra awards, and funds will be granted depending on research experience and performance. Additionally private office and necessary research resources are available. Please submit your *cv* and published papers (full-text in PDF, no more than 3 pieces) to: qiyuan@dufe.edu.cn or contact: Qi Yuan at: (86) 411-84713729/84711060.

WAKE FOREST UNIVERSITY, Schools of Business are seeking qualified candidates for Fall 2013 non-tenure-track Professor of Practice faculty appointments in management accounting and tax. These positions are intended to be teaching-intensive. To view a full job description including qualifications and to apply, please visit the Wake Forest University Employment site at: https://wakejobs.silkroad.com/. Review of *vitae* will begin August 1, 2012. Applications will be accepted until the positions are filled.

WAKE FOREST UNIVERSITY, Schools of Business are seeking qualified candidates for a Fall 2013 tenured or tenure-track faculty appointment in management accounting with opportunities to teach in our undergraduate and graduate accounting and other business programs on our main campus in Winston-Salem as well as our Charlotte campus. Professors, Associate professors, and experienced Assistant professors are encouraged to apply. JOB QUALIFICATIONS: Credentials and experience appropriate to academically qualified status (e.g., Ph.D., and established teaching and research record) as well as to rank and tenure status are a requirement. For an expanded position description and online application instructions, please visit the Wake Forest Employment site at https://wakejobs.silkroad.com/.

BRIDGEWATER STATE UNIVERSITY seeks applicants for an Assistant Professor of Accounting position. Job Description: The primary responsibilities are to teach upper- and lower-level courses in Accounting, with the possibility of teaching select Finance courses and graduate courses. The successful candidate will also participate in the continued development of the graduate and undergraduate curriculum. Required Minimum Qualifications: An earned doctorate or matriculation in a regionally accredited doctoral program in Accounting or an earned master's degree in Accounting, Finance, or related field with professional certification as Certified Public Accountant, Certified Management Accountant, Certified Internal Auditor, or Certified Information Systems Auditor. College-level teaching experience. Preferred Qualifications: Industry/government experience in accounting. Evidence of scholarly activity. Applicants should be strongly committed to excellence in teaching and advising, and to working in a multicultural environment that fosters diversity. They should also have an ability to use technology effectively in teaching and learning, the ability to work collaboratively, evidence of scholarly activity, and a commitment to public higher education. Additional Information: Applications including (1) a cover letter, (2) a *curriculum vitae* that includes contact information for three references, (3) summary information on courses taught and student evaluations, and (4) a brief statement that describes research interests and teaching philosophy may be submitted at: http://jobs.bridgew.edu. Will accept applications until job is fulfilled.

YOUNGSTOWN STATE UNIVERSITY, the successful applicant should possess a doctorate from an AACSB-accredited institution; a major in accounting is preferred. Applicants who are in the final stages of the dissertation process will be considered. Other requirements include an enthusiastic commitment to accounting education and teaching excellence; demonstrated ability to do publishable research in relevant peer-reviewed journals; ability to work with the business community to further the goals of the department, college, and university; and evidence of strong communication and interpersonal skills. Professional certification(s) and experience in the accounting field are also preferred. Date Available: August 2013. Other Information Relevant to this Position: The Lariccia School of Accounting and Finance has 400 accounting majors. The Williamson College of Business Administration (WCBA) has 1,700 undergraduate students with 110 M.B.A. students. The WCBA moved into a new $34 million, state-of-the-art facility in August 2010. Closing Date for Applications: Review of applications will begin immediately and will continue until filled. Applicants must submit the following documents, online only: (1) a letter of application, (2) a current *vita* with employment history and dates, (3) transcript(s) documenting academic qualifications for this position, (4) three references (including name, address, phone number, and email address for each), (5) a statement of teaching philosophy and qualifications, and (6) copies of teaching evaluation summaries (if available). Only online applications will be considered. These documents must be submitted at: http://web.ysu.edu/gen/ysu/Employment__Job_Opportunities_m653.html.

WESTERN WASHINGTON UNIVERSITY invites applications for tenure-track Assistant or Associate Professor position in accounting beginning Fall 2013. Systems or auditing specialization preferred but other specialization areas may be considered. The position is subject to funding availability. Doctoral degree in accounting or equivalent by the appointment date, AACSB academic or professional qualification and strong teaching and research record or clearly demonstrated potential are required. Duties include teaching undergraduate and graduate courses, university service, and scholarly publication. WWU is located between Seattle and Vancouver, BC, overlooking Bellingham Bay and the Strait of Georgia. The University has 14,900 students and the college is AACSB-accredited. Applications must include a letter of application (include how qualifications are met), *curriculum vitae*, evidence of teaching effectiveness, transcripts, and names of three references. All application materials must be uploaded through the Electronic Application System for Employment (EASE) at: http://www.wwu.edu/jobs. Application review begins: September 20, 2012. AA/EO.

ST. BONAVENTURE UNIVERSITY, Department of Accounting invites applications for a tenure-track position at the Assistant or Associate Professor level. The successful candidate will teach undergraduate and graduate courses, predominantly in the area of Financial Accounting and/or Audit. Review of applications will begin immediately and continue until the position is filled. The position appointment is expected to begin with the Fall 2013 semester, contingent upon funding. The candidate will also be expected to undertake scholarly research in a collaborative environment that welcomes inter-disciplinary and practice-oriented scholarship, as well as discipline-based and theoretical work. Applicants must meet the following required qualifications: (1) earned doctorate in accounting or related field by the time of appointment; (2) demonstrated excellence in teaching and willingness to work closely with students in curricular and co-curricular activities, including service, professional, and/or research activities; and (3) an established or emerging research program. Accounting certification and prior professional accounting experience are preferred. The School of Business, accredited by the AACSB, will be moving into its new state-of-the-art Swan Business Center home in Fall 2013. Accounting faculty, students, and programs also benefit from the endowed McQuade Center for Accounting Excellence. The Department of Accounting offers two program tracks: a 120-hour B.B.A. in Accounting and a 150-hour B.B.A./M.B.A. program in Accounting registered for CPA Exam qualifications. Our B.B.A./M.B.A. graduates are highly recruited by Big 4 CPA firms and other large employers. Applications may be submitted online at: accountingsearch@sbu.edu. For additional information please contact the department chair: Dr. Susan B. Anders at: sanders@sbu.edu or (716) 375-2063. St. Bonaventure University is the nation's premier Franciscan university. The University, School, and Department have a strong commitment to achieving diversity among faculty and staff, and encourage applications from members of under-represented groups.

UNIVERSITY OF NORTH TEXAS (UNT), Department of Accounting in Denton, Texas (DFW Metroplex) invites applications for a tenure-track faculty position at the Associate/Full Professor rank. The successful candidate will provide leadership for the program in Accounting, including engaging with faculty and mentoring junior faculty and doctoral students. The Department of Accounting at UNT is committed to academic excellence and diversity within their faculty, staff, and student body. The Department of Accounting offers a full range of academic degree programs including Bachelor of Accounting (B.B.A.), professional 150-hour program (B.S./M.S.) in Taxation or Accounting, M.S. in Taxation or Accounting, and an Accounting doctoral program. For information, visit: http://www.cob.unt.edu/acct. Our programs are among the most innovative in the country, our faculty exhibit expertise in a host of accounting disciplines, and the department is separately accredited by AACSB. Review of applicants will begin immediately and continue until the position is closed. Salary is competitive and commensurate with qualifications. Candidate must hold a doctorate in Accounting (or equivalent) with an established high-quality capital markets and/or archival academic publication record and have demonstrated the ability to work with others in developing and conducting high-quality research. CPA, CMA, CIA or equivalent certification is preferred, but not required. The successful candidate will provide leadership for the capital markets/archival research area, including engaging with faculty, mentoring junior faculty. and doctoral students. For additional information, contact: Dr. Govind Iyer, Chair of the Search Committee at: govind.iyer@unt.edu, or Dr. Don W. Finn, Chair of the Department of Accounting at: don.finn@unt.edu. UNT is an AA/ADA/EOE.

INDIANA UNIVERSITY BLOOMINGTON, Accounting Department, Kelley School of Business invites applications for Associate or Full Professor (tenured) faculty positions to begin Fall 2013. Candidates for these positions must possess a doctoral degree and have achieved excellence in scholarly research and teaching. Preference will be given to candidates with teaching and research interests in Financial Accounting. Contact Laureen Maines, Phone: (812) 855-2611; Email: lmaines@indiana.edu.

INDIANA UNIVERSITY BLOOMINGTON, Accounting Department, Kelley School of Business invites applications for Assistant or Associate Professor (untenured) faculty positions to begin Fall 2013. Candidates for these positions must possess a doctoral degree and exhibit a strong commitment to and initial success in excellence in scholarly research and teaching. Preference will be given to candidates with interest in Financial Accounting, Managerial Accounting, and/or Auditing. Contact Laureen Maines, Phone: (812) 855-2611; Email: lmaines@indiana.edu.

INDIANA UNIVERSITY BLOOMINGTON, Accounting Department, Kelley School of Business invites applications for Assistant Professor faculty positions to begin Fall 2013. Candidates for this position must possess a doctoral degree and exhibit a strong commitment to excellence in scholarly research and teaching. Preference will be given to candidates with interest in Financial Accounting, Managerial Accounting, and/or Auditing. Applications must be received by November 30, 2012 to be assured of full consideration; however, applications will be accepted until the position is filled. Contact Laureen Maines, Phone: (812) 855-2611; Email: lmaines@indiana.edu.

CALIFORNIA STATE UNIVERSITY, FRESNO, Sid Craig School of Business, Department of Accountancy, invites applications for a tenure-track position available beginning August 2013. We seek candidates with a doctoral degree in Accountancy (Ph.D. or D.B.A.) from an accredited institution (AACSB or equivalent). ABDs may be considered. Candidates must be committed to excellence in teaching; scholarly and applied research; and university and community service. Salary placement depends upon academic preparation and professional experience. Summer stipends for research are available. Please complete application online at: http://apptrkr.com/264726. For inquiries contact: Dr. Patricia Huff, Search Committee Chair, Department of Accountancy, California State University, Fresno, 5245 North Backer Avenue, M/S PB-7, Fresno, CA 93740-8001; Phone: (559) 278-2021; Fax: (559) 278-8834; Email: patricia@csufresno.edu. California State University, Fresno is an Affirmative Action/Equal Opportunity Employer and has a strong commitment to the principle of diversity. For complete ad and details visit: http://apptrkr.com/264726

THE UNIVERSITY OF TEXAS AT BROWNSVILLE (UTB) seeks a full-time, tenure-track Assistant Professor of Accounting to teach graduate/undergraduate courses, engage in scholarly research/publications and provide professional service. Starting employment date is Fall 2013. Duties: Responsible for teaching undergraduate and graduate accounting courses, advising accounting students, participating in Department, School, and University committee work and participation in University, College, and community activities. Required Qualifications: Ph.D. or D.B.A. in accounting from an AACSB International accredited institution; must be academically qualified under AACSB guidelines; have a record of scholarship or demonstrate a potential for scholarship, be an effective teacher; have effective communication and organizational skills. Preferred Qualifications: Professional Certification (CPA, CMA); ability to maintain collegial, collaborative relationships with the faculty and administrators; and ability to foster quality teaching, research, and service. Salary: Commensurate with experience and qualifications. Deadline: Applications will be reviewed upon receipt and continue until the position is filled. Application Procedure: Please submit a faculty application (download at: www.utb.edu/ba/hr/employment), letter of interest describing how you meet the qualifications for the position, *curriculum vitae*, copies of graduate and undergraduate transcripts, and a list of three references (including address, telephone, and email address).

UNIVERSITY OF NORTHERN IOWA, Department of Accounting invites applications for Assistant, Associate, or Full Professor of Accounting beginning August 2013. All areas of teaching and research may apply, however candidates with an interest in taxation are preferred. Applicants who are seeking a balance of teaching/research responsibilities will find a collegial work environment at UNI. Ph.D./D.B.A. in Accounting from an AACSB-accredited institution is required; ABDs will be considered with evidence of degree completion by August 1, 2013. Other required qualifications include record of excellence in teaching, demonstrated ability to produce publishable research, and evidence of strong communication and interpersonal skills. To apply online, visit: http://jobs.uni.edu. UNI is an Equal Opportunity Employer with a comprehensive plan for Affirmative Action. The Department encourages applications from persons of color, women, persons living with disabilities, and veterans.

CALIFORNIA STATE UNIVERSITY, NORTHRIDGE, Department of Accounting and Information Systems is accepting applications for a tenure-track position as an Assistant Professor beginning Fall 2013. Teach courses in undergraduate and/or graduate programs. Work with students from diverse backgrounds. Engage in an ongoing program of scholarship that demonstrates professional growth. Assume an active role in the planning and governance of the institution through service on Department, College, and campus-wide committees. Doctoral degree from an AACSB-accredited institution in the Accounting discipline or related field. Also, CPA (or CMA), teaching experience, and research in accounting are desirable. Applicants with a J.D. and M.S. in Accountancy (or Taxation and L.L.M. in Taxation) are encouraged to apply. Position will be open until filled. Please submit your current *curriculum vitae*, and three letters of recommendation with complete contact information to: Dr. Paul J. Lazarony at: paul.lazarony@csun.edu. CSUN is an equal Opportunity/Affirmative Action Employer.

CALIFORNIA STATE UNIVERSITY, FULLERTON, Department of Accounting is seeking diverse tenure-track faculty at an open rank position in the areas of Audit, Tax and AIS (Accounting Information Systems). The successful candidates will be innovative teachers who are effective communicators and have strong instructional and computer skills. At the same time, all faculty are expected to maintain a rigorous, ongoing commitment to research that results in publication in high-quality peer-reviewed journals. Student advisement and Department, College, and University service are also considered to be part of the responsibility of tenure-track faculty. The Department offers AACSB separately accredited programs at the undergraduate and graduate levels. Successful candidates must meet AACSB standards for academic qualification and can be classified as participating faculty. Qualifications: Candidates must have an earned Ph.D. in accounting (completed by time teaching starts); preference is given to candidates who hold CPA certification and who have professional experience; evidence of strong instructional and computer skills; an ability to communicate effectively with a wide and culturally diverse range of students is required. Contact Dr. Betty Chavis at: bchavis@fullerton.edu, or (657) 278-2225.

PACIFIC LUTHERAN UNIVERSITY, School of Business invites applications for an Assistant/Associate Professor of Accounting to begin September 1, 2013. Ph.D. in Accounting or closely related field required. ABD will be considered, but Ph.D. must be in hand by September 1, 2013. Review of application materials will begin October 15, 2012, but the position will remain open until filled. Full description, qualifications, and application available at: http://employment.plu.edu. PLU is an AA/EOE.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY, Sloan School of Management's Accounting Group is seeking new faculty at all levels for the academic year beginning July 1, 2013, or as soon as possible thereafter. Applicants will be evaluated on their potential for and/or record of excellence in research and teaching; faculty responsibilities include teaching financial accounting at graduate and undergraduate level. Please include evidence of research such as a publication, working papers, or dissertation proposal with other application materials. The deadline for applications is November 30, 2012. However, applications will be evaluated as soon as they are complete, if submitted at an earlier date. Applications must include a cover letter, *curriculum vitae*, and three letters of recommendation. Applications should be addressed to: Professor Joseph P. Weber, Chair, Accounting Group Search Committee. To submit an application, please visit: http://sloanfacultysearches.mit.edu/accounting. MIT is an Equal Opportunity Employer committed to building a culturally diverse intellectual community and strongly encourages applications from women and minorities.

UNIVERSITY OF MARYLAND UNIVERSITY COLLEGE (UMUC) seeks part-time adjunct faculty to teach accounting primarily online in the graduate and undergraduate schools. Online classes and the requisite training for web-based teaching can be accomplished entirely online. Face-to-face and hybrid teaching opportunities for the Undergraduate School in the Maryland, Virginia, and Washington, DC metro areas. UMUC is the largest public university in the United States. As one of the 11 degree-granting institutions of the University System of Maryland, this global university specializes in high-quality academic programs tailored to working adults. UMUC has earned a worldwide reputation for excellence as a comprehensive virtual university and, through a combination of classroom and distance-learning formats, provides educational opportunities to 90,000 students. The University is proud to offer highly acclaimed faculty and world-class student services to educate students online, throughout Maryland, across the United States, and in 27 countries around the world. Requirements: Ph.D. in Accounting or closely related field. (Master's will be considered if a candidate has a CPA and extensive teaching or professional experience); and teaching as well as professional experience. The scholar-practitioner adjunct faculty who teach in the Accounting programs are at the forefront of their fields and bring practical experience to the classroom. Apply at: http://www.umuc.edu/facultyrecruit. UMUC is an Equal Opportunity Employer.

BOSTON COLLEGE, Carroll School of Management seeks applicants for a full-time, tenure-track position at the Assistant Professor level in the Department of Accounting for Fall 2013. Applicants must demonstrate an ability to do high-quality accounting research. In addition, applicants must demonstrate the potential for excellence in teaching at both the undergraduate and graduate levels. We offer a collegial environment and an energetic faculty with close ties to both Boston and national financial and academic communities. We seek individuals with an openness to diverse intellectual interests and an appreciation for Boston College's Jesuit and liberal arts traditions. Applicants should provide a *curriculum vitae*, examples of written work and three letters of reference to: Billy Soo, Chair, Department of Accounting, Carroll School of Management, Boston College, Chestnut Hill, MA 02467. Electronic applications are preferred and should be sent to: CSOM.accounting@bc.edu. Applications received by December 1, 2012 will receive full consideration. The Carroll School of Management is an Equal Opportunity Employer.

PACIFIC LUTHERAN UNIVERSITY, School of Business invites applications for an Assistant/Associate Professor of Finance to begin September 1, 2013 Ph.D. in Finance or closely related field required. ABD will be considered, but Ph.D. must be in hand by September 1, 2013. Review of application materials will begin October 15, 2012, but the position will remain open until filled. Full description, qualifications, and application available at: http://employment.plu.edu. PLU is an AA/EOE employer.

COLUMBIA UNIVERSITY, Business School seeks applications for faculty positions in Accounting. Rank is open. Appointment at senior rank requires record of outstanding scholarship. Candidates with an interest in Financial Accounting research will be preferred. We are looking for an outstanding person with evidence of a strong commitment to both teaching and research. Candidates should possess a doctoral degree or be close to completion. We offer a highly supportive research environment and easy interaction with faculty in other disciplines. Salary and benefits are competitive. Please send resume, an unpublished research paper, and letters of recommendation to: Professor Stephen Penman, Columbia University Business School, 612 Uris Hall, 3022 Broadway, New York, NY 10027. To apply for this position, go to our on-line application site after November 30: http://academicjobs.columbia.edu/applicants/Central?quickFind=56566. Deadline: January 2, 2013. Columbia Business School is seeking applicants who will promote the diversity mission of the University through their research, teaching, and/or service. Columbia University is an Equal Opportunity/Affirmative Action employer.

DARTMOUTH COLLEGE, Tuck School of Business, graduate school of management anticipates a faculty search in accounting during the 2012–13 academic year. We are open to all levels of appointment—Assistant Professor, Associate Professor, or Full Professor—and we will be recruiting at the new-Ph.D. level. Applicants must have a Ph.D. or be near completion of a doctoral degree in accounting or a related field. We strongly encourage applications to be submitted by January 4, 2013. We seek candidates who will produce research of excellent quality that will have high impact on the field. There must be evidence that the candidate can conduct research and teach at a level of quality consistent with Tuck's high standards. We are interested in candidates who can interact effectively with others and enhance the intellectual environment of the school. The Tuck School of Business, founded in 1900, is consistently ranked among the top ten U.S. business schools. The Accounting group includes Robert Resutek, Leslie Robinson, Richard Sansing, and Phillip Stocken. Faculty applications and all supporting documents must be submitted by email to: Faculty.recruiting@tuck.dartmouth.edu. Specify in the subject line of the email that you are applying for a financial accounting position. Dartmouth College is an Equal Opportunity/Affirmative Action employer. Women and minorities are encouraged to apply.

UNIVERSITY OF ROCHESTER, William E. Simon Graduate School of Business Administration is seeking candidates for tenure-track positions at the Assistant/Associate Professor level, effective July 1, 2013. Applicants must have doctorate degree or ABD in Business Administration or Accounting. We are one of the nation's top business schools, with full-time M.B.A. and Ph.D. programs, a part-time M.B.A. program, and Executive M.B.A. programs in Rochester and Switzerland. Please visit our website: http://www.simon.rochester.edu. Apply online at: https://psportal.its.rochester.edu/applyonline/ job ID #176233. Applicants should electronically submit their *curriculum vitae*, three letters of recommendation, and copies of research papers and publications to: wujo@simon.rochester.edu. Applicants are encouraged to apply by November 30, 2012; applications received by February 1, 2013 will be considered. The University of Rochester has a strong commitment to principles of diversity and, in that spirit, actively encourages applications from groups underrepresented in higher education. The University of Rochester is an Affirmative Action/Equal Opportunity Employer.

NORTHEASTERN UNIVERSITY (Boston, MA) is seeking applications for tenure-track positions effective September 2013 at the Assistant or Associate Professor rank. Candidates must have or expect to have a doctorate in accounting or a closely related business field before the start date and be committed to excellence in research and teaching. The Accounting Group at Northeastern is nationally recognized for its high-quality behavioral accounting research, focused on auditing, governance, tax, managerial ,and ethical judgment and decision making. We provide a supportive environment where collaboration in research and teaching is encouraged. The position offers competitive compensation and teaching responsibilities that enable a significant level of high-impact research. Rank and salary are dependent on qualifications. Summer funding, graduate assistants, and travel support are available. Accounting courses are offered at both the undergraduate and at the graduate levels (M.B.A., M.S./M.B.A., M.S.A., and M.S. Tax). For consideration, submit your cover letter, *cv*, and two samples of scholarly work at: http://www.cba.neu.edu/faculty/, click through to "Faculty Positions" on the right-hand side. Northeastern University is an Affirmative Action/Equal Opportunity/Title IX Employer and welcomes applications from minorities, women, and disabled persons.

THE UNIVERSITY OF TEXAS AT DALLAS (UTD) invites applications for two faculty positions in Accounting, effective September 2013. Appointment at Assistant, Associate, or Full Professor level will be considered. Appointees must have a Ph.D. in accounting or equivalent (or have a ABD status and expect to have the Ph.D. within a year of hire date) and demonstrate the ability to conduct a nationally recognized research program. Review of applications will begin immediately and continue until the position is filled. The Naveen Jindal School of Management at UTD is a research-centered school, ranked 16th (worldwide) in the UTD database that tracks publications in leading business journals (http://som.utdallas.edu/top100Ranking/). Applicants should submit *curriculum vitae* (education, research, teaching, and student evaluations, work experience, and a list of publications and presentations) and at least three letters of reference at: http://go.utdallas.edu/pmn120911. For more information, visit http://som.utdallas.edu/ or contact Gail Thompson at: bgthomp@utdallas.edu. Indication of gender and ethnicity for affirmative action statistical purposes is requested as part of the application. The University of Texas at Dallas is an Equal Opportunity/Affirmative Action Employer. All qualified applicants will receive consideration for employment without regard to race, color, religion, sex, national origin, disability, age, citizenship status, Vietnam era, or special disabled veteran's status, or sexual orientation.

Tulane
University
FREEMAN SCHOOL OF BUSINESS

ACCOUNTING
A. B. FREEMAN SCHOOL OF BUSINESS
TULANE UNIVERSITY

The A. B. Freeman School of Business is seeking applicants for full-time, tenure track faculty positions at all ranks to begin July 1, 2013. Candidates are expected to hold a Ph.D. in accounting or related discipline and candidates for professor should currently hold the rank of full or associate professor. Salary, teaching load and research support are commensurate with other major research institutions.

We will consider candidates with research interests in any accounting area. In terms of teaching, we seek candidates who have strong teaching skills at all student levels, undergraduate and graduate. At the senior level, we seek candidates who have demonstrated excellence in scholarship and program leadership.

The accounting faculty at the Freeman School has expanded recently and is projected to continue to grow. This year we added Jasmijn Bol, Zhenhua Chen, Kris Hoang, and Padmakumar Sivadasan as new ladder track faculty. Additional information about the Freeman School and the faculty in its accounting area may be found at: http://www.freeman.tulane.edu/desktop.php.

Tulane University enrolls approximately 13,000 students and is a privately funded, comprehensive research university located in New Orleans. New Orleans is a vibrant, multi-cultural community celebrated worldwide for its music, art, and food.

Applicants should send a letter, indicating their interest, and an up-to-date curriculum vitae. All materials should be submitted electronically to:

Sharon Moore (businessdean@tulane.edu)
Executive Assistant to the Dean
A. B. Freeman School of Business
Tulane University
7 McAlister Drive
New Orleans, LA 70118
504-865-5407

Applicants are encouraged to submit materials by November 30, 2012. Screening of applications will begin immediately and will continue until the position is filled.

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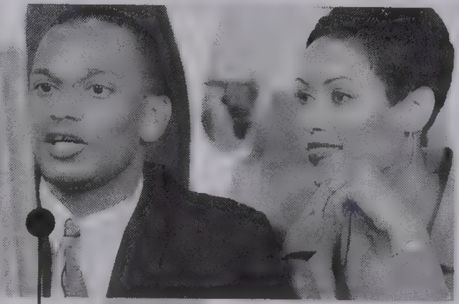

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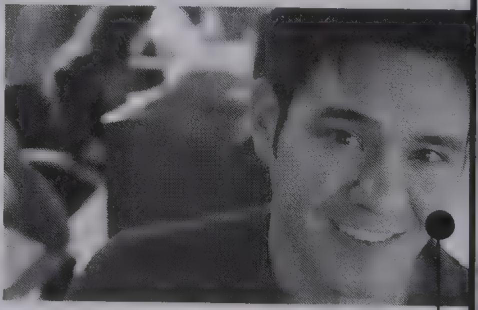

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CHICAGO BOOTH

The University of Chicago Booth School of Business is seeking to appoint outstanding scholars to tenure-track positions in Accounting which would begin in the 2013-14 academic year.

Applications are invited from individuals who have earned a PhD (or equivalent) or expect to receive a doctorate in the near future. Members of our faculty are expected to conduct original research of exceptionally high quality, to teach effectively, and to participate in and contribute to the academic environment. Junior candidates will be judged on potential, and we will rely heavily on the advice of established scholars.

Each candidate should submit a curriculum vitae, a sample of written work, and the names of at least two scholars qualified and willing to evaluate the candidate's ability, training, and potential for research and teaching. Applications will be accepted online at http://facultyapply.chicagobooth.edu. We will start formally reviewing applications on November 30, 2012 and strongly encourage you to complete your application by then. We will continue to accept applications until March 20, 2013.

The University of Chicago is an Affirmative Action/Equal Opportunity Employer.

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